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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE,

THIRTY-FIRST MEETING,

HELD AT

Montreal, Canada,

AUGUST, 1882.

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TREASURER.

WILLIAM LILLY of Mauch Chunk.

MEMBERS OF THE STANDING COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

Past Presidents.—James D. Dana of New Haven; James Hall of Albany; Isaac Lea of Philadelphia; F. A. P. Barnard of New York; J. S. Newberry of New York; B. A. Gould of Boston; T. Sterry Hunt of Montreal; Asa Gray of Cambridge; J. Lawrence Smith of Louisville; Joseph Lovering of Cambridge; John L. LeConte of Philadelphia; J. E. Hilgard of Washington; Simon Newcomb of Washington; O. C. Marsh of New Haven; George F. Barker of Philadelphia; George J. Brush of New Haven; J. W. Dawson of Montreal.

Vice Presidents of the last Meeting.—Wm. Harkness of Washington; T. C. Mendenhall of Columbus; H. C. Bolton of Hartford; W. P. Trowbridge of New Haven; E. T. Cox of San Francisco; W. H. Dall of Washington; A. H. Tuttle of Columbus; Daniel Wilson of Toronto; Alexander Winchell of Ann Arbor; E. B. Elliott of Washington.

Officers of the Meeting.—C. A. Young of Princeton; W. A. Rogers of Cambridge; H. A. Rowland of Baltimore; Edward W. Morley of Cleveland; De Volson Wood of Hoboken; C. H. Hitchcock of Hanover; W. J. Beal of Lansing; J. D. Cox of Cincinnati; O. T. Mason of Washington; F. B. Hough of Lowville; F. W. Putnam of Cambridge; J. R. Eastman of Washington; Alfred Springer of Cincinnati; W. W. Johnson of Annapolis; C. K. Wead of Ann Arbor; J. W. Langley of Ann Arbor; Alexis A. Julien of New York; S. A. Forbes of Normal; Carl Seiler of Philadelphia; G. H. Perkins of Burlington; Joseph Cummings of Evanston; William Lilly of Maunch Chunk.

From the Association at large.—One Fellow to be elected from each Section.

LOCAL COMMITTEE

FOR THE

MINNEAPOLIS MEETING.

CHAIRMAN.

Hon. George A. Pillsbury.

SECRETARY.

Prof. N. H. Winchell.

TREASURER.

Hon. George A. Pillsbury.

SUBCOMMITTEE ON INVITATIONS AND RECEPTION.

Chairman.—President W. W. Folwell.

SUBCOMMITTEE ON FINANCE.

Chairman.—J. C. Seeley, Esq.

SUBCOMMITTEE ON TRANSPORTATION AND EXCURSIONS.

Chairman.—Thomas Lowry, Esq.

SUBCOMMITTEE ON ENTERTAINMENT, HOTELS, LODGINGS AND LUNCHEONS.

Chairman.—Hon. A. C. Rand.

SUBCOMMITTEE ON ROOMS AND PLACES OF MEETING.

Chairman.—Hon. Eugene M. Wilson.

SUBCOMMITTEE ON PRINTING.

Chairman.— David Blakely, Esq.

GENERAL COMMITTEE.

Hon. George A. Brackett; Hon. A. C. Rand; Hon. John De Laittre; Ex-Gov. John S. Pillsbury; W. W. McNair, Esq.; Dr. Wm. W. Folwell; Charles W. Johnson; Gen. A. B. Nettleton; Hon. W. W. Washburn; T. B. Walker; Hon. O. C. Merriman; E. V. White; Hon. H. T. Welles; Col. H. G. Hicks; Thomas Lowry, Esq.; Winthrop Young; Hon. W. S. King; David Blakely; Hon. R. B. Langdon; Hon. D. L. Kiehle; J. C. Seeley, Esq; Anthony Kelly; Dr. A. F. Elliott; Hon. F. W. Brooks; Hon. Eugene M. Wilson.

SPECIAL COMMITTEES.

1. *Permanent Committee on Weights, Measures and Coinage.*

F. A. P. Barnard, of New York,
B. A. Gould, of Boston,
J. E. Hilgard, of Washington,
H. A. Newton, of New Haven,
J. Lawrence Smith, of Louisville,
E. B. Elliott, of Washington,
John Trowbridge, of Cambridge,
A. M. Mayer, of Hoboken.

2. *Committee to Memorialize the Legislature of New York for a New Survey of Niagara Falls.*

G. W. Holley, of Niagara Falls,
J. T. Gardner, of Albany.

3. *Committee on Membership.*

The Members of the Standing Committee.

4. *Committee on the best methods of Science Teaching in the Public Schools.*

E. L. Youmans, of New York,
A. R. Grote, of New York,
J. S. Newberry, of New York,
J. W. Powell, of Washington,
N. S. Shaler, of Cambridge,
N. T. Lupton, of Nashville.

5. *Committee on the Registration of Deaths, Births, and Marriages.*

E. B. Elliott, of Washington,
J. B. Killebrew, of Nashville,
E. T. Cox, of San Francisco,
F. B. Hough, of Lowville,
J. S. Copes, of New Orleans,
John Collett, of Indianapolis.

6. *Committee on Standard Time.*

Ormond Stone, of Univ. of Va.
S. P. Langley, of Allegheny,
E. C. Pickering, of Cambridge,
J. R. Eastman, of Washington,
Lewis Boss, of Albany,
Leonard Waldo, of New Haven,
J. K. Rees, of St. Louis,
G. W. Hough, of Lowville,
H. S. Pritchett, of Glasgow.

7. *Committee on Stellar Magnitudes.*

E. C. Pickering, of Cambridge,
Lewis Boss, of Albany,
S. W. Burnham, of Chicago,
Asaph Hall, of Washington,
William Harkness, of Washington,
E. S. Holden, of Madison,
Simon Newcomb, of Washington,
C. H. F. Peters, of Clinton,
Ormond Stone, of Univ. of Va.
C. A. Young, of Princeton.

8. *Committee on State Geological Surveys.*

G. C. Swallow, of Columbia,
James Hall, of Albany,
John R. Procter, of Frankfort,
N. H. Winchell, of Minneapolis,
W. C. Kerr, of Raleigh,
J. W. Powell, of Washington,
Edward Orton, of Columbus.

9. *Committee to confer with Committees of Foreign Associations for the Advancement of Science with reference to an International Convention of Scientific Associations.*

T. Sterry Hunt, of Montreal,
Alexander Agassiz, of Cambridge.
Simon Newcomb, of Washington.

10. *Committee to confer with a Committee of the British Association for the Advancement of Science on the Annual Records of Science.*

J. W. Dawson, of Montreal,
F. W. Clark, of Washington,
Edward S. Dana, of New Haven,
H. W. Martin, of Baltimore,
C. A. White, of Washington.

11. *Committee to devise a plan for the proper indexing of the Literature of the Chemical Elements.*

H. Carrington Bolton, of Hartford,
Ira Remsen, of Baltimore,
F. W. Clark, of Washington,
A. R. Leeds, of Hoboken,
A. A. Julien, of New York.

12. *Committee on a Primary Meridian and an International Standard of Time.*

F. A. P. Barnard, of New York,
C. A. Young, of Princeton,
T. Sterry Hunt, of Montreal,
J. R. Eastman, of Washington,
Charles Carpmael, of Toronto,
E. B. Elliott, of Washington.

13. *Committee on International Congress of Geologists.*

James Hall, of Albany,
J. W. Dawson, of Montreal,
J. S. Newberry, of New York,
T. Sterry Hunt, of Montreal,
C. H. Hitchcock, of Hanover,
Raphael Pumpelly, of Owego,
J. P. Lesley, of Philadelphia.

14. *Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys.*

James Hall, of Albany,
T. Sterry Hunt, of Montreal,
J. S. Newberry, of New York,
N. H. Winchell, of Minneapolis,
C. H. Hitchcock, of Hanover,
J. P. Lesley, of Philadelphia,
G. H. Cook, of Brunswick,
E. A. Smith, of Tuscaloosa,
Joseph Le Conte, of San Francisco.

This Committee has power to add to its number.

15. *Auditors.*

Henry Wheatland, of Salem,
Thomas Meehan, of Germantown.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Bock,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Bock,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,*	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		J. S. St. John,* 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,	Joseph Lovering,	J. L. LeConte. 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada,	Alexis Caswell,* 9	Alexis Caswell,*	John LeConte,	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†

1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETINGS AND OFFICERS OF THE ASSOCIATION (*Continued*).

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.

PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.
F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.
F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, jr.,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,	F. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	G. J. Engelmann,	———	W. S. Vaux.†
F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†
F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux
F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†

[1] In the absence of E. C. Pickering.

[2] The Subsection united with Sec. B.

[3] In the absence of G. S. Blackie.

[4] In the absence of Ira Remsen.

[5] In the absence of George Little.

[6] In the absence of A. C. Wetherby.

[7] In the absence of A. M. Mayer.

[8] In the absence of George Engelmann.

[9] In the absence of W. R. Nichols.

[10] In the absence of John Trowbridge.

[11] In the absence of S. P. Sharples.

[12] In place of H. W. Wiley, called away.

* Deceased.

† Not present

COMMONWEALTH OF MASSACHUSETTS.

IN THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

TO INCORPORATE THE "AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE."

Be it enacted by the Senate and House of Representatives, in General Court assembled, and by the authority of the same, as follows:

SECTION 1. Joseph Henry of Washington, Benjamin Pierce of Cambridge, James D. Dana of New Haven, James Hall of Albany, Alexis Caswell of Providence, Stephen Alexander of Princeton, Isaac Lea of Philadelphia, F. A. P. Barnard of New York, John S. Newberry of Cleveland, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of Cambridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cambridge and John LeConte of Philadelphia, their associates, the officers and members of the Association, known as the "American Association for the Advancement of Science," and their successors, are hereby made a corporation by the name of the "American Association for the Advancement of Science," for the purpose of receiving, purchasing, holding and conveying real and personal property, which it now is, or hereafter may be possessed of, with all the powers and privileges, and subject to the restrictions, duties and liabilities set forth in the general laws which now or hereafter may be in force and applicable to such corporations.

SECTION 2. Said corporation may have and hold by purchase, grant, gift or otherwise, real estate not exceeding one hundred thousand dollars in value, and personal estate of the value of two hundred and fifty thousand dollars.

SECTION 3. Any two of the corporators above named are hereby authorized to call the first meeting of the said corporation in the month of August next ensuing, by notice thereof "by mail," to each member of the said Association.

SECTION 4. This act shall take effect upon its passage.

HOUSE OF REPRESENTATIVES, March 10, 1874.
Passed to be enacted,
JOHN E. SANFORD, *Speaker*

IN SENATE, March 17, 1874.
Passed to be enacted,
GEO. B. LORING, *President.*

March 19, 1874.
Approved,
W. B. WASHBURN.

SECRETARY'S DEPARTMENT,
Boston, April 8, 1874.
A true copy, Attest:
DAVID PULSIFER,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.[*]

Incorporated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ARTICLE 1. The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of America, to give a stronger and more general impulse and more systematic direction to scientific research, and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

ART. 2. The Association shall consist of Members, Fellows, Patrons, and Honorary Fellows.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in General Session.

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in General Session.[†]

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 6. Honorary Fellows of the Association, not exceeding three for each section, may be elected; the nominations to be made by the Standing Committee and approved by ballot in the respective sections before election by ballot in General Session. Honorary Fellows shall be entitled to all the privileges of fellows and shall be exempt from all fees and assessments, and entitled to all publications of the Association issued after the date of their election.

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[*] As amended at the Cincinnati Meeting, August, 1881, and to take effect at the Montreal Meeting, August, 1882.

[†] When this article was adopted in 1874 the following clause was added, but was stricken out as inoperative when the constitution was amended in 1881:—

"But all persons who may be members at the time of the adoption of this constitution may become fellows by signifying their desire to this effect before the first day of August, 1875."

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, an Assistant General Secretary, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Standing Committee shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the

Sections, which, after examination, he shall transmit with his own records to the Permanent Secretary within two weeks after the adjournment of the meeting. He shall receive proposals for membership and bring them before the Standing Committee.

ART. 13. The Assistant General Secretary shall be the Secretary of the Standing Committee. He shall give to the Secretary of each Section the titles of papers assigned to it by the Standing Committee.

ART. 14. The Permanent Secretary shall be the executive officer of the Association under the direction of the Standing Committee. He shall attend to all business not specially referred to committees nor otherwise constitutionally provided for. He shall keep an account of all business that he has transacted for the Association, and make annually a general report for publication in the annual volume of Proceedings. He shall attend to the printing and distribution of the annual volume of Proceedings, and all other printing ordered by the Association. He shall issue a circular of information to members and fellows at least three months before each meeting, and shall, in connection with the Local Committee, make all necessary arrangements for the meetings of the Association. He shall provide the Secretaries of the Association with such books and stationery as may be required for their records and business, and shall provide members and fellows with such blank forms as may be required for facilitating the business of the Association. He shall collect all assessments and admission fees, and notify members and fellows of their election, and of any arrearages. He shall receive, and bring before the Standing Committee, the titles and abstracts of papers proposed to be read before the Association. He shall keep an account of all receipts and expenditures of the Association, and report the same annually at the first meeting of the Standing Committee, and shall pay over to the Treasurer such unexpended funds as the Standing Committee may direct. He shall receive and hold in trust for the Association all books, pamphlets and manuscripts belonging to the Association, and allow the use of the same under the provisions of the Constitution and the orders of the Standing Committee. He shall receive all communications addressed to the Association during the interval between meetings, and properly attend to the same. He shall at each meeting report the names of fellows and members who have died since the preceding meeting. He shall be allowed a salary which shall be determined by the Standing Committee, and may employ one or more clerks at such compensation as may be agreed upon by the Standing Committee.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Assistant General Secretary, and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Standing Committee shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Assistant General Secretary, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The Committee shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other proceedings shall be published, and have the general direction of the publications of the Association; manage the financial affairs of the Association; arrange the business and programmes for General Sessions; suggest subjects for discussion, investigation or reports; nominate members and fellows; and receive and act upon all invitations extended to the Association and report the same at a General Session of the Association.

ART. 19. The Nominating Committee shall consist of the Standing Committee, and one member or fellow elected by each of the Sections. It shall be the duty of this Committee to meet at the call of the President and nominate the general officers for the following meeting of the Association. It shall also be the duty of this Committee to recommend the time and place for the next meeting. The Vice President and Secretary of each Section shall be recommended to the Nominating Committee by a sub-committee consisting of the Vice President, Secretary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one week or longer, at such time and place as may be determined by vote of the Association, and the preliminary arrangements for each meeting shall be made by the Local Committee, in conjunction with the Permanent Secretary and such other persons as the Standing Committee may designate.

ART. 21. General Sessions shall be held at 10 o'clock, A. M., unless otherwise ordered, on every day of the meeting, Sunday excepted, and at such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into nine Sections, namely:— A, *Mathematics and Astronomy;* B, *Physics;* C, *Chemistry, including its application to agriculture and the arts;* D, *Mechanical Science;* E, *Geology and Geography;* F, *Biology;* G, *Histology and Microscopy;* H, *Anthropology;* I, *Economic Science and Statistics.* The Standing Committee shall have power to consolidate any two or more Sections temporarily, and such consolidated Sections shall be presided over by the senior Vice President and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be three fellows elected by ballot after open nomination, who, with the Vice President and Secretary, shall form its Sectional Committee. The Sectional Committees shall have power to fill vacancies in their own numbers. Meetings of the Sections shall not be held at the same time with a General Session.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section nor Subsection until it has been received from the Standing Committee and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Standing Committee with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

ART. 31. The Permanent Secretary shall have the Proceedings of each meeting printed in an octavo volume as soon after the meeting as possible, beginning one month after adjournment. Authors must prepare their papers or abstracts ready for the press and forward them to the Permanent Secretary within this interval, otherwise only the abstracts or titles taken from the "title blanks" will appear in the printed volume. The Standing Committee shall have power to order the printing of any paper by abstract or title only. Whenever practicable, proofs shall be forwarded to authors for revision. If any additions or substantial alterations are made by the author of a paper after its submission to the Secretary, the same shall be distinctly indicated. Illustrations must be provided for by the authors of the papers, or by a special appropriation from the Standing Committee. Immediately on publication of the volume, a copy shall be forwarded to every member and fellow of the Association who shall have paid the assessment for the meeting to which it relates, and it shall also be offered for sale by the Permanent Secretary at such price as may be determined by the Standing Committee. The Standing Committee shall also designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in the objects of the Association and residing at or near the place of the proposed meeting. It is expected that the Local Committee, assisted by the officers of the Association, will make all essential arrangements for the meeting, and issue a circular giving necessary particulars, at least one month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be in the charge of the Permanent Secretary, who shall have a list of the same printed and shall furnish a copy to any member or fellow on application. Members and fellows who have paid their assessments in full shall be allowed to call for books and pamphlets, which shall be delivered to them at their expense, on their giving a receipt agreeing to make good any loss or damage and to return the same free of expense to the Secretary at the time specified in the receipt given. All books and pamphlets in circulation must be returned at each meeting. Not more than five books, including volumes, parts of volumes, and pamphlets, shall be held at one time by any member or fellow. Any book may be withheld from circulation by order of the Standing Committee.

ADMISSION FEE AND ASSESSMENTS.

ART. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 35. The annual assessment for members and fellows shall be three dollars.

ART. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

ART. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ACCOUNTS.

ART. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Standing Committee.

ALTERATIONS OF THE CONSTITUTION.

ART. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Election of members.
7. Election of fellows.
8. Unenumerated business.
9. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28).

MEMBERS.[3]

Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, Dr. Chas. C., Trenton, N. J. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Abert, Col. James W., Newport, Ky. (31).
Abert, S. Thayer, 1907 Pennsylvania Avenue, Washington, D. C. (30).
Acker, E. O'C., Ass't Eng'r, U. S. N., Norristown, Pa. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Adams, Capt. Robert C., Montreal, Can. (31).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Aikin, Prof. W. E. A., Baltimore, Md. (12).
Allen, Edmund T., 404 Market St., St. Louis, Mo. (27).
Allen, Dr. G. M., 102 Clark St., Cincinnati, Ohio (30).

[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. The payment of ten dollars at one time entitles a member to the subsequent volumes bound in cloth, or by the payment of twenty dollars, to the volumes bound in half morocco.

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications. The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allen, J. M., Hartford, Conn. (22).
Alling, J. C., Supt. City Schools, Albert Lea, Minn. (31).
Alling, Miss Mary R., Acra, Greene Co. N. Y. (29).
Allis, Solon M., Tucson, Arizona (29).
Ammen, Daniel, Rear Admiral U. S. Navy, Washington, D. C. (26).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Anderson, Newton M., Columbus, Ohio (30).
Andrews, L. W., Ph. D., Springfield, Mass. (31).
Angus, William, 214 St. James St., Montreal, Can. (31).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Archambault, U. E., Montreal, Can. (31).
Armstrong, Lucius H., St. Nicholas, Duval Co., Fla. (30).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Arnold, Lauren B., Rochester, N. Y. (30).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard M., Millville, N. J. (29).
Atwood, E. S., East Orange, N. J. (29).
Atwood, Oscar, Rutland, Vt. (31).
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).
Austin, Miss Mary, Mattapan Station, Milton, Mass. (31).
Ayres, Prof. Brown, Univ. of Louisiana, New Orleans, La. (31).
Ayres, Dr. S. C., 64 W. 7th St., Cincinnati, Ohio (30).

Babbit, John, Fredericton, N. B. (29).
Bailey, E. H. S., Bethlehem, Pa. (25).
Baker, A. L., 32 So. Charles St., Baltimore, Md. (30).
Baker, B. N., 32 So. Charles St., Baltimore, Md. (30).
Baker, Frank, M. D., 366 C St., Washington, D. C. (31).
Baker, Prof. I. O., Illinois Industrial Univ., Champaign, Ill. (30).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29). [(31).
Baldwin, Miss Mary A., Summer Ave., Cor. Kearney St., Newark, N. J.
Baldwin, Very Rev. Dean Maurice S., Montreal, Can. (31).
Balen, Abraham D., Plainfield, N. J. (31).
Ballard, H. H., Lenox, Mass. (31).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., 105 Dearborn Avenue, Chicago, Ill. (29).
Banes, Charles H., 2416 Spring Garden St., Philadelphia, Pa. (31).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).
Barclay, Robert, A. B., St. Louis, Mo. (30).
Barker, Martha M., care of H. R. Barker, Steam and Gas Fixtures Lowell, Mass. (31).

Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Hotel Vendôme, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. H. (28).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
BASSNETT, THOMAS, Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Batterson, J. G., Hartford, Conn.(23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beadle, Delos W., St. Catharines, Ontario, Can. (31).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Beall, Thomas, Lindsay, Ontario, Can. (31).
Beamer, Miles (30).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beaudry, J. Alphonse U., 97 St. James St., Montreal, Can. (31).
Beauregard, Gen. Gustave T., New Orleans, La. (30).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Coleman's Station, N. Y. [(29).
Beebe, Wm. S., Brevet Major, U. S. Army, 25 Grace Court, Brooklyn, N. Y. (30).
Belknap, Chas., Lieut. U. S. N., U. S. S. Vandalia, care Navy Dept., Washington, D. C. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
Bell, Alex. Melville, 88 Fayette St., Georgetown, D. C. (31).
Bell, John J., Exeter, N. H. (22).
Bell, Thomas D., McGill College, Montreal, Can. (31).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. [(25).
Bemrose, Joseph, 158 P. O., Montreal, Can. (31).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).
Benjamin, Marcus, 43 East 67th St., New York, N. Y. (27).
Berliner, Emile, 95 Milk St., Boston, Mass. (29).
Best, W. F., St. John, N. B. (29).
Beyer, H. G., Passed Ass't Surgeon, U. S. N., 125 Park Ave., Baltimore, Md. (31).
Bickmore, Mrs. Albert S., 12 East 41st St,, New York, N. Y. (31).

Bicknell, Hon. Thos. W., Boston, Mass. (29).
Bigelow, Otis, 605 7th St., Washington, D. C. (30).
Bigelow, Wm. Sturgis, M. D., 61 Beacon St., Boston, Mass. (29).
Bill, Charles, Springfield, Mass. (17).
Birge, Charles P., Keokuk, Iowa (29).
Blackford, Eugene G., 809 Bedford Avenue, Brooklyn, N. Y. (29).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Blackwell, Mrs. A. B., Somerville, N. J. (30).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake, Prof. John R., Davidson College, N. C. (29).
Blakslee, Prof. T. M., Harpersville, N. Y. (31).
Blanchard, Frederick, Box CC, Lowell, Mass. (29).
Blatchford, Eliphalet W., 375 North La Salle St., Chicago, Ill. (17).
Blatchford, John S., 13 Exchange St., Boston, Mass. (29).
Blodgett, A. N., M.D., Boylston St., Boston, Mass. (29).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).
Bobbitt, Mrs. John, Greensburg, Decatur Co., Ind. (30).
Boerner, Chas. G., Vevay, Ind. (29).
Bolles, Dr. Wm. P., 571 Dudley St., Dorchester, Mass. (29).
Bond, Geo. W., Boston, Mass. (29).
Bookwalter, J. W., Springfield, Ohio (30).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., Box 615, Brookline, Mass. (29).
Bowers, Miss Virginia K., care Wm. Russell, Memphis Appeal Office, Memphis, Tenn. (27).
Bowker, W. H., 43 Chatham St., Boston, Mass. (30).
Bowles, George J., 1466 St. Catherine St., Montreal, Can. (31).
Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
Brackett, John W., 581 Washington St., Boston, Mass. (29).
Brackett, Solomon H., St. Johnsbury, Vt. (29).
Bradford, Edward H., M. D., 150 Boylston St., Boston, Mass. (29).
Bradford, Royal B., Lt. Comd'r, U. S. N., Torpedo Station, Newport, R. I. (31).
Bradley, C. W., cor. 3d and Walnut Sts., Cincinnati, Ohio (27).
Bragg, Everett B., No. 3 Park Place, New York, N. Y. (29).
Brainerd, Thos. C., P. O. Box 720, Montreal, Can. (31).
Brandegee, T. S., Berlin, Conn. (29).
Branlette, Mrs. T. E., Louisville, Ky. (29).
Branstrup, Dr. W. T., Vincennes, Ind. (30).
Bray, Prof. C. D., College Hill, Mass. (29).
Bray, Mrs. Maria H., West Gloucester, Mass. (29).

Breckinridge, S. M., Box 2794, St. Louis, Mo. (27).
Brewster, William, 61 Sparks St., Cambridge, Mass. (29).
Brissette, Milton H., Montreal, Can. (31).
Bronnson, W. H., Lt. Comd'r U. S. N., U. S. C. and G. Survey, St'r "Blake," Navy Yard, New York, N. Y. (31).
Bronson, E. H., Ottawa, Ontario, Can. (31).
Brooks, Miss Margarette W., Salem, Mass. (29).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brounell, Prof. Walter A., 725 Irving St., Syracuse, N. Y. (30).
Brown, Miss Anne, 528 West 7th St., Cincinnati, Ohio (31).
Brown, Arthur, C. E., Oakland, Alameda Co., Cal. (31).
Brown, Asa Warren, Newport, Ky. (30).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Prof. C. J., Randolph, N. Y. (31).
Brown, Fred. G., care of McKesson and Robbins, 93 Fulton St., New York, N. Y. (31).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
Brown, Jonathan, 390 Broadway, Somerville, Mass. (29).
Brühl, Gustav, N. W. cor. John and Hopkins Sts., Cincinnati, Ohio (28).
Bryant, Wm. S., 61 Beacon St., Boston, Mass. (30).
Buck, C. Elton, Wilmington, Del. (29).
Buck, Henry C., West Somerville, Mass. (29).
Buckingham, Chas. L., U. S. Patent Office, Washington, D. C. (28).
Buffum, Miss Fannie A., Linden, Mass. (29).
Bull, John, Room 30, Corcoran Building, Washington, D. C. (31).
Bulloch, Walter H., 99 W. Monro St., Chicago, Ill. (30).
Burdick, Edson A., 408 Spruce St., Le Droit Park, Washington, D. C. (30).
Burke, Jos. C., Middletown, Conn. (29).
Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burland, Jeffrey H., Montreal, Can. (31).
Burpee, J. P. C., St. John, N. B. (30).
Burr, Prof. William H., Troy, N. Y. (31).
Bush, Rev. Stephen, D. D., Waterford, N. Y. (19).
Butcher, Mrs. Z. D., Washington, D. C. (29).
Butler, A. W., Brookville, Franklin Co., Ind. (30).
Butler, Cyrus, 24 Cliff St., New York, N. Y. (29).
Bynner, Edwin L., 13 Pemberton Square, Boston, Mass. (29).
Byrnes, R. M., M. D., cor. 5th and Race Sts., Cincinnati, Ohio (30).

Cabot, Sam'l, jr., 53 Kilby St., Boston, Mass. (29).
Calder, Edwin E., Box 17, Providence, R. I. (29).
Caldwell, Albert G., Lt. Comd'r U. S. N., Torpedo Station, Newport, R. I. (31).
Caldwell, Dr. Frank, Cincinnati Hospital, Cincinnati, Ohio (30).
Calkins, Dr. Marshall, Springfield, Mass. (29).

Campbell, Rev. Prof. John, Presbyterian College, Montreal, Ca
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25
Capen, Miss Bessie T., Northampton, Mass. (28).
Cargill, H. L., St. Lawrence Hotel, Montreal, Can. (31).
Carman, Ezra A., Dept. of Agric., Washington, D. C. (30).
Carman, Lewis, Bangall, N. Y. (29).
Caron, C. K., Louisville, Ky. (30).
Carpenter, Geo. O., jr., care St. Louis Lead & Oil Co., St. Louis,
Carpmael, Charles, Director of Magnetic Observatory, Toronto,
Carrington, Col. Henry B., U. S. A., care Adj. Gen. U. S. A., Wa
D.C. (20).
Case, Col. Theo. S., Ed. Western Review of Science, Kansas City,
Cassino, S. E., Naturalists' Agency, 41 Arch St., Boston, Mas
Castle, Frederick A., M. D., 102 East 57th St., New York, N. Y.
Castner, Dr. H. Y., 197 Pearl St., New York, N. Y. (30).
Caswell, John H., 11 West 48th Street, New York, N. Y. (26).
Chabert, L'Abbé Joseph, 80 St. Gabriel St., Montreal, Can. (31).
Chace, Arnold B., Valley Falls, R. I. (29).
Chadbourn, Erlon R., Lewiston, Me. (29).
Chadwick, James R., M. D., Clarendon St., Boston, Mass. (29).
Chance, H. Martyn, 2438 Fairmount Avenue, Philadelphia, Pa. (
Chandler, Prof. Chas. Henry, Ripon, Wis. (28).
Channing, Dr. Wm. F., Providence, R. I. (29).
Chapman, F. M., Danforth, Iroquois Co., Ill. (17).
Chapman, Henry C., M. D., 1226 Walnut St., Philadelphia, Pa. (2
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (2
Chase, Prof. F. A., Fisk University, Nashville, Tenn. (26).
Chase, Mrs. Marine J., 1622 Locust St., Philadelphia, Pa. (31).
Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
Chatfield, A. F., Albany, N. Y. (29).
Chenault, Prof. J. W., Louisville, Ky. (30).
Chester, Lt. Com'dr Colby M., U. S. N., Coast Survey Office, Was
D. C. (28).
Childe, John Healey, Boston, Mass. (31).
Cisco, J. G., Jackson, Tenn. (29).
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Ma
Clark, John S., 64 Pinckney St., Boston, Mass. (31).
Clark, Simeon T., M.D., Lockport, Niagara Co., N. Y. (25).
Clarke, Miss Cora H., Jamaica Plain, Mass. (29).
Clarke, Prof. John M., Northampton, Mass. (31).
Clarke, Maurice D., 68 Ohio St., East Cambridge, Mass. (29).
Clarke, Robert, Cincinnati, Ohio (30).
Cleveland, J. L., 547 W. 8th St., Cincinnati, Ohio (30).
Clevenger, Dr. S. V., 189 37th St., Chicago, Ill. (29).
Cloud, Jno. W., Altoona, Pa. (28).
Coakley, George W., LL.D., Hempstead, L. I. (29).

Cobb, Samuel C., 235 Boylston St., Boston, Mass. (29).
Cobb, Rev. Sanford H., Saugerties, N. Y. (29).
Coffin, Amory, Phœnixville, Pa. (31).
Coffinberry, W. L., Grand Rapids, Mich. (20).
Colburn, Richard T., Elizabeth, N. J. (31).
Colby, C. E., M. E., School of Mines, Columbia College, New York, N. Y. (28).
Cole, Frederick, 1394 St. Catherine St., Montreal, Can. (31).
Colie, Edw. M., East Orange, N. J. (30).
Collier, M. Dwight, St. Louis, Mo. (27).
Colman, Henry, M.D., Swampscott, Mass. (25).
Colton, G. Woolworth, 182 William St., New York, N. Y. (22).
Comstock, Dr. T. Griswold, 507 North 14th St., St. Louis, Mo. (29).
Conger, Rev. Charles L., 1729 Eastern Avenue, Cincinnati, Ohio (30).
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Youmans, Wm. Jay, M. D., Popular Science Monthly, 549 Broadway, New York, N. Y. (28).
Young, Prof. Andrew H., Hanover, Ind. (30).

Zimmerman, Charles D., Buffalo, N. Y. (30).
Zimmerman, William, Room 22, 142 Dearborn St., Chicago, Ill. (30).
Zinke, E. Gustav, M. D., Elm St., near 14th, Cincinnati, Ohio (30).
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[1407 MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

HONORARY FELLOW.[1]

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Abbe, Prof. Cleveland, Washington, D. C. (16). 1874.
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Alexander, John S., 1935 Arch St., Philadelphia, Pa. (20). 187
Allen, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29).
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Alvord, Henry E., C. E., Houghton Farm, Mountainville, O N. Y. (29). 1882.
Anthony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 18
Appleton, Prof. John H., Brown Univ., Providence, R. I. (22).
Ashburner, Wm., 1014 Pine St., San Francisco, Cal. (29). 1882
Atkinson, Edward, 131 Devonshire St., Boston, Mass. (29). 18
Atwater, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29
Austen, Peter T., Ph. D., Rutgers College, Lock Box No. 2, N wick, N. J. (26). 1879.
Austin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

Bailey, Prof. Loring W., University of Fredericton, N. B. (18).
Bailey, Prof. W. W., Brown University, Providence, R. I. (18
Baird, Prof. S. F., Sec'y Smithsonian Inst., Washington, D. C. (
Baker, Marcus, Coast Survey Office, Washington, D. C. (30).
Ballou, Geo. F., Waltham, Mass. (29). 1881.
Bandelier, Ad. F., Highland, Ill. (25). 1879.
BARKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1
Barnard, F. A. P., President Columbia College, New York, N. Y.
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Bartlett, J. R., Comd'r U. S. N., care of C. & G. Survey Office, ton, D. C. (30). 1882.
Bassett, Homer F., Waterbury, Conn. (23). 1874.
Batchelder, Dr. J. H., Salem, Mass. (18). 1874.
Batchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8).
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Belknap, George E., Capt. U. S. N., care Fred. L. Wheeler & 3522, Boston, Mass. (29). 1881.

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[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitut

*** The number in parenthesis indicates the meeting at which the mem the Association; the date at the end of the line the time when made a Fello

When the name is given in small capitals, it designates that the Fello life member, and is entitled to the Annual Volume of Proceedings.

* Deceased.

Bell, Dr. Alex. Graham, Scott Circle, 1500 Rhode Island Ave., Washington, D. C. (26). 1879.
Bell, Samuel N., Manchester, N. H. (7). 1874.
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880.
Blair, Henry W., U. S. C. & G. Survey Office, Washington, D. C. (26). [1882.
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Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, Auburndale, Mass. (23). 1874.
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Borden, Spencer, Fall River, Mass. (29). 1882.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
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Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
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Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Britton, N. L., Columbia College, New York, N. Y. (29). 1882.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., care of Yale College Observatory, New Haven, Conn. (11). 1874.
Brown, Mrs. Robert, jr., New Haven, Conn. (17). 1874.
BRUSH, PROF. GEORGE J., Yale College, New Haven, Conn. (4). 1874.
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass. (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.
Burrill, Prof. T. J., Illinois Industrial Univ., Champaign, Ill. (29). 1882.

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Call, Prof. R. Ellsworth, David City, Neb. (29). 1882.
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Capen, Rev. Francis L., Boston, Mass. (23). 1874.
Carhart, Prof. Henry S., Evanston, Ill. (29). 1881.
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Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peabody Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Chamberlain, Wm. I., Sec. Ohio State Bd. of Agric., Columbus, Ohio (30). 1882.

Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., 50th S
Ave., New York, N. Y. (19). 1875.
Chandler, Seth C., jr., Harvard Observatory, Cambridge, M
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19).
Chanute, O., care Erie Railway, New York, N. Y. (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29)
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, I
1877.
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(29). 1882.
Clark, Alvan G., Cambridgeport, Mass. (28). 1880.
Clark, Prof. John E., Mathematics, Yale College, New Haven, C
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D
1874.
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Collett, Prof. John, Chief Indiana Bureau of Statistics of
Indianapolis, Ind. (17). 1874.
Collier, Peter, Dept. of Agric., Washington, D. C. (29). 1882.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Alba
(28). 1880.
Comstock, J. Henry, Cornell Univ., Ithaca, N. Y. (28). 1882.
Comstock, Prof. M. L., Ph. D., Galesburg, Knox Co., Ill. (21).
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (1
Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1
Cox, Prof. Edward T. (19). 1874.
Cox, Hon. Jacob D., 103 Broadway, Cincinnati, Ohio (30). 1881
Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879.
Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874.
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Cummings, Rev. Joseph, D.D., Pres. Northwestern Univ., Evan
(18). 1874.
Curtis, Dr. Josiah, Washington, D. C. (18). 1874.
Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17).

Dabney, Chas. W., jr., Ph. D., Agric. Experiment Station,
N. C. (30). 1882.
Dall, Mrs. Caroline H., 31st St., Washington, D. C. (18). 1874.
Dall, Wm. H., Smithsonian Institution, Washington, D. C. (18).
Dana, Edward Salisbury, New Haven, Conn. (23). 1875.
Dana, Prof. James D., New Haven, Conn. (1). 1875.

Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11). 1874.
Davidson, Prof. Geo., Asst. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881.
Dawson, Dr. J. W., Principal McGill Coll., Montreal, Canada (10). 1875.
Day, F. H., M. D., Wauwatosa, Wis. (20). 1874.
Dean, George W., U. S. Coast Survey, Fall River, Mass. (15). 1874.
Dimmock, George, Cambridge, Mass. (22). 1874.
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Dodge, Charles R., Washington, D. C. (22). 1874.
Dolbear, A. Emerson, College Hill, Mass. (20). 1880.
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Drown, Prof. Thos. M., Lafayette College, Easton, Pa. (29). 1881.
Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882.
Dudley, Charles B., Altoona, Pa. (23). 1882.
Dudley, Wm. L., Miami Med. College, Cincinnati, Ohio (28). 1881.
Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880.
Dutton, Capt. C. E., U. S. Ord. Dep't, Washington, D. C. (28). 1875.
Dwight, Prof. Wm. B., Vassar College, Poughkeepsie, N. Y. (30). 1882.
Dwyer, John, M. D., Hartford, Conn. (23). 1874.

Eads, Jas. B., 314 Chestnut Street, St. Louis, Mo. (27). 1879.
Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). 1879.
Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19). 1874.
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, Ohio (24). 1875.
Edison, Thos. A., Menlo Park, N. J. (27). 1878.
Edmands, J. Rayner, Observatory, Cambridge, Mass. (29). 1880.
Egleston, Prof. Thos., School of Mines, Columbia College, New York, N. Y. (27). 1879.
Eimbeck, Wm., U.S.C.S., Washington, D. C. (17). 1874.
Elliott, Arthur H., School of Mines, Columbia College, New York, N. Y. (28). 1880.
Elliott, Ezekiel B., Treasury Dept., Washington, D. C. (10). 1874.
Elsberg, Louis, M.D., 614 Fifth Avenue, New York, N. Y. (23). 1874.
Emerson, Prof. Benjamin K., Amherst, Mass. (19). 1877.
Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
Emerton, James H., Salem, Mass. (18). 1875.
Emmons, S. F., Box 2836, Denver, Col. (26). 1879.
Engelmann, Dr. George, St. Louis, Mo. (1). 1875.
Engelmann, Geo. J., M. D., 3003 Locust St., St. Louis, Mo. (25). 1878.
Ernst, Carl W., Daily Advertiser, Boston, Mass. (23). 1874.
Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph. D., St. Johnsbury, Vt. (14). 1874.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.

Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Farquharson, Dr. Robert James, Des Moines, Iowa (24). 1880.
Ferguson, Maj. Thos. B., Ass't U. S. Fish Commissioner, Washing D. C. (28). 1881.
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (1881.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 18
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874.
Fletcher, Dr. Robert, Surgeon General's Office, Washington, D. C. 1881.
Flint, James M., Surgeon U. S. N., 42 Dartmouth St., Boston, Mass. 1882.
Foote, Dr A. E., 1228 Belmont Avenue, Philadelphia, Pa. (21). 1874
Forbes, Prof. S. A., Normal, Ill. (27). 1879.
Frazer, Dr. Persifor, Ass't Geologist, 2d Geol. Survey of Pa., Clinton St., Philadelphia, Pa. (24). 1879.
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24).
Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 188
Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882.

Gage, Simon Henry, Ithaca, N. Y. (28). 1881.
Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874.
Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25).
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, (25). 1877.
Garman, S., Museum Comp. Zoology, Cambridge, Mass. (20). 1874.
Gatschet, Albert S., Bureau of Ethnology, Washington, D. C. (30).
Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1
Gilbert, G. K., Box 240, Salt Lake City, Utah (18). 1874.
Gillman, Henry, 155 Adams Avenue East, Detroit, Mich. (24). 187
Gilman, Daniel C., President of the Johns Hopkins University, Balti Md. (10). 1875.
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1
Goldschmidt, S. A., Ph. D., Morse Building, 140 Nassau St., New N. Y. (24). 1880.
Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 18
Goode, Prof. G. Brown, Curator National Museum, Washington, (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washi D. C. (24). 1879.
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1). 1875.
Green, Traill, M. D., Easton, Pa. (1). 1874.
Grimes, J. Stanley (17). 1874.

Grote, Aug. R., New Brighton, Staten Island, N. Y. (22). 1875.
Guyot, Prof. Arnold, Princeton, N. J. (1). 1875.

Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17). 1875.
Hague, Arnold, 10 West 29th St., New York (26). 1879.
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Edwin H., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hale, Horatio, Clinton, Ontario, Can. (30). 1882.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.
Harger, Oscar, Yale College Museum, New Haven, Conn. (25). 1879.
HARKNESS, PROF. WM., U. S. N., U. S. Observatory, Washington, D. C. (26). 1878.
Harrington, Prof. Mark W., Ann Arbor, Mich. (22). 1875.
Harrison, Dr. B. F., Wallingford, Conn. (11). 1874.
Hasbrouck, Prof. I. E., Rutgers Coll., New Brunswick, N. J. (23). 1874.
Haskins, Rev. David Greene, Cambridge, Mass. (25). 1874.
Hastings, C. S., Johns Hopkins University, Baltimore, Md. (25). 1878.
Hazen, Brig. Gen. Wm. B., Chief Signal Office, U. S. A., Washington, D. C. (30). 1882.
Hedrick, B. S., Exam'r U. S. Pat. Office, Washington, D. C. (19). 1874.
Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, Franklin C., P. O. Box 338, Princeton, N. J. (29). 1881.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 788 Congress St., Portland, Me. (3). 1875.
Hill, Walter N., Chester, Pa. (29). 1881.
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hitchcock, Romyn, 53 Maiden Lane, New York, N. Y. (29). 1881.
Hodges, N. D. C., Salem, Mass. (29). 1882.
Hoffmann, Dr. Fred., 797 Sixth Ave., New York, N. Y. (28). 1881.
Holden, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881.
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.

Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10). 1874.
Hyde, Prof. E. W., Walnut Hills, Cincinnati, Ohio (25). 1881.

James, Jos. F., 177 Race St., Cincinnati, Ohio (30). 1882.
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Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Jewell, Theo. F., Lt. Comdr. U.S.N., Torpedo Station, Newport (25). 1882.
Jillson, Dr. B. C., Illinois Industrial Univ., Champaign, Ill. (14).
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22).
Johnson, Prof. W. W., Annapolis, Md. (29). 1881.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24)

Kedzie, Prof. Robert C., Lansing, Mich. (29). 1881.
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Kennan, George, 1318 Mass. Avenue, Washington, D. C. (30). 188
Kent, William, Pittsburgh, Pa. (26). 1881.
Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 188
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23).
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18).
Lanza, Prof. Gaetano, Mass. Inst. of Technology, Boston, Mass 1882.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15).
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Lazenby, Prof. Wm. R., Ithaca, N. Y. (30). 1882.
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LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881.
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Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 18
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philad Pa. (2). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26)
LILLY, GEN. WM., Mauch Chunk, Carbon Co., Pa. (28). 1882.
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Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22).
Little, W. C., Albany, N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 187
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 18
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.

Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881.
Loudon, Prof. James, Toronto, Can. (25). 1881.
Loughridge, Prof. R. H., Berkeley, Cal. (21). 1874.
Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Captain U. S. N., Navy Yard, Boston, Mass. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Capt. of Ordnance U. S. A., National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Hon. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

Mabery, C. F., Cambridge, Mass. (29). 1881.
Macfarlane, James, Towanda, Pa. (29). 1882.
Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882.
Mallery, Brevet Lieut. Col. Garrick, U. S. Army, Box 585, Washington, D. C. (26). 1879.
MANN, B. PICKMAN, U. S. Dep't of Agric., Washington, D. C. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
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MARSH, PROF. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. Daniel S., Rutgers Female College, New York, N. Y. (23). 1879.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayer, Prof. A. M., So. Orange, N. J. (19). 1874.
McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 321 Michigan Ave., Chicago, Ill. (29). 1881.
McGee, W. J., U. S. Geol. Survey, Salt Lake City, Utah (27). 1882.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McNair, Lt. Commander Antoine R., U. S. N., Box 1071, Saratoga Springs, N. Y. (28). 1882.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Louisville, Ky. (24). 1876.
Mendenhall, Prof. T. C., Columbus, Ohio (20). 1874.
Merriman, C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882.

Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22).
Morley, Prof. Edward W., Hudson, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18).
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22)
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13)
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882.
Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24).
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia Coll
York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13).
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, E. L., Ph. D., Central Univ. Richmond, Ky. (28). 1881
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18)
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24)
Norton, Prof. W. A., New Haven, Conn. (6). 1875.
Norwood, Charles J., Ass't Kentucky Geological Survey, Ru
Ky. (26). 1881.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 187
Ordway, Prof. John M., Mass. Inst. Technology, Boston,
1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanica
Columbus, Ohio (19). 1875.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22)
Osler, Prof. William, McGill College, Montreal, Canada (28). 1
Owen, Dr. Richard, New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., jr., 115 Angell St., Providence, R. I. (16).
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen.
Office of Chief of Engineers, Washington, D. C. (29). 1881
Parkhurst, H. M., Law Stenographer, 25 Chambers St., New Y
(23). 1874.
Peckham, S. F., Providence, R. I. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peet, Rev. Stephen D., Clinton, Wis. (24). 1881.
Peirce, B. O., Beverly, Mass. (18). 1874.
Penhallow, Prof. D. P., Experiment Station, Mountainville, N
1882.

Perkins, Prof. George H., Burlington, Vt. (17). 1882.

Perkins, Maurice, Schenectady, N. Y. (15). 1875.

Peter, Dr. Robert, Ky. Geol. Survey, Lexington, Ky. (29). 1881.

Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.

Phillips, A. W., New Haven, Conn. (24). 1879.

Phippen, Geo. D., Salem, Mass. (18). 1874.

Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.

Pike, Dr. William H., University College, Toronto, Can. (29). 1881.

Pilling, James C., Box 585, Washington, D. C. (28). 1882.

Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882.

Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879.

Powell, Major J. W., U. S. Geologist, 910 M St., Washington, D. C. (23). 1875.

Prentiss, D. Webster, M. D., Washington, D. C. (29). 1882.

Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.

Prime, Frederick, jr., Allentown Iron Co., 230 So. 3d St., Philadelphia, Pa. (24). 1875.

Pritchett, Henry S., Observatory, Washington Univ., St. Louis, Mo. (29). 1881.

Procter, John R., Director Ky. Geol. Survey, Frankfort, Ky. (26). 1881.

Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.

Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.

Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.

Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.

Pynchon, Rev. T. R., Pres. Trinity College, Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.

Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.

Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.

Redfield, J. H., care A. Whitney & Sons, Philadelphia, Pa. (1). 1874.

Reed, E. Baynes, London, Ontario, Can. (27). 1882.

Rees, Prof. John K., Washington University, St. Louis, Mo. (26). 1878.

Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875.

Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881.

Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.

Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.

Richards, Mrs. Robert H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.

Ricketts, Pierre de Peyster, Ph.D., 10 Cedar St., New York, N. Y. (26). 1880.

RILEY, PROF. C. V., U. S. Entomologist, 1700 13th St. N.W., Wa D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Rockwell, Gen. Alfred P., Office Great Falls Manufacturing Co. Mass. (10). 1882.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, 1874.
Rogers, Prof. Robert E., Jefferson Medical College, Philadel (18). 1874.
Rogers, W. A., Ass't Prof. Harvard College Observ., Cambrid (15). 1875.
Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.
Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, 1875.
Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.
Ross, Waldo O., 189 Devonshire St., Boston, Mass. (29). 1882.
Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.
Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1
Russell, I. C., U. S. Geological Survey, Salt Lake City, Utah (2
Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (18).

Safford, Dr. James M., Nashville, Tenn. (6). 1875.
Sampson, Comdr. W. T., U. S. N., Naval Observatory, Washing (25). 1881.
Saunders, William, London, Ontario, Canada (17). 1874.
Schanck, Prof. J. Stillwell, Princeton, N. J. (4). 1882.
Schott, Charles A., U. S. Coast and Geodetic Survey Office, W D. C. (8). 1874.
Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24)
SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874.
Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, 1874.
Seiler, Carl, M. D., Philadelphia, Pa. (29). 1882.
Sestini, Rev. B., Woodstock, Md. (29). 1882.
Sheafer, P. W., Pottsville, Pa. (4). 1879.
Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1
Sigsbee, Chas. D., Comd'r U. S. N., Navy Dep't, Washington, 1882.
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.
Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1
Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19).
Skinner, Joseph J., New Britain, Conn. (23). 1880.
Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20
Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (
Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (
Smith, Dr. J. Lawrence, Louisville, Ky. (1). 1874.
SMITH, QUINTIUS C., M. D., Box 707, Austin, Texas (26). 188

Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875.
Smock, Prof. John Conover, Box 282, New Brunswick, N. J. (28). 1879.
Snow, Prof. F. H., Lawrence, Kan. (29). 1881.
Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882.
Spencer, Prof. J. William, Univ. of Missouri, Columbia, Mo. (28). 1882.
Spencer, John W., Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, Box 573, Cincinnati, Ohio (24). 1880.
Stallo, J. B., Masonic Temple, Cincinnati, Ohio (30). 1882.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, George H., Colorado Springs, Col. (29). 1882.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Leander McCormick Observatory, Univ. of Va., Va. (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Sturtevant, E. Lewis, M. D., Geneva, N. Y. (29). 1882.
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20). 1881.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.
Swift, Lewis, Ph. D., Rochester, N. Y. (29). 1882.

Tainter, Sumner, 1221 Connecticut Ave., Washington, D. C. (29). 1881.
Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Taylor, Arthur F., Ph. D., Case School of Applied Science, Cleveland, Ohio (29). 1882.
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29). 1881.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thomas, Benj. F., Ph. D., Univ. State of Missouri, Columbia, Mo. (29). 1882.
Thurston, Prof. R. H.,Stevens Institute, Hoboken, N. J. (23). 1875.
Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881.
Townshend, Prof. N. S., Columbus, Ohio (17). 1881.
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27). 1881.
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.

True, Fred W., U. S. National Museum, Washington, D. C.
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.
Upham, Warren, Nashua, N. H. (25). 1880.

Van der Weyde, P. H., M. D., Box 3619, New York, N. Y. (1
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28).
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1
Vose, Prof. George L., Mass. Inst. Technology, Boston, Mass

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Muse
paratlve Zoology, Cambridge, Mass. (23). 1874.
Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (
Waldo, Leonard, S. D., New Haven, Conn. (28). 1880.
Walker, Prof. J. R., 154 Canal St., New Orleans, La. (19).
Walker, Prof. Joseph B. (20). 1874.
Wallace, Wm., Ansonia, Conn. (28). 1882.
WALLER, E., School of Mines, Columbia College, New York,
1874.
Walling, H. F., U. S. Coast and Geodetic Survey Office, W
D. C. (16). 1874.
Ward, Prof. Henry A., Rochester, N. Y. (18). 1875.
Ward, Lester F., U. S. Geological Survey, Washington,
1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warder, Prof. Robert B., North Bend, Hamilton Co., Ohio (1
WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 187
Warren, Cyrus M., Brookline, Mass. (29). 1882.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich.
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23)
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Maywood, Ill. (21). 1874.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington,
1879.
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17).
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25

Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1880.
Whitney, Solon F., Watertown, Mass. (20). 1874.
Whittlesey, Col. Charles, 1305 Euclid Ave., Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (28). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Dept. of Agric., Washington, D. C. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, H. S., Cornell Univ., Ithaca, N. Y. (18). 1882.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (8). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Winsor, Justin, Librarian Harvard Univ., Cambridge, Mass. (29). 1882.
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881.
Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875.
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874.

Zentmayer, Joseph, 147 So. Fourth St., Philadelphia, Pa. (29). 1882.

[515 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 1923.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.

Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.

Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y

Adams, Edwin F., Charlestown, Mass. (18). [19, 1853.

Adams, Samuel, Jacksonville, Ill. (18). Born Dec.19, 1806. Died April 29, 1877.

Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec.

Ainsworth, J. G., Barry, Mass. (14). [14, 1873.

Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.

Allen, Zachariah, Providence, R. I. (1). Died March 17, 1882.

Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.

Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.

Andrews, Ebenezer B., Lancaster, Ohio (7). Died Aug. 21, 1880, aged 59.

Anthony, Charles H., Albany, N. Y. (6). Died in 1874.

Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14, 1861.

Armstrong, John W., Fredonia, N. Y. (24).

Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11, 1878.

Aufrecht, Louis, Cincinnati, Ohio (30).

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.

Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.

Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.

Bardwell, F. W., Lawrence, Kan. (18). Died in 1878.

Barnard, John G., New York, N. Y. (14). Died May 14, 1882.

Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.

Barry, Redmond, Melbourne, Australia (25).

Beck, C. F., Philadelphia, Pa. (1).

Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.

Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.

Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.

Benedict, George W., Burlington, Vt. (16). Born Jan. 11, 1796. Died in 1871.

Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19,1877.

Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18, [1847.

Binney, John, Boston, Mass. (3).

Blackie, Geo. S., Nashville, Tenn. (26).

Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.

Blanding, William, ———, R. I. (1).

Blatchford, Thomas W., Troy, N. Y. (6).

Blatchley, Miss S. L., New Haven, Conn. (19).

Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.

Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.

Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.

Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.

Braithwaite, Jos., Chambly, C. W. (11).

Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.

Briggs, Robert, Philadelphia, Pa. (29).

Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.

Brown, Andrew, Natchez, Miss. (1).

Burnap, G. W., Baltimore, Md. (12). Born Nov. 30, 1802. Died Sept. 8, 1859.

Burnett, Waldo I., Boston, Mass. (1). Died July 1, 1854, aged 27.

Butler, Thomas B., Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, F. A., New York, N. Y. (27). Died in 1879.

Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.

Carpenter, Thornton, Camden, S. C. (7).

Carpenter, William M., New Orleans, La. (1).

Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5, 1880.

Case, William, Cleveland, Ohio (6).

Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.

Chadbourne, Paul Ansel, Amherst, Mass. (10). Born Oct. 21, 1823. Died Feb. 23, 1883.

Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.

Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.

Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec. 13, 1870.

Cheney, Miss Margaret S., Jamaica Plain, Mass. (29).

Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

Clark, Henry James, Cambridge, Mass. (18). Died July 1, 1873, aged 47.
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.
Cramp, J. M., Wolfville, N. S. (11). Born July 25, 1796. Died Dec. 6, 1881.
Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.
Croswell, Edwin, Albany, N. Y. (6). Born in May, 1797. Died June 13,
Curry, W. F., Geneva, N. Y. (11). [1871.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.
Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born in 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15,
Dexter, G. M., Boston, Mass. (11). [1867.
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Draper, Henry, New York, N. Y. (28).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10). Died Aug. 9, 1855, aged 54.
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14).
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1). Born Sept. 12, 1797. Died March 4, 1881.
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16, 1873.
Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8,
Fisher, Mark, Trenton, N. J. (10). [1874.
Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.
Fitch, O. H., Ashtabula, Ohio (7). Died Sept. 17, 1882, in his 80th year.
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb G., New Orleans, La. (21). Died in Aug., 1881.
Foster, John W., Chicago, Ill. (1). Born March 4, 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6,
Fox, Charles, Grosse Ile, Mich. (7). [1865.
Frazer, John F., Philadelphia, Pa. (1). Born July 8, 1812. Died Oct. 14,
French, J. W., West Point, N. Y. (11). [1872.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1). Died Jan. 12, 1850, aged 46.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10). Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus A., Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859.

Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880, aged 68.
Hale, Enoch, Boston, Mass. (1). Born Jan. 29, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (1). Born Jan. 17, 1781. Died May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1). Died April 20, 1853, aged 57.
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1). Died July 27, 1866, aged 80.
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878.
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb. 28, 1828. Died Mch. 5, 1875.
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.
Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.

Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born June 15, 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Born June 21, 1805. Died Aug. 29, 1880.
James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, George W., Waterville, Me. (1). Died in 1878.
Keep, N. C., Boston, Mass. (18). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.
King, Mitchell, Charleston, S. C. (8). Born June 8, 1783. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicagó, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died
LaRoche, R., Philadelphia, Pa. (12). [Sept. 14, 1875.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died in 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8). Born Sept. 8, 1830. Died June
Lincklaen, Ledyard, Cazenovia, N. Y. (1). [27, 1862.
Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.

Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died June 22, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15).
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June 24, 1872.

Maack, G. A., Cambridge, Mass. (18).
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept. 16, 1871.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10).
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb. 27, 1859.
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
McMahon, Mathew, Albany, N. Y. (11).
M'Conihe, Isaac, Troy, N. Y. (5).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Died April 2, 1869, aged 76.
Mitchell, Wm. H., Florence, Ala. (17).
Morgan, Lewis H., Rochester, N. Y. (10). Born Nov. 21, 1818. Died Dec. 17, 1881.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8,
Munroe, William, Concord, Mass. (18). Died April 27, 1877. [1866.
Mussey, William H., Cincinnati, Ohio (30). Born Sept. 13, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.
Nicholson, Thomas, New Orleans, La. (21).

Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.

Norton, John P., New Haven, Conn. (1). Born in July, 1822. Died Sept. 5, 1852.

Noyes, J. O., New Orleans, La. (21).

Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.

Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.

Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.

Olmsted, Alexander F., New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.

Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.

Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.

Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.

Painter, Minshall, Lima, Pa. (7).

Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.

Parkman, Samuel, Boston, Mass. (1). Died Dec. 15, 1854, aged 88.

Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.

Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.

Perkins, George R., Utica, N. Y. (1). Born May 3, 1812. Died Aug. 22, 1876.

Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.

Perry, John B., Cambridge, Mass. (16). Died Oct. 3, 1872, aged 52.

Perry, M. C., New York, N. Y. (10).

Piggot, A. Snowden, Baltimore, Md. (10).

Plumb, Ovid, Salisbury, Conn. (9).

Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6, 1870.

Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.

Pourtalès, Louis François de, Cambridge, Mass. (1). Born in 1822. Died July 18, 1880.

Pruyn, John V. L., Albany, N. Y. (1). Born June 22, 1811. Died Nov. 21, 1877.

Pugh, Evan, Centre Co., Pa. (14).

Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.

Putnam, J. Duncan, Davenport, Iowa. (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.

Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.

Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.

Robb, James, Fredericton, N. B. (4).

Robinson, Coleman T., Buffalo, N. Y. (15).

Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.

Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.

Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.

Rogers, William Barton, Boston, Mass. (1). Born Dec. 7, 1804. Died May 30, 1882.

Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.

Schaeffer, Geo. C., Washington, D. C. (1). Died in 1878.

Schley, William, New York, N. Y. (28). Died in 1882.

Scott, Joseph, Dunham, C. E. (11). Died in 1865.

Senter, Harvey S., Aledo, Ill. (20). Died in 1875.

Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct. 10, 1872.

Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.

Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.

Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 22, 1864.

Skinner, John B., Buffalo, N. Y. (15). Died in 1871.

Slack, J. H., Philadelphia, Pa. (12).

Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec.

Smith, J. V., Cincinnati, Ohio (5). [26, 1880.

Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.

Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.

Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877.

Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March 14, 1866.

Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.

Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.

Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.

St. John, Joseph S., Albany, N. Y. (28). Died Nov. 23, 1882.

Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.

Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born Jan. 20, 1778. Died Oct. 3, 1853.
Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1). Died Nov. 9, 1853, aged 63.
Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.
Thompson, Alexander, Aurora, N. Y. (1).
Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan.
Thurber, Isaac, Providence, R. I. (9). [19, 1856.
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York, N. Y. (1). Born August 15, 1796. Died March 10, 1873.
Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.
Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.
Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.
Tyler, Edward R., New Haven, Conn. (1). Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).
Vaux, William S., Philadelphia, Pa. (1). Born May 19, 1811. Died May 5, 1882.

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.
Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.
Walsh, Benjamin D., Rock Island, Ill. (17).
Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.
Warren, Gouverneur Kemble, Newport, R. I. (12). Died Aug. 8, 1882, in his 64th year.
Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.

Watertown, Charles, Wakefield, Eng. (1).
Watkins, Samuel, Nashville, Tenn. (26).
Watson, J. Craig, Ann Arbor, Mich. (13). Born Jan. 28, 1838. Died Nov. 23, 1880.
Webster, Horace B., Albany, N. Y. (1). Died Dec. 8, 1848.
Webster, J. W., Cambridge, Mass. (1). Died Aug. 30, 1850, aged 57.
Webster, M. H., Albany, N. Y. (1).
Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.
Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.
Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.
Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.
Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.
Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.
White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.
Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.
Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.
Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.
Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.
Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.
Williamson, R. S., San Francisco, Cal. (12).
Wilson, W. C., Carlisle, Pa. (12).
Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.
Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.
Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.
Wright, John, Troy, N. Y. (1).
Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.
Young, Ira, Hanover, N. H. (1). Died Sept. 14, 1858, aged 57.

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ADDRESS

BY

PROFESSOR GEORGE J. BRUSH,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

A SKETCH OF THE PROGRESS OF AMERICAN MINERALOGY.

MR. PRESIDENT, AND FELLOW MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

THE change in the Constitution effected at our last meeting, extending the scope of the Association and dividing it into nine sections, each with a Vice President, whose duty it is to deliver an address to the section over which he presides, has relieved the retiring President from attempting a general review of the progress of science during the past year.

I turn, therefore, to a more special subject, and invite your attention this evening to a sketch of the progress of American Mineralogy, since the commencement of this century, with particular reference to the labors of some of the early workers in the science on this continent.

During the last quarter of the eighteenth century, while great activity existed and rapid advance was made in the study of chemistry and mineralogy in Europe, almost nothing was accomplished in this new country. It is true that students in other departments of science, especially members of the medical profession, in the cities of Philadelphia, New York and Boston, attempted to arouse an interest in mineralogy, believing that the diffusion of a knowledge of this science would be of the utmost importance in the material development of the country. There were, however, no text-

books to aid the inquirer. There were no collections of mineral
to stimulate the student. In the absence of these it was almo
impossible that an interest in this science should be fostered,
that a spirit of investigation should be awakened.

As the first distinct beginning of the science, I may mention
association formed in 1798 in the city of New York, which
sumed, as they expressed it, "the name and style of the Americ
Mineralogical Society[1]. It announced as its object "the investi
tion of the Mineral and Fossil bodies which compose the Fabric
the Globe, and more especially for the Natural and Chemical H
tory of the Minerals and Fossils of the United States." The
tinguished Dr. Samuel Latham Mitchill, who seems to have b
a man of universal genius, was at once its first president, its
brarian, and its cabinet-keeper. The committee of the soc
issued a circular, in which, while expressing themselves, "desi
of obtaining and diffusing by every means in their power a co
and extensive knowledge of the mineral treasures of their cou
they earnestly solicited their fellow-citizens to communicat
them on all mineralogical subjects, but especially on the fol
ing," viz.:

> "1. Concerning the stones suitable to be manufactured into *gun-*
> where are they found? and in what quantity? 2. Concerning
> *brimstone or sulphur* or the waters or minerals whence it may be extr
> 3. Concerning *salt-petre:* where (if at all) found native? or the
> which produce it in the United States? 4. Concerning mines and o
> *lead:* in what places? the situation? how wide the vein? and in
> kind of rock it is bedded."

This warlike demand seems to call more for the discovery
materials for national defence than for the advancement of sc
and besides being a commentary on the spirit of the times,
a rather humorous impression of their strangely inadequat
ception of the science of mineralogy, and its possible beari
practical life. But in justice to them I should add that it is
announced that "specimens of ores, metals, coals, spars, gy
crystals, petrifactions, stones, earths, slates, clays,
limestones, marbles, and every fossil substance that
discovered or fall in the way of a traveller, which can
light on the mineralogical history of America, will be

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[1] *The Medical Repository*, Vol. II, p. 114 (1799), edited by S. L. Mitchill an
Miller.

ined and analyzed without cost, sufficient pieces, with the owner's leave, being reserved for placing in the society's collection." I have quoted the circular almost verbatim to give you some idea of the genuine though crude longings for knowledge felt by our early mineralogists, and also of the generous spirit in which they worked.

A still more forcible picture of the ignorance of the time is given by the elder Professor Silliman in 1818:[2] "Notwithstanding the laudable efforts of a few gentlemen," he says, "to excite some taste for mineralogy, so little had been effected in forming collections, in kindling curiosity and diffusing information, that only fifteen years since (1803), it was a matter of extreme difficulty to obtain *among ourselves* even the *names* of the most common stones and minerals; and one might inquire earnestly and long before he could find any one to identify even *quartz*, *feldspar* or *hornblende* among the simple minerals, or *granite*, *porphyry* or *trap* among the rocks. We speak from experience, and well remember with what impatient, but almost despairing curiosity we eyed the bleak, naked ridges which impended over the valleys and plains that were the scenes of our youthful excursions. In vain did we doubt that the glittering spangles of mica, and the still more alluring brilliancy of pyrites, gave assurance of the existence of the precious metals in those substances, or that the cutting of glass by the garnet, and by quartz, proved that these minerals were the diamond; but if they were not precious metals, and if they were not diamonds, we in vain inquired of our companions, and even of our teachers, what they were."

Such, then, was the state of knowledge in mineralogy here at the commencement of the century. A few American minerals, collected by travellers from time to time, had before this been taken to Europe for identification, but among these were discovered only two minerals new to science. The Moravian missionaries found at St. Paul in Labrador, the beautiful species of feldspar called by Werner *labrador-stein*, which in more modern times we know under the name of *labradorite*. Klaproth, the most eminent analytical chemist of his time, discovered that the so-called fibrous barytes from Pennsylvania was the sulphate of the then newly discovered earth strontia.[3] He thus, for the first time, identified the mineral species *celestite* which was subsequently found in various localities in Europe.

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[2] Review of Cleaveland's Mineralogy, *American Journal of Science* (1), I, p. 36.

[3] *Beiträge zur chemischen Kentniss der Mineralkörper*, II, 92.

Although little had been accomplished in America previous t 1800, the first quarter of the new century was destined to sho great development here in the study of mineralogy. During th early years of this quarter several collections of European mine als were brought to this country by American gentlemen who ha availed themselves during a residence in Europe of the best oppo tunities for acquiring a knowledge of the science from the gre masters of the subject in Germany and France. About this tir also several colleges in the country had instituted chairs of che istry and mineralogy, and a commencement was thus made teaching these sciences in the higher schools. As the result these influences the number of persons interested in mineralc was largely increased, and an active search for minerals was in ated throughout all of the older United States and to a consic able extent also in Canada.

So energetically were these explorations followed up tha 1825 a Catalogue[4] of American minerals was published by Samuel Robinson, with their localities arranged geographic and giving only such as were known to exist in the United St and the British Provinces. It formed an octavo volume of three hundred pages.

That much credit was due to *many* workers during this pe both in the field and in the laboratory, there can be no ques but among them all I find four men standing forth so promin as leaders, that I have thought it would be well for us to briefly something of the character of these men and their l for the advancement of mineralogy in this country.

First among these I will mention Dr. **Archibald Bruce**.[5] was the son of Dr. William Bruce, a surgeon in the British and was born in New York in 1777. He was graduated at C bia College; subsequently studied medicine, and in 1798 w Edinburgh where, in 1800, he obtained his doctor's degre that university. He was early interested in natural scien while still in college found, his biographer says, "the col and examination of minerals — a pursuit not then at all at to in this country — was his particular relief from other studies; for even during his recreation he was ever on the lookout for something new or instructing in mineralogy."

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[4] *Catalogue of American Mineral Localities*, by Dr. Samuel Robinson. 8v Boston, 1825.

[5] For a biographical notice of Dr. Bruce see *American Journal of Science*, 209, 1818.

When he went to Europe he took with him a large number of American minerals, and through exchanges with institutions and prominent mineralogists abroad, he established friendly relations with those most interested in his favorite science. After the completion of his medical studies, he travelled for two years on the continent of Europe, making the acquaintance of the Abbé Haüy, and other eminent mineralogists, and collecting an extensive cabinet of valuable minerals, which on his return to this country in 1803, he brought with him to New York. This collection, with another brought to New York about the same time by Mr. B. D. Perkins — both being made fully accessible to all interested in seeing them — contributed, it was said, more than any agencies had ever done before, to excite in the public an active interest in the science of mineralogy. Besides this Dr. Bruce entered into extensive correspondence with others interested in the subject, was active in visiting and discovering new mineral localities, and in advising, encouraging and inspiring young mineralogists. Finally, after well considering the matter, he established the first purely scientific periodical ever published in America. This was called the *American Mineralogical Journal*,[6] and the first number of it was published in 1810. It contained original contributions, chiefly on mineralogy, from a number of investigators. "It was received," says the elder Silliman, "in this country and in Europe in a flattering manner; it excited at *home* great zeal and effort in support of the sciences which it fostered, and *abroad* it was hailed as the harbinger of our future exertions." But, alas! it was in advance of the age, and, after struggling for several years, was given up on the publication of the fourth number. Possibly it would have continued longer had it not been for the failing health of its founder.

This Journal contained several important papers by Dr. Bruce: among them, the investigation and description of two new mineral species, the *native magnesia* of Hoboken and the *red zinc oxide* o Sussex Co., New Jersey. These are the first American species described by an American mineralogist. So thoroughly was the work done by Bruce, that these species remain to-day essentially as he described them, and his papers may well be studied by mineralogists now as models of accuracy and clearness of statement

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[6] The *American Mineralogical Journal*, being a collection of facts and observations tending to elucidate the mineralogy and geology of the United States of America; together with other information relating to mineralogy, geology and chemistry derived from scientific sources. Conducted by ARCHIBALD BRUCE, M.D., Professor of Materia Medica in the Medical Institution of New York, and Queen's College (N. J.). Vol. 1, 270 pp. New York. 1814.

Dr. Bruce did much also for the elevation of the medical profession, was one of the founders of the New York Medical Society and was largely influential in obtaining the charter of the Colleg of Physicians and Surgeons, in which he was subsequently th Professor of Materia Medica and Mineralogy.

He is described as a successful teacher, a man of wide a quirements and of great integrity, which qualities, with h abounding generosity and hospitality, commanded the respe and regard of all who knew him. It was a great loss to scien and to his country that so able an investigator should ha been cut off at the early age of 42. He died in New Yo Feb. 24, 1818.

I have mentioned that the importation and exhibition of coll tions of minerals from Europe had contributed much to exc an interest in the study of mineralogy. It was necessary to h known minerals for study and comparison in order properly to termine those obtained by exploration here. In 1805, Colo GEORGE GIBBS of Rhode Island, for many years a resident in rope, returned from his travels with a collection of minerals most extensive and valuable ever brought to America. Colc Gibbs was a zealous cultivator of mineralogy and, fortunately science, a young man of wealth. He used his money freely the purchase of whole cabinets, as well as in personal explorati in search for minerals.

The larger part of his collection was made by the purchas two famous European cabinets, one from the heirs of Gigot d'O a noted French collector, and said to have been the result of f years' labor, the other from Count Gregoire de Razamowsk Russian nobleman, long resident in Switzerland. D'Orcy's inet numbered over 4,000 specimens, chiefly from France, many, Italy and Great Britain; Razamowsky's contained a 6,000 specimens from the Russian Empire and the remainder cipally from Germany and Switzerland; in all, with the othe lections made by Colonel Gibbs, it is said that more than 20,000 specimens were brought by him to this country.[7]

In 1807, a portion of this collection was opened in Newport, and many interested in mineralogy made pilgrimages there, to view the treasures it contained. Among others was Professor Silliman, who states in his diary, that he spent many weeks in studying the minerals with Colonel Gibbs, finding in the latter "a scientific friend and a professional instructor and guide." That Colonel Gibbs reciprocated Professor Silliman's feelings of friendship there can be no doubt; for, after various offers to deposit his collection for exhibition in Boston, New York and elsewhere, to the great surprise of Professor Silliman, he proposed to open the cabinet at Yale College, provided rooms should be fitted up for its reception.

The proposition was promptly responded to by the authorities of the college, and in 1810, 1811 and 1812, under the personal supervision of Colonel Gibbs, it was opened and arranged, and generously placed at the disposition of the institution and the public. The opening of this collection in New Haven formed an important epoch in the history of the college, and gave a powerful impetus to science throughout the country. It was not only studied by the pupils of the college, but it was visited by travellers from all parts of the United States.

In 1825, the collection had for fifteen years been exhibited without any advantage to the owner, other than the satisfaction of observing the great amount of good which was effected by the knowledge it disseminated, and the enthusiasm with which it inspired students. Colonel Gibbs then offered the whole for sale, giving the college the preference as purchaser. Fortunately and mainly through the influence of Professor Silliman, the institution succeeded in raising the funds ($20,000) necessary for its purchase, and the ownership of this collection has exercised a most important influence in the development of natural science at New Haven.

Colonel Gibbs, however, did not confine himself to the collection of minerals in Europe. On his return to this country he made extensive journeys and opened up new mineral localities, giving his time and specimens freely to aid others who were interested in this special study. At Yale, as an incentive to students, he for many years offered prizes for superiority of attainments in mineralogical knowledge and for services rendered to th science by useful discoveries and observations.[8]

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[7] *Medical Repository* (2), V, 213 (1808).

He published valuable papers both in the *American Mineralogical Journal* and the *American Journal of Science*, and did muc by his counsel and coöperation to support these publications. Indeed, it was from Colonel Gibbs that Professor Silliman first r ceived the suggestion that he should institute a new Journal Science, in order that the advantages already gained by the sho lived Mineralogical Journal might be secured and further progre for science might be made.[9]

In every way Colonel Gibbs proved himself a liberal patron science, and it was most fortunate for the promotion of mineralo in this country that he should so unselfishly have devoted wealth and his personal influence to its advancement. He d August 5, 1833, aged 57.

Much as had been accomplished by the free exhibition of cabin and the explorations and investigations of enthusiastic work in mineralogy during the years from 1805 to 1815, a great dr back was now felt to the progress of the science from the wan text-books. Most of the literature of the subject was in Ger and French, but the works of the French and German authors not then been translated and consequently were accessible onl the few who were acquainted with these languages.

In English there were not many treatises on the subject. by Richard Kirwan, the eminent Irish mineralogist of the century, was a renowned work in its day, but, as the last ed of it had been published in 1794, it was already too old to b much service to the student. Jameson's treatise was some more recent (1804),[10] but its great fulness and exclusive dev to the Wernerian system made it an undesirable book for begin aside from the fact that its price was such that few studen those days could afford to buy it. So much progress had made at home and abroad, that a work was needed here should include the modern discoveries, and one also which sl gather up the scattered facts already published in regard to American minerals.[11]

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[8] *American Mineralogical Journal*, p. 269.

[9] Obituary notice of Colonel Gibbs, by Professor Silliman, *American Journal ence* (1), XXV, 214.

[10] Of Jameson's *Mineralogy*, 1st edition, the 1st volume was published in 1 volume in 1805, 3d volume in 1808.

Fortunately for the further progress of science in this country this was done by Professor Parker Cleaveland. His work was published in 1816, and was entitled "An Elementary Treatise on Mineralogy and Geology."[12]

Professor Cleaveland was professor of mathematics and natural philosophy in Bowdoin College, and, like many other professors of science in the early history of American colleges, was charged by the trustees to lecture also on mineralogy and chemistry. He was an enthusiastic student of mineralogy, was well acquainted with the literature of the science in various languages, had been a successful teacher of the subject for many years, and withal was both an explorer and investigator, and held intimate relations with the leading mineralogists of the day. The work was modelled on the general plan of Brongniart, combining the excellences of both the French and German schools, and gave in detail almost everything then known in regard to American minerals. It supplied the pressing need for a thorough, systematic and *American* treatise on mineralogy, well suited to all classes of students, and it was written in such a masterly style that it won for its author the highest praise from the leading mineralogists of the world. "It brought," says Professor Silliman, "within the reach of the American student the excellences of Kirwan, Jameson, Haüy, Brochant, Brongniart and Werner, and we are not ashamed," he says, "to have this work compared with those of these celebrated authors."[13] His biographer[14] states that "he received letters of respect and congratulation from Sir David Brewster, Sir Humphrey Davy and Dr. Macculloch, in England, from Berzelius, in Stockholm, Germar of Halle, from Brongniart, Baron Cuvier and the Abbé Haüy, in Paris."

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[11] The *Manual of Mineralogy*, by A. Aikin, was reprinted in Philadelphia in 1815, but it contained nothing in regard to American minerals.

[12] An elementary treatise on Mineralogy and Geology, being an introduction to the study of these sciences, and designed for the use of pupils,—for persons attending lectures on the subjects,—and as a companion for travellers in the United States of America. By Parker Cleaveland, Professor of Mathematics and Natural Philosophy, and Lecturer on Chemistry and Mineralogy in Bowdoin College. One volume, 668 pp. 8vo, with six plates. Boston, 1816.

[13] Review of Cleaveland's Mineralogy, loc. cit.

[14] The main facts as stated in regard to Professor Cleaveland are from an address on his life and character by Leonard Woods, D. D., President of Bowdoin College. Brunswick, 1860.

The work at once took rank as one of the leading author on the science and was introduced as a class-book in the prin schools and colleges in America. The first edition was soon hausted and a new and revised edition with more than a hun pages of new matter was published in 1822. The demand w great that this likewise was soon out of print and a third ed was called for by the public, but Professor Cleaveland had a this time become so engrossed in the administration of the a of the new Medical School at Brunswick that he was unable t spond to the call, having turned his thoughts and efforts in directions.

Unfortunately for the science of mineralogy, in which he obtained such eminence as an author and teacher, he no l contributed actively to its progress, although he continue work as lecturer on the science so long as he lived. He died 16, 1859, in the seventy-ninth year of his age.

The last to be mentioned of these early leaders is Pro BENJAMIN SILLIMAN. His name is so intimately associated the progress of science on this continent during the first h the present century, that his life work is more or less famil all[15]. But the important service he rendered in the early hist mineralogy deserves especial recognition here, not only f work he himself did in the laboratory and the field, but be his enthusiasm and zeal were a constant inspiration to other

Commencing with the historic "candle-box" of unlabelled which he took to Dr. Adam Seybert, of Philadelphia, to be n he began with enthusiasm the acquisition of knowledge an gathering of material to illustrate the mineral kingdom. I a residence in England and Scotland in 1805–6 he had oppo ties to add to his information, and collect many specimens, from the mines of Derbyshire and Cornwall. On his ret America he at once applied the knowledge he had acqui making an exploration of the mineral structure of the envir New Haven, and read a paper on this subject to the Conn Academy of Arts and Sciences in September, 1806.

In the following year he induced the corporation of the to purchase the mineral collection of Mr. B. D. Perkins of New York (already referred to), for one thousand dollars, thus placing the institution in possession of means for illustrating the science of mineralogy far in advance of anything it had before enjoyed.

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[15] See Life of BENJAMIN SILLIMAN, M. D., LL. D., Late Professor of Ch Mineralogy and Geology in Yale College. Chiefly from his manuscript remini diaries, and correspondence, by GEO. P. FISHER, Professor in Yale College. umes, 12 mo. New York, 1866.

The occurrence of the fall of the Weston Meteorite in December, 1807, offered an opportunity for Professor Silliman to undertake, in connection with his colleague, Professor Kingsley, an investigation into the circumstances of the phenomena, and the character of the stones which fell at that time. The results of this investigation were presented to the American Philosophical Society and published in the *American Philosophical Transactions* in 1809. The diligence employed in obtaining all the facts possible from eyewitnesses of the occurrence, and the care and skill shown in the chemical and mineralogical examination of the meteorite, made this paper one of the most remarkable memoirs of the time, and attracted the attention of philosophers throughout the world.

As already stated, it was the personal enthusiasm and magnetic influence of Professor Silliman which led Colonel Gibbs to deposit his great cabinet of minerals in New Haven, under the care of his friend. It was due to the same qualities in Professor Silliman that the college secured the permanent possession of this invaluable collection, which probably has done more than any other single agency to create an interest in and disseminate a knowledge of mineralogy in this country.

The establishment of the *American Journal of Science* in 1818, now everywhere recognized as of inestimable value to all departments of science, was peculiarly helpful to mineralogy, and the early volumes are rich in articles on this subject. Professor Silliman's original contributions to science were more in chemistry and geology, but he also is the author of several important papers on mineralogy, and was the discoverer of the occurrence of native tungstic acid as a mineral species. For more than fifty years he continued as a teacher in Yale College, and when he resigned his professorship in 1853, he had the satisfaction to have as his successor in the department of mineralogy and geology Professor James D. Dana, who was already among the foremost mineralogists of the day, and whose published works, before and since his accession to this professorship, have done so much for the advancement of mineralogy.

Professor Silliman retired from his active labors in his seventy-fourth year, still in full possession of his remarkable physical and mental powers, and lived honored and revered until Nov. 24, 1864, when he passed to his rest.

It will be inferred from what has been said of these pioneers that the developments and discoveries of minerals, during the first twenty-five years of the century, were due entirely to individual enthusiasm and private enterprise. Up to this time no aid had been received from either state or national governments, and in looking over the work accomplished during this period we are fille
with wonder and admiration at the energy and rare devotion t
science exhibited. The larger portion of the continent was an un
broken wilderness, and the facilities of communication even in th
settled parts of the country were of the most primitive characte
Yet at the present day, with our means of rapid transportatio
many naturalists would hesitate to undertake the long journe
then made for purely scientific purposes.

Geologists as well as mineralogists will recall how much science
indebted to such men as William Maclure, James Pierce, Tho
Nuttall (the botanist), and others who made extensive tr
through the whole territory east and in some instances to the w
of the Mississippi River. Maclure not only devoted his time
money to making and publishing a geological survey of the Uni
States and Canada, the first report of which was made in 180
but to him the Academy of Natural Sciences, in Philadelphia, o
its first endowment.[17]

I shall be pardoned, I trust, if I mention still another signa
stance of private liberality in this connection. Gen. Stephen
Rensselær, of New York, a generous patron of science, defr
all the expenses of a geological survey of the country adjace
the Erie Canal, including the making of a geological section
Lake Erie to the eastern coast of Massachusetts.[18] This su

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[16] Published later with additions, in a volume of 127 pp., 8vo, with two plates.
adelphia, 1817.

[17] Memoir of William Maclure, Esq., late President of the Academy of Natu
ences of Philadelphia, by S. G. Morton, M. D. *Am. J. Sci.*, (1) XLVII, 1 (1844

[18] A Geological and Agricultural Survey of the district adjoining the Erie Cana
State of New York, taken under the direction of Hon. Stephen Van Rensselær
I. containing a description of the rock formations, together with a profile ex
from the Atlantic to Lake Erie. 163 pp. 8vo. Albany, 1824.

was under the charge of Professor Amos Eaton, with a competent corps of assistants, and was continued for four years, from 1820 to 1824, at a cost of many thousands of dollars. General Van Rensselaer was also the founder of the first school of technical science in this country—the Rensselaer Polytechnic Institute, at Troy, which was placed under the charge of Professor Eaton. It may be interesting here, in these days of summer schools, to recall, although parenthetically, that what was probably the first summer school of science in the United States was established more than fifty years ago in connection with this institution. The school consisted of a flotilla of towed canal boats, and the route was from Troy to Lake Erie. It took two months for the trip and all important points on the way were visited. Instruction by lectures and examinations was given in mineralogy, geology, botany, zoology, chemistry, experimental philosophy and practical mathematics, particularly land surveying, harbor surveying and engineering. One of the largest boats in the flotilla was fitted up as a laboratory, with cabinets in mineralogy and geology and also scientific books for reference. Students were taught the method of procuring specimens and were required to make collections of whatever was interesting on the route.[19]

The public mind was finally awakened to the importance of the work which these explorers and investigators had carried on single handed. Government now came to the aid of Science. In 1824, one State legislature, that of North Carolina, authorized a geological survey to be made. This example was followed in 1830 by Massachusetts and soon after by New York, Pennsylvania, Virginia and other states, and also by the national government, until as is now well known, the whole territory of the United States and Canada, either has been or is in the process of being surveyed. Several of the state surveys published independent volumes on the mineralogy of their respective states, and these surveys have been a powerful auxiliary in extending our knowledge of the occurrence of minerals on this continent. The opening of mines and quarries throughout the country has also furnished abundant material for study. The large number of original contributions which have been published in the volumes of State Surveys, the treatises by American authors, and the still larger number of memoirs and papers communicated to our academies of science and scientific journals cannot be even enumerated in this place, neither is it my purpose to attempt to give here a list of the names of those who have been actively engaged in making researches on American minerals.[20] Still less can I attempt to give an account of the work that has been and is being done by living mineralogists. The sketch which I have presented of the four typical workers has in a measure shown the character of our early mineralogists, the earnest spirit in which they labored, and what they accomplished in the first quarter of the century. The point to which the science has reached in the last quarter of the century cannot be unfamiliar to you all.

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[19] *American Journal of Science* (1), XVIII, 200. (1830.)

In the time that remains I desire to call your attention to some of the developments made in the field in which our mineralogists have worked. It was thought by many scientists in the first half of this century that our rocks seemed likely to afford less variety of mineral contents than the rocks of Europe. Further study, however, and more careful and extended observations encourage us to believe that our mineral riches, even in variety of species, will compare favorably with those of other continents. Already fully one-half of the known mineral species have been found here. The present number of known minerals is variously estimated to be from seven hundred to one thousand. There have been described, as occurring here, nearly three hundred supposed new American minerals. Of these, perhaps one-quarter are new to science and the remainder have either been proved to be identical with species already described, or their characters are so imperfectly given that further investigation is needed to ascertain what they are.

Among these new minerals are some of great interest to science. Time, however, will not allow, even if your patience would permit me, to give the facts in detail; but in justice to the describers of those announced to be new, I will print, as an appendix to this address, as complete a list as I have been able to make of the names of the proposed new American mineral species, with the names of their sponsors. The list will, I trust, be instructive both as a warning and an encouragement to investigators. The ambition to make new species is recognized as a drawback in every department of science, and mineralogy has probably not suffered in this respect more than other sciences. Nor do I believe that American mineralogists have, as a class, been less careful in describing new species than their European confrères. There have been flagrant examples of carelessness in both hemispheres, and the growing tendency during the last ten or fifteen years to call things new which have been only imperfectly investigated cannot be too strongly censured. "If two very simple rules," says a recent writer,[21] "could be conscientiously followed by those investigating supposed new mineral species, the science of mineralogy would be vastly benefited. These are: first, that the material analyzed should in every case be proved by a careful microscopical and chemical examination to be *homogeneous;* and second, that the thorough investigation which is to establish the position of a "new species" should precede, not follow, the giving of a new name. A mineral which can only be partially described does not deserve a name.

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[20] For a list of papers by American chemists, including a large proportion of papers published in mineralogy, see *American Contributions to Chemistry*, an address delivered on the occasion of the celebration of the Centennial of Chemistry at Northumberland, Pa., August 1, 1874, by BENJAMIN SILLIMAN. 175 pp. 8vo. Reprinted from the *American Chemist* for August, September and December, 1874.

In comparing the minerals found in America with those of Europe, although interesting minor variations are observed, it can hardly be expected that very marked differences should exist. This is, of course, due to the fact, that in the inorganic kingdom, nature has everywhere to do with the same elements, under essentially like conditions. A large number of remarkable analogies between the minerals of the two continents will occur to any one familiar with the subject, as, for example, the character of the occurrence of individual minerals in the rocks of the northeastern United States and Canada as compared with those of Norway and Sweden, and numberless instances of like association of minerals in various parts of Europe find their counterparts here.

A marked feature of American minerals is the grand scale upon which crystallization has taken place, individual crystals of large size being very common. The granite veins of New England afford striking examples of this kind. We have common mica, in sheets a yard across; feldspar has been observed where a single cleavage plane measured ten feet; gigantic hexagonal prisms of beryl, four feet long and more than two feet in diameter, and weighing over two tons, have been described; spodumene crystals six to seven feet in length and a foot or more across, and masses of rock crystal of immense size, have been found. Canada and New York have given crystals of apatite, phlogopite and sphene which for these species are of marvellous grandeur in dimensions. Many other American localities might be mentioned where giant crystals occur. While it is true that these are extraordinary instances, it is also true as a general fact, common to a very large proportion of the minerals found in this country, that the species occur in much larger crystals than those obtained from European localities.

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[21] Preface to the Third Appendix to the fifth edition of Dana's *Mineralogy*. New York. 1882.

Another point worthy of note is the occurrence in comparatively
large quantities, and over wide areas, of some of the rarer elements as constituents of the minerals found. In illustration of
this we have, among the rare earths, *glucina* combined with silica
and alumina in the mineral *beryl*, occurring in large quantity and
perhaps in a hundred or more places; *zirconia*, in the minera
zircon, is also very widespread in its range of occurrence as a
original constituent of the older rocks, as well as a vein mineral
localities are known which have furnished this rare species by th
hundred weight. The *cerium* earths are found largely in the mi
eral *allanite*, which occurs in so many places that it may be said
be a common mineral in the United States. These earths are al
found in the rare phosphate *monazite*, a mineral that in Ameri
has a wide range of localities, and recently this species has be
found in crystals of two, three, and, in one instance, of eig
pounds in weight. Again, three new earth metals, *mosandru*
phillipium and *decipium* have been described as occurring with t
cerium earths and yttria in the North Carolina *samarskite*. T
rare alkali metal *lithium*, sometimes associated with the still ra
metals *rubidium* and *cæsium*, is found not only of widespread
currence in our lithia micas, but the mineral *spodumene*, conta
ing from five to eight per cent of lithia, occurs by the ton in
least one locality and must be looked upon as one of the comm
American minerals, being found in the granite veins in Ma
New Hampshire, Massachusetts and Connecticut, and as far so
as North Carolina and Georgia. Lithia also is one of the
stituents of the phosphate *triphilite*, and there are several lo
ties known where this mineral occurs abundantly. **Again,**

have the frequent occurrence of some of the rare metals which form metallic acids: *Columbium*, the first metal, new to science, discovered in America, associated with its twin metal *tantalum*, is found in columbite in our granite veins from Maine to Georgia, a range of more than a thousand miles, in a score or more of places, and sometimes is obtained by the hundred weight at a single locality. The American variety of *samarskite*, another rare columbate, has also been found in masses of fifty pounds or more in weight, and these acids occur in still other American species. *Molybdenum*, both as sulphide and in the oxidized form as native molybdic acid and molybdate of lead, is found in many localities, and occasionally in large quantity. Quite recently *vanadium* compounds have been discovered in several places, and *tungstates* have also been observed over a wide range of country. *Titanium* has been found in enormous quantities in extensive deposits of titanic iron as well as in the form of rutile and in sphene. The rare metal *tellurium* occurs native in Colorado in one locality where single masses of twenty-five pounds in weight have been taken out, and several new tellurium compounds have been found in our western mines.

It is, perhaps, unnecessary to enumerate more fully the many occurrences of other rare elements in American minerals. Enough has already been said to show that important developments have been made in the discovery and investigation of the minerals found in our American rocks during the past eighty years. Nevertheless it is but a commencement in the work. Only a very small portion of our territory has been explored with any thoroughness, and none of it exhaustively. The enormous production of the precious metals, and the extensive deposits of ores of the more common metals which have been opened up during the past twenty or thirty years, have placed us in the front rank as metal producers, but we are still far behind Europe in the variety of minerals obtained from our mines. This may be due, in some instances, to the character of the veins or ore deposits, there being, as in many of our gold and silver mines, remarkably few associated minerals. In other cases, however, it is doubtless due to the fact that very few persons connected with our mines have even an elementary knowledge of the rudiments of mineralogy, while in continental Europe almost every mining officer is familiar with all the ordinary minerals. Thanks to the training of our schools of science, an improvement in this respect is already noticeable, as is shown in the discoveries made in the mines of our Western States and Territories during the past few years.

While the service done for mineralogy by our geological surveys is gratefully acknowledged, we feel we have a right to demand much more from them in the future. Mineralogy has been too largely looked upon as a guide to the discovery of useful ores and minerals and not as a matter for scientific study; fortunately during the past decade the discoveries in optical mineralogy, and their importance in the determination of the constituent minerals of the crystalline rocks, have led many geologists to again recognize the desirability of a knowledge of our science. Much will be accomplished if those in charge of geological surveys will direct competent persons to make observations, not only on the main mineral constituents of rocks but also on the manner of occurrence of individual minerals. The careful inspection of quarries and mines is greatly to be desired. These are rich sources for minerals; but unless constant watchfulness is exercised, valuable material for science is in danger of being buried out of sight.

It is too true that many of the most interesting discoveries already recorded seem to have been due more to the result of fortunate accident than of systematic and intelligent exploration. If our trained mineralogists, instead of devoting most of their attention to the examination of specimens in cabinets collected by others, would give more time to personal observation in the field in the study of the order and manner of occurrence of mineral species in place, our knowledge would doubtless be greatly promoted. Again, if our wealthy amateurs could be induced to spend their money as freely in the exploration of promising American localities as in the importation of costly European specimens, we might hope for many important discoveries, and they could have the satisfaction not only of gaining novelties for their collections, but incidentally they would do much to foster science.

In order to keep pace with the progress of the science, we need many more workers who will devote themselves especially to mineralogical research, and we need more of the spirit of the early workers. It is my belief that the number of persons at present interested in the study here, either as amateurs or investigators, is relatively less than in 1825. The mineralogy of to-day is a very different subject from the mineralogy of the commencement of the period over which we have so hastily glanced. Then the study of minerals was confined almost exclusively to their external characters. Led by Werner[22] and reinforced by his most gifted pupil Mohs, the majority of mineralogists claimed mineralogy to be a purely natural history science. They gave their attention, as has been well said, entirely to "*how the mineral looked*," and not at all to "*what it was*." On the other hand the development of analytical chemistry by the labors of Klaproth and Berzelius led many to take up mineralogy from a purely chemical standpoint. These two schools working independently brought great confusion into the science. The discoveries of Haüy in crystallography, and especially his labors in establishing a mathematical foundation for the geometrical form of crystals, and the recognition that the constancy of form depended on the constancy of the "integrant molecule," were steps which paved the way for modern mineralogy. In this a union of all the physical, geometrical and chemical properties is required in order to determine the true character of a mineral.

Further, we are called upon to investigate the history of its origin, its relation to associated species, the changes which it undergoes, and the causes and results of those changes. Here we have to do largely with both geology and chemistry. From this it becomes evident that a much broader foundation is now required for the mineralogist than in the early days of the century. The bearing of physics, geology and chemistry in the study of the mineral kingdom must be thoroughly recognized and appreciated by every investigator who desires to contribute to further progress. No mineralogist can expect to have a profound knowledge in all these directions, but he must be at least capable of intelligently applying to his subject the results obtained by experts in these sciences. Mineralogy is deeply indebted to special investigators in all these departments. Without their coöperation it would have been impossible to discover the relations of form and other physical characters with that fundamental arrangement of molecules whose nature it is now admitted controls all the properties of a substance.

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[22] Werner, although using physical characters in the main, and entirely in the determination of minerals, recognized chemistry in arranging minerals in families or groups, but Mohs, Jameson and others founded their systems "totally independent of any aid from chemistry." (Preface, Jameson's *Mineralogy*, 3d ed., Vol. I, p. iii.)

The study of natural crystals has yielded rich material for the physicist. In the department of optics it has given results from which many fundamental laws have been deduced; and natural crystals, too, have furnished, in many cases, the very apparatus which made investigations possible. Some chemists claim that mineralogy is not at all a science by itself, and constitutes only a small part of inorganic chemistry. It can be unquestionably conceded that a knowledge of chemistry is fundamental, and in consequence this claim has a certain plausibility. On the other hand, we contend that it was largely the labors of the mineralogists on the physical characters of minerals, and especially their demonstration of the relation of form to chemical composition which finally awakened chemists to a more profound study of their own subject. The law of isomorphism was discovered by a chemist, whose training as an expert crystallographer in the examination of natural crystals made it possible for him to recognize the wonderful relation of form to composition. Dimorphism was first established from observations made on minerals, and it is in the study of the mineral kingdom that the laws of isomorphism and dimorphism find abundant demonstration. From the further investigation of the chemical nature of minerals we may hope for new light on the molecular constitution of substances which as yet the chemist has been unable to reproduce. We have already indicated the interdependence of geology and mineralogy. May we not claim the same interdependence of mineralogy, physics and chemistry, letting each go on in its own sphere, contributing to the general progress, sure that every new fact observed and every new law discovered will be for the common advancement of all?

APPENDIX.

A List[23] *of Names given to Minerals from the United States and Canada, for the most part described as New Species.*

Acadialite (= Acadiolite, *T. Thomson*)	*F. Alger* and *C. T. Jackson.*
Adamsite	*C. U. Shepard.*
Aglaite	*A. A. Julien.*
Alaskaite	*G. A. König.*
Albertite	*C. Robb.*
Algerite	*T. S. Hunt.*
Ambrosine	*C. U. Shepard.*
Amesite	*C. U. Shepard.*
Animikite	*H. Wurtz.*
Annite	*J. D. Dana.*
Anthophyllite, hydrous	*T. Thomson.*
Apatoid (*m*)[24]	*C. U. Shepard.*
Aquacreptite	*C. U. Shepard.*
Aragotite	*F. E. Durand.*
Arkansite	*C. U. Shepard.*

Baltimorite	*T. Thomson.*
Barnhardite	*F. A. Genth.*
Barytocelestite (= baryto sulphate of strontia	*T. Thomson.*
Beaumontite	*A. Lévy.*
Begeerite	*G. A. König.*
Bernhardinite	*J. M. Stillman.*
Bismuthaurite	*C. U. Shepard.*
Blackmorite	*A. C. Peale.*
Boltonite	*C. U. Shepard.*
Bowenite	*J. D. Dana.*
Brucite (= Native Magnesia, *Bruce*)	*F. S. Beudant.*

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[23] This list is compiled almost entirely from the *Fifth edition of Dana's Mineralogy, and Appendices I-III to the same work*, in which will be found references as to where these minerals were originally described, as also any further facts published in regard to them, and their present position in the science.

[24] (*m*) signifies that the substance was found in a meteorite.

Brucite (Chondrodite)	*G. Gibbs.*
Byerite	*J. W. Mallet.*
Bytownite	*T. Thomson.*
Calaverite	*F. A. Genth.*
Calcimangite	*C. U. Shepard.*
Calcozincite	*C. U. Shepard.*
Calstronbarite	*C. U. Shepard.*
Calyptolite	*C. U. Shepard.*
Campbellite (*m*)	*St. Meunier.*
Canaanite	*S. L. Dana.*
Cantonite	*N. A. Pratt.*
Carrollite	*W. L. Faber.*
Cassinite	*I. Lea.*
Catlinite	*C. T. Jackson.*
Celestialite (*m*)	*J. L. Smith.*
Celestite (= schwefelsaurer Strontianit aus Pennsylvanien, *Klaproth*, 1797)	*A. G. Werner.*
Centrallasite	*H. How.*
Cerinite	*H. How.*
Chalcodite	*C. U. Shepard.*
Chalcophanite	*G. E. Moore.*
Chathamite	*C. U. Shepard.*
Chelmsfordite	*J. F.* and *S. L. Dana.*
Cherokine	*C. U. Shepard.*
Chesterlite	*T. F. Seal.*
Chiltonite	*E. Emmons.*
Chladnite (*m*)	*C. U. Shepard.*
Chlorastrolite	*C. T. Jackson* and *J. D. Whitney.*
Chlorophyllite	*C. T. Jackson.*
Chromchlorite	*R. Hermann.*
Chrysophane (= Seybertite)	*A. Breithaupt.*
Cleavelandite	*H. J. Brooke.*
Cleiophane	*T. Nuttall.*
Clingmanite	*B. Silliman, Jr.*
Clinochlore	*W. P. Blake.*
Clintonite (= Seybertite)	*W. W. Mather.*
Coloradoite	*F. A. Genth.*
Columbite (= ore of Columbian, *C. Hatchett*, 1802)	*R. Jameson.*
Cookeite	*G. J. Brush.*
Copperasine	*C. U. Shepard.*
Coracite	*J. L. LeConte.*
Corundellite	*B. Silliman, Jr.*
Corundophilite	*C. U. Shepard.*
Cryophyllite	*J. P. Cooke.*
Cryptomorphite	*H. How.*

Culsageeite	*J. P. Cooke.*
Cummingtonite (= Hornblende)	*C. Dewey.*
Cummingtonite (= Rhodonite)	*C. F. Rammelsberg.*
Cuproscheelite	*J. D. Whitney.*
Cyanolite	*H. How.*
Cymatolite (= Cumatolite)	*C. U. Shepard.*
Cyrtolite	*W. J. Knowlton.*

Danaite	*A. A. Hayes.*
Danalite	*J. P. Cooke.*
Danburite	*C. U. Shepard.*
Daubréelite (m)	*J. L. Smith.*
Dawsonite	*B. J. Harrington.*
Delawarite	*I. Lea.*
Deweylite	*E. Emmons.*
Diabantite	*G. W. Hawes.*
Dickinsonite	*G. J. Brush* and *E. S. Dana.*
Dimagnetite	*C. U. Shepard.*
Ducktownite	*C. U. Shepard.*
Dudleyite	*F. A. Genth.*
Dysyntribite	*C. U. Shepard.*
Dysluite	*W. H. Keating.*
Dyssnite	*F. v. Kobell.*

Edenite	*E. F. Glocker.*
Edwardsite	*C. U. Shepard.*
Emerald Nickel	*B. Silliman, Jr.*
Emerite	*C. U. Shepard.*
Emmonite	*T. Thomson.*
Enceladite (= Warwickite, *Shepard*)	*T. S. Hunt.*
Eosphorite	*G. J. Brush* and *E. S. Dana.*
Eremite	*C. U. Shepard.*
Erilite	*H. C. Lewis.*
Erusibite	*C. U. Shepard.*
Erythrite	*T. Thomson.*
Euchlorite	*C. U. Shepard.*
Eucryptite	*G. J. Brush* and *E. S. Dana.*
Eumanite	*C. U. Shepard.*
Euphyllite	*B. Silliman, Jr.*
Eupyrchroite	*E. Emmons.*

Fairfieldite	*G. J. Brush* and *E. S. Dana.*
Ferroilmenite	*R. Hermann.*
Ferrotellurite	*F. A. Genth.*
Ferrotitanite (= Schorlomite)	*J. D. Whitney.*
Fillowite	*G. J. Brush* and *E. S. Dana.*
Fowlerite	*C. U. Shepard.*
Franklinite	*P. Berthier.*

Genthite (= Nickel-gymnite, *Genth*)	*J. D. Dana.*
Gibbsite	*J. Torrey.*
Glossecollite	*C. U. Shepard.*
Goshenite	*C. U. Shepard.*
Grahamite	*H. Wurtz.*
Gunnisonite	*F. W. Clarke* and *N. W. Perry*
Gymnite	*T. Thomson.*
Haddamite	*C. U. Shepard.*
Hallite	*A. R. Leeds.*
Harrisite	*C. U. Shepard.*
Hatchettolite	*J. L. Smith.*
Haydenite	*P. Cleaveland.*
Hebronite	*F. v. Kobell.*
Henryite	*F. M. Endlich.*
Hermannite	*A. Kenngott.*
Hermannolite	*C. U. Shepard.*
Hetærolite	*G. E. Moore.*
Hexagonite	*E. Goldsmith.*
Hiddenite	*J. L. Smith.*
Hitchcockite	*C. U. Shepard.*
Holmesite (= Holmite)	
Holmite (= Seybertite)	*T. Thomson.*
Hortonite (= altered pyroxene)	
Hortonolite	*G. J. Brush.*
Houghite	*C. U. Shepard.*
Howlite	*J. D. Dana.*
Hudsonite	*L. C. Beck.*
Huebnerite	*E. Riotte.*
Huntilite	*H. Wurtz.*
Huronite	*T. Thomson.*
Hydrocuprite	*F. A. Genth.*
Hydrofranklinite	*W. T. Roepper.*
Hydromagnesite (pulverulent mineral from Hoboken, N. J., = magnesia-alba, *T. Wachtmeister*)	*F. v. Kobell.*
Hydroniccite	*C. U. Shepard.*
Hydronickelmagnesite	*C. U. Shepard.*
Hydrotitanite	*G. A. König.*
Ilesite	*A. F. Wuensch.*
Indianaite	*E. T. Cox.*
Iodolite (*m*)	*C. U. Shepard.*
Ionite	*S. Purnell.*
Jacksonite (= Anhydrous Prehnite)	*J. D. Whitney.*
Jefferisite	*G. J. Brush.*

Jeffersonite	*L. Vanuxem* and *W. H. Keating.*
Jenkinsite	*C. U. Shepard.*

Keatingine	*C. U. Shepard.*
Kerrite	*F. A. Genth.*
Küstelite	*A. Breithaupt.*

Labradorite (= **Labradorstein,** *Werner*)	*J. C. Delamétherie.*
Lancasterite	*B. Silliman, Jr.*
Latrobite	*H. J. Brooke.*
Lawrenceite (*m*)	*A. Daubrée.*
Ledererite	*C. T. Jackson.*
Lederite	*C. U. Shepard.*
Leidyite	*G. A. König.*
Lennilite	*I. Lea.*
Lernilite (wrong orthography for Lennilite)	*A. Schrauf.*
Lesleyite	*I. Lea.*
Leucanterite	*C. U. Shepard.*
Leucaugite	*J. D. Dana.*
Lincolnite	*E. Hitchcock.*
Lintonite	*S. F. Peckham* and *C. W. Hall.*
Lionite	*T. Berdell.*
Lithiophilite	*G. J. Brush* and *E. S. Dana.*
Loganite	*T. S. Hunt.*
Louisite	*D. Honeymann.*
Loxoclase	*A. Breithaupt.*
Luckite	*A. Carnot.*

Maconite	*F. A. Genth.*
Macfarlanite	*A. H. Sibley.*
Maclurite (= Chondrodite)	*H. Seybert.*
Maclurite (= Pyroxene)	*T. Nuttall.*
Magnesia, native (= Brucite, *Beudant*)	*A. Bruce.*
Magnolite	*F. A. Genth.*
Mallardite	*A. Carnot.*
Mangan-amphibole	*R. Hermann.*
Manganese, ferro-silicate,	*T. Thomson.*
Manganese, sesqui-silicate (= Dyssnite, *v. Kobell*)	*T. Thomson.*
Marasmolite	*C. U. Shepard.*
Marcylite	*C. U. Shepard.*
Marionite	*W. Elderhorst.*
Mariposite	*B. Silliman, Jr.*
Marmolite	*T. Nuttall.*
Masonite	*C. T. Jackson.*
Melaconite	*J. D. Dana.*

Melan-Asphalt (= Albertite, *Robb*)	*C. M. Wetherill.*
Melanolite	*H. Wurtz.*
Melanosiderite	*J. P. Cooke.*
Metacinnabarite	*G. E. Moore.*
Microlite	*C. U. Shepard.*
Molybdate of Iron	*D. D. Owen.*
Monrolite	*B. Silliman, Jr.*
Montanite	*F. A. Genth.*
Moronolite	*C. U. Shepard.*
Mullicite	*T. Thomson.*
Nacrite (= Muscovite)	*T. Thomson.*
Necronite	*H. H. Hayden.*
Nemalite	*T. Nuttall.*
Neurolite	*T. Thomson.*
Newportite	*J. G. Totten.*
Niccochromite	*C. U. Shepard.*
Nickel-gymnite (= Genthite, *Dana*)	*F. A. Genth.*
Nickel vitriol	*T. S. Hunt.*
Nuttallite	*H. J. Brooke.*
Ontariolite	*C. U. Shepard.*
Owenite	*F. A. Genth.*
Ozarkite	*C. U. Shepard.*
Paracolumbite	*C. U. Shepard.*
Parailmenite	*C. U. Shepard.*
Parathorite	*C. U. Shepard.*
Parophite	*T. S. Hunt.*
Pattersonite	*I. Lea.*
Paulite	*A. G. Werner.*
Peckhamite (*m*)	*J. L. Smith.*
Pelhamine	*C. U. Shepard.*
Pennite	*R. Hermann.*
Peristerite	*T. Thomson.*
Perthite	*T. Thomson.*
Philadelphite	*H. C. Lewis.*
Phlogopite	*A. Breithaupt.*
Phosphuranylite	*F. A. Genth.*
Phyllite	*T. Thomson.*
Phytocollite	*H. C. Lewis.*
Polyadelphite	*T. Thomson.*
Polylite	*T. Thomson.*
Prehnite, anhydrous,	*J. D. Whitney.*
Priceite	*B. Silliman, Jr.*
Protovermiculite	*G. A. König.*
Psittacinite	*F. A. Genth.*

Pyromelane	*C. U. Shepard.*
Pyrophosphorite	*C. U. Shepard, Jr.*

Rahtite	*C. U. Shepard.*
Randite	*G. A. König.*
Raphilite	*T. Thomson.*
Rastolyte	*C. U. Shepard.*
Reddingite	*G. J. Brush* and *E. S. Dana.*
Remingtonite	*J. C. Booth.*
Rensselærite	*E. Emmons.*
Retinalite	*T. Thomson.*
Rhodophyllite	*F. A. Genth.*
Richmondite	*A. Kenngott.*
Rœpperite (= iron-manganese chrysolite, *Rœpper*, = Stirlingite, *Kenngott*)	*G. J. Brush.*
Rœpperite (= iron-manganese dolomite)	*A. Kenngott.*
Rogersite	*J. L. Smith.*
Roscoelite	*J. Blake.*
Rutherfordite	*C. U. Shepard.*

Schirmerite	*F. M. Endlich.*
Schirmerite	*F. A. Genth.*
Schoharite	*A. Eaton.*
Schorlomite	*C. U. Shepard.*
Schreibersite (*m*)	*C. U. Shepard.*
Seybertite	*T. G. Clemson.*
Shepardite (= Schreibersite, *Shepard*) (*m*)	*W. Haidinger.*
Siderophyllite	*H. C. Lewis.*
Silicoborocalcite (= Howlite, *Dana*)	*H. How.*
Sillimanite	*G. T. Bowen.*
Sipylite	*J. W. Mallet.*
Somervillite	*A. Dufrénoy.*
Sonomaite	*E. Goldsmith.*
Spartaite	*A. Breithaupt.*
Spartalite (= Zincite)	*H. J. Brooke* and *W. H. Miller.*
Steeleite	*H. How.*
Stellite	*T. Thomson.*
Sterlingite	*J. P. Cooke.*
Sterlingite (= Zincite)	*F. Alger.*
Stibioferrite	*E. Goldsmith.*
Stirlingite (= Rœpperite, *Brush*)	*A. Kenngott.*
Sussexite	*G. J. Brush.*

Talkeisenerz	*A. Breithaupt.*
Telaspyrine	*C. U. Shepard.*
Tellurbismuth	*D. M. Balch.*

Tephroite	*A. Breithaupt.*
Terenite	*E. Emmons.*
Texasite (= Emerald Nickel)	*A. Kenngott.*
Thalite	*D. D. Owen.*
Thinolite	*C. King.*
Tincalconite	*C. U. Shepard.*
Torrelite	*T. Thomson.*
Trautwinite	*E. Goldsmith.*
Triploidite	*G. J. Brush* and *E. S. Dana.*
Troostite	*C. U. Shepard.*
Tungstic Ochre	*B. Silliman.*
Tysonite	*O. D. Allen* and *W. J. Comstock.*
Unionite	*B. Silliman, Jr.*
Uranothorite	*P. Collier.*
Vanuxemite	*C. U. Shepard.*
Venerite	*T. S. Hunt.*
Vermiculite	*T. H. Webb.*
Vermontite	*A. Breithaupt.*
Warwickite	*C. U. Shepard.*
Washingtonite	*C. U. Shepard.*
Wheelerite	*O. Loew.*
Whitneyite	*F. A. Genth.*
Willcoxite	*F. A. Genth.*
Williamsite	*C. U. Shepard.*
Wilsonite	*T. S. Hunt.*
Xanthitane	*C. U. Shepard.*
Xanthite	*T. Thomson.*
Youngite (= Hornblende)	
Zaratite (= Emerald Nickel)	*A. Casares.*
Zincite (= red oxide of zinc, *A. Bruce*)	*W. Haidinger.*
Zinc, silicious oxide,	*L. Vanuxem* and *W. H. Keating.*
Zircarbite	*C. U. Shepard.*
Zonochlorite	*A. E. Foote.*

CONTRIBUTIONS TO THE GEOLOGICAL HISTORY
OF THE
AMERICAN CONTINENT.

[The Address of the retiring President, delivered before the Montreal Meeting of the American Association for the Advancement of Science, 1857.[1]]

BY

JAMES HALL.

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

ONE who accepts a trust has no right to make excuses for not fulfilling its obligations; and yet I would willingly excuse myself from the performance of this last act of the retiring President.

With imperative duties enough to occupy every hour of my time, I cannot do justice to you, gentlemen, or to your kind partiality in assigning to me this position. Circumstances beyond my control have placed it out of my power to make the preparation which was due to the occasion.

You have so often been reminded of the high objects and purposes which bring us annually together, that I need not repeat this now. It is the one great social and scientific festival of the man of science throughout the year, of all others, the most desired: — to the tired and lonely worker, who is endeavoring to add new truths to our stock of knowledge, or to elucidate more clearly what we but partially know; to the teacher of science who has been freed from his daily routine of duties;—this occasion is one of many pleasant anticipations. We all meet as one brotherhood, as the children of one Parent under whose government we delight to live, and whose laws, impressed upon ourselves and upon everything around us, we would better understand. We come together for mutual counsel and mutual aid for the true interpretation of these laws respecting our relation to the animate and inanimate world around us, and our relations to the Great Author of the whole.

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[1] It is only proper for the author to say that during this long interval he has taken no opportunity of revising the original manuscript of his address, which he had long hoped to be able to accomplish. It has been copied without any supervision by him or any changes suggested by him. It has been his desire to present the address as then given, and to make any desired changes or explanations by supplementary notes. A few changes in proof correction, in order to render some points more clear, or to avoid repetition are all that have been done. None of the important additions to our knowledge of the subjects discussed, during the past quarter of a century, have been either directly or indirectly referred to or incorporated in the address.

The address is submitted to the scientific public as an expression of the views and opinions entertained by the writer at that period.

On every recurring occasion of our meeting it is a subject of congratulation; and more particularly for those who can so well remember when the year wore on without those pleasant anticipations of a reunion with sympathizing friends and colaborers in science. But this occasion, more than all others, gives cause for congratulation,—cause even to excite enthusiasm, that true to the enlightened views we profess, and true to the instincts of an unprejudiced scientific spirit, we have forgotten political and civil limits, have forgotten the names of governments and countries, and with cosmopolitan spirit we meet beneath the ægis of Her Britannic Majesty, Queen Victoria, though still upon the American Continent; and now may we more fully claim the right to the title of the AMERICAN Association for the Advancement of Science.

I might, were it the proper occasion, go on to say, in how far the establishment of telegraph wires, railroad lines and various social and commercial relations have tended to bring about this meeting in Canada; but scientific sympathies have done more; and I cannot help thinking too, that the discovery of another fact, that the rocks are of the same age and the same character in Canada as in the adjoining territory, has, in a manner served to assimilate the living inhabitants of this wide country, by showing most conclusively that the ancient inhabitants of this continent were of one nation and one government, and indeed are here today with us, if not to discuss, at least to furnish subjects of discussion, and who will maintain their ground notwithstanding geological systems and system makers. Nor should it be forgotten that the geologist of Canada, with that noble and unselfish spirit which characterizes the true man, and the man of real science, has adopted a nomenclature already framed for an adjoining State; for although directing the investigations over an area six times as large as New York, he saw nothing in the prolongations of the same beds that could induce him to change the name of rocks already well determined. With this immense area Sir William Logan had a right to establish his own nomenclature; but in true scientific magnanimity, which affects no local nomenclature, Canada has been added to New York and the West, that over our common country may extend a unity of scientific language.

But from the present, which brings only delightful associations, shall we not turn one moment to the past, and to the memories of the departed? The last year has been a sad one for our little band of workers in science. Never has a year before cut down so many true and tried ones from amongst us. The names of Redfield, Tuomey, Mitchell and Bailey, need but be mentioned to find a mournful response in the heart of every one who knew them.[2]

. . . .

Our association, composed as it is of laborers in every department of science, has perhaps a right to expect that the retiring President should embrace in his discourse a view of the progress of science in America for the past year. But even if the investigator in one science were able to present such a view, I do not believe that the time allotted for our annual address is sufficient for such a retrospect.

For one who is in constant intercourse with the mute and inanimate existences of former creations, there is little hope that he can offer a language that shall be acceptable to the living, or that his subjects shall interest others than those who are themselves engaged in the same pursuits; and yet, gentlemen, in my poverty, I have nothing else to offer you than the results which flow from geological and palæontological investigations during the past few years, and to offer collaterally some *Contributions to the Geological History of the North American Continent.*

While I would claim for no science a preëminence, I must here protest against that narrow view which has been expressed by some, that Natural History is not a science, and who perhaps believe that mathematical formulæ lie at the basis of all sciences and that these must be used in all their minutiæ to reach any valuable result. But let us inquire whether, after all, natural science as it is termed by way of distinction (one might suppose to distinguish it from *artificial* science) be not truly a mathematical science. Every object in nature is a mathematical figure of some sort, and its form and proportions may be expressed in mathematical formulæ.

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[2] The long interval of time which has elapsed renders it undesirable to repeat here what immediately follows concluding the introductory portion of the address.

Let us take one of those little bodies known as crinoids, which are of various spheroidal or ovoid shapes. They are made up of plates of various forms, three, five or seven sided, with sides of various length and angles as various. More than this, these plates are not plane surfaces, and their curvature has such relation to the sides and angles, and to the form of the whole body, that when put together they shall produce just that form and no other. And now will you tell me by what processes a mathematician will set about calculating the length of the sides, the proportions of angles and the requisite curvature of each of these plates? Long before the mathematician could arrange his formulæ, the naturalist has instinctively measured all, and has reconstructed the body as unerringly as the motion of light.

But then you will ask me, perhaps, what is the use of all this? I will tell you of a use for it. When, like the navigator at sea, the naturalist may be wandering many hundreds of miles away from any fixed point in the geological horizon or geological sequence, he may meet with some dismembered plates of one of these bodies in the rock upon which he stands—no mathematical calculations, no astronomical observations or chemical analysis, will serve to determine the geological latitude and longitude. The density of the mass fails you, its chemical composition will not aid you, and you are thrown back upon the few poor pentagonal, hexagonal or heptagonal plates of a little insignificant body by which to determine the place. Do you measure the sides and angles and sit down to work out the formula with your known and your unknown quantities represented by initial characters? No! The naturalist instinctively, with mathematical exactness, sees the elements of the problem, and the result itself stands revealed before him. All that every other science has failed to do is here accomplished.

I might even occupy the entire time of this address in explaining the beautiful and curious structures of the crinoids. In some periods of geological history they are, so far as known, few, and their mode of construction is in each type very distinct, and every plate is recognizable not only as referrible to the species, but to its place in the body.

In later periods of the palæozoic era there are great numbers of species constructed upon the same general plan, the same number and form of plates at the base, and in the first, second and third series or ranges of plates of the body; and although showing distinctions in the different species, they are not so strongly marked because of the great number of individuals constructed upon the same general plan. But as if in order to individualize themselves, and to overcome the want of character arising from similarity in basal structure, they have adopted another mode of distinction which is equally dependent on mathematical principles. The bodies are surrounded near the summit by arms, and there is a bilateral arrangement so that when viewed from one side which is called the oral side, the two halves correspond. Now on each side of this dividing line you will find the arms originating in one instance in pairs or double, and this prevails throughout, and you have ten arms in five pairs, *et cetera*. The first deviation from this is a group of *three* arms on each side of the ovarian aperture, while all the others are in pairs. Again you have groups of four on either side of the ovarian aperture, and groups of four in double pairs throughout the circumference. They present every possible combination of the numbers 2, 3, 4 and 5, in this manner individualizing themselves where the type is multiplied in species; and these are among the lowest forms which engage the attention of the palæontologist.

Without showing how, at every step from the lowest coral cell to the higher forms of life, the naturalist becomes the mathematician, it will be enough to mention to you the achievement of a naturalist (whose name I need not repeat here) in the conception and production of the outline of a fish from a single scale,—the solution of a problem upon purely mathematical principles aided by a profound knowledge of structural development.

Could the physicist have ever determined the original areas of land and water upon the face of the globe during former geologic periods, or have proved that the sea, once spread over continental areas, was always shallow during many successive creations?

Even when we had determined that our continent was outspread with the sediments of a former ocean, could any geometer show us the direction of the ancient current like our modern Gulf

Stream or its lines of greatest force and the lateral subsidence of this force? and could he point out the quiet mid-ocean areas that prevailed during that long, long palæozoic period, or during later geological ages?

Geology, then, may assume to know something of the structure of the earth, not less than the sciences which weigh and measure it; and when the vast accumulations of geological collections in our country shall be reduced to system, and the story of their origin told, we shall learn that there are laws in operation not differing from other natural laws, though seen and studied by different modes.

The picking up of stones by a child may not be science, nor is the blowing of soap bubbles by the same child any more a science, but either one of these amusements by a philosopher may lead to the discovery of laws which we call science.

It is here that one of the great difficulties of geological science lies; for geology presents herself to us in two aspects, the Physical and the Natural History. I might add also as a third, the Chemical aspect, so large a part has chemistry played in geological phenomena, being indeed at the foundation of many physical changes, and because there are no higher problems on which chemical science has expended her force than those furnished by geology.

To some minds, geology scarcely assumes the rank of a science, except where treated of from a physical point of view. They consider the simplest physical law adequate to the explanation of the most stupendous phenomena. To them, mountain chains rise and are abraded, and the entire crust of the earth is folded and plicated in obedience to certain laws. They see no difficulty in the way of imagining torrents of water moving onward and upward, carrying masses of rocks over heights far above their origin by some simple gyratory force. The entire earth becomes with equal ease to them, either a pliable, elastic, or compressible mass, or a non-elastic body; sometimes one, sometimes the other, changing gradually, or by sudden cataclysms, according to the fancy of the expounder. No wizard's wand ever played so many pranks as the poor earth in the hands of these theorizing geologists.

The more slowly moving, but not less industrious and devoted laborers in science, can never hope to climb the height of that Parnassus which the physical theorist in geology reaches at a single bound. We stand almost aghast at the passage and the progress, but after all we are forced to confess that our *wonder* has been more excited than our reasoning powers; and I am in duty bound to say that geology has suffered more from these theories than from all other causes, though we have much to fear from the natural historians at the present time.

It is the misfortune of all sciences that when, having acquired a certain degree of popularity or interest in the public mind, men are found who, with more or less of true science, are still so fond of making theories of their own, that they at once invent a plausible and acceptable plan which goes forth to the world with all the circumstance attending a real scientific investigation. With truth enough to spare it from the anathemas of science, it still carries false doctrines and false views. Nor is this peculiar to geology, for other sciences have suffered equally from the false zeal of injudicious friends who attempt the reconciliation of new and legitimate views of science with old prejudices and preconceived opinions.

In geology this reconciliation is attempted between the science and Scripture, and every phase of geological science has had its reconciliation with the received opinions or prejudices regarding the origin of our earth, as derived from the first chapter of Genesis. The absurdity of all this is apparent, when we consider that with advancing discoveries every quarter of a century presents our science in so different a phase that a *new reconciliation* is required. Yet each reconciliation and interpretation is advanced with *ex cathedra* authority; and not satisfied with promulgating scientific truths, the author would insist upon making their interpretations dogmas of religious teaching and religious belief.

Another point which I conceive to be more pernicious than this is the Millerism in geology at the present time. But time will not permit a review of this subject.

Let me now, gentlemen, attempt to present you with something more peculiarly appropriate to this occasion; and, not to go beyond the pale of geological science, I may be permitted to call to your attention some of the more important results which have been derived from the investigations of the past few years.

There was a time when we were taught to believe that all the mountain regions of New England, the Appalachian chain, those of Northern New York and the Laurentian mountains, were projecting prominences of the ancient, irregular, primary nucleus of this portion of our planet; but that time has long since passed, and I conceive it to be one of the most important steps in the progress of modern geology that, having shown most conclusively that this science has nothing to do with the origin of things, we have proved that she recognizes no *beginning* of existing conditions. There is no *known* point that can be appealed to as a part of the primary nucleus of the earth, according to the theory which regards the earth as formerly an incandescent body.

All that we can see among the evidences of the earlier condition of the globe shows us that the physical conditions were then as now: that even the *beginning* which we recognize in geology shows our continent as an ocean-bed laved by the widespread waters, and solid materials broken down and transported then as now by currents and spread out in stratified deposits over the ocean-bed. The evidences of these conditions meet us in the elevated ranges of the Adirondack mountains in New York and the Laurentian mountains of Canada, and various isolated points elsewhere. These most ancient of all the rocks which we know tell us only of *water* and its distributing action in first spreading abroad the materials, and their subsequent metamorphism and elevation.

How many similar sedimentary formations may have preceded this one, we know not, but we have reason to believe that this is not the earliest, and we may yet know more of those of prior date. And even now the investigations in the Canadian Survey have shown the necessity of dividing the sediments of this period which we have considered as one; the facts proving these rocks to have been formed during two epochs, forming the Lower and Upper Laurentian.

We have moreover the evidence of preëxisting stratified deposits, for here in these ancient sediments of the Laurentian period are found enclosed masses of preëxisting stratified rocks. Even these ancient crystalline masses, which had been believed to be the oldest if not the very primordial nucleus of the earth, are found to contain laminated masses, not conforming to the stratification or lamination of these beds, but lying at a different angle, telling you that here is a fragment of some *older* stratified mass, which had been broken off, transported by the ocean current, and embedded among the finer débris of a later deposit. Even here then we find that not only are these rocks not the primary nucleus, but that the materials of which they are made are not derived directly from that nucleus.

Instead therefore of the earlier condition of our planet exhibiting the action of great heat, and the results of igneous influences, we find only evidences of water, for metamorphism does not necessarily imply the action of heat in a high degree. Here are the oldest rocks known, still preserving their lines of bedding and their outcrops, as distinctly marked as in the more modern formations.

We are indebted to the Geological Survey of Canada for fully establishing this *Laurentian System* as a series of older stratified deposits and the promulgation of another, the *Huronian System*, which must hereafter form a part of the geological history of our continent.

These Laurentian deposits of immense thickness, and regularly stratified, are nevertheless almost throughout highly crystalline. In the gneiss of this period, which preserves its regular lamination, were embedded large masses of a preëxisting gneiss, with its planes of lamination lying at various angles to the lines of bedding in the enclosing rock. Masses of this kind often appear like an immense accumulation of bowlders, the interstices of which have been filled with sand and mud, the whole having become indurated and subsequently metamorphosed; the bowlders retaining the character and lamination of their original bedding, while the new deposit has assumed its own system of lamination conformable to the lines of bedding.

In this manner we have disposed of these areas of crystalline rocks belonging to the more ancient class, or those prior to any evidence we have of the appearance of life upon our planet; but the analogy in their mode of occurrence, and consequently their manner of formation with the fossiliferous deposits, induce the hope, and I may say even the expectation, that we shall one day prove them to have been the seat of animal and vegetable life. Notwithstanding that our experience thus far negatives such inference, I hold that there is nothing unphilosophical in the belief that all the conditions of their deposition, so far as we now can see, were as favorable to life as the newer formations, and there is no ground upon which we can safely conclude that animal remains will not yet be found in these rocks. That there was a period when life began upon our earth I do not doubt, but there are no evidences beyond those of a negative character, to prove that it might not have begun long previous to the commencement of those rocks at present known as fossiliferous. This same negative evidence has been applied to New England and other portions of the country long after many of us had shown that the mountains of New England consisted of strata of fossiliferous age; and it will probably be a long time yet before all will admit that the Green mountains are Silurian, and the White mountains Devonian and Carboniferous, in their age.

In one respect the lower crystalline rocks present a point of high interest. In their greatest development they are known only towards the north, both in America and Europe. On this continent they rise to the northeast, forming a long barrier nearly parallel to the line of the St. Lawrence; and thence stretching away to the westward by the great lakes they gradually die out in low isolated areas, which form no strong feature, and are finally entirely obscured by later formations. It may still be said that we have intrusive granites of older date, and that these may be parts of that original nucleus. There is no evidence to sustain this view. Granitic masses penetrating the Laurentian rocks may still be portions of the preëxisting stratified deposits; and when we find crystalline granite intruded in the more recent Silurian, Devonian or Carboniferous strata, we may infer that they are derived from the formation entirely beneath. We have no evidence that, even by granitic veins or extruded masses of so-called igneous granite, there is any communication from the surface with that imagined primary incandescent nucleus of our earth.

This point prepares us for a different appreciation of the phenomena presented by crystalline masses. If it does not simplify the whole subject of geological science, it at least takes away the source whence have been derived a large part of those absurd speculations, where imaginary laws of physics and chemistry have been applied to illustrate and sustain an equally false and imaginary geology.

The grand problem of geology is the entire history, chemical, physical, zoölogical and botanical, of the groups of strata constituting the formations of the globe. From the first evidences of life as we know it in the strata, to the present time, is the phase in the earth's history with which geology has most to do; and it is here that the labor of systematizing and classifying has been mainly expended. But even with the best intentions and with the best efforts there are different views expressed, and one type or another, as seems most in accordance with the author's views or his knowledge, is made to mark the eras or periods. This is true not only of the more general but also of the more detailed classifications which have been advanced and adopted.

All these classifications must, confessedly, be founded upon imperfect knowledge, for the geology of the globe has been but partially studied, and negative knowledge, or the absence of all knowledge, is too often treated in generalizations as positive knowledge.

It might be presumptuous to hope to advance any new views upon this subject; still there is one which seems to me not likely to receive sufficient attention in the increasing interest and attractions of palæontology. This science instead of being studied in connection with geology, and as an exponent of geological phenomena, has come to be regarded as a great object of independent investigation; and rocks are looked upon merely as the conservatory or museum of the natural history of the past geological periods. Certainly I ought not to oppose a view which exalts palæontological science; and still, gentlemen, I would not in this study lose sight of the attendant physical conditions, which are equally important to be regarded. The progressive steps in the development of animal life have not been made without great physical changes; and we can never arrive at a true history of our planet till both are studied in connection.

The relation of the physical conditions to the vital force and to its development is a problem of vast importance. It should always be borne in mind, that the expression of animal life is coincident with physical conditions, and that the vital force is never developed independent of physical conditions. This is true without giving any undue importance to these conditions, or any countenance to materialistic views.

The subdivisions into formations, groups and systems, have always been marked by physical changes. Where a group of strata has its materials like those of the preceding group, there will the forms most resemble those of the preceding period; and where the lithological change is greatest, there will be found the greatest change in the nature of the organisms. When an intervening formation cuts off all the species of a preceding group, and the next, or third, is similar to the first in lithological character, so far will the fossils resemble each other not only in generic forms, but in the characteristics of species. We have then the converse of this proposition, that uniform physical conditions give uniform fauna. And I may here say, that while this holds in a large majority of instances, it does not hold true in all; because there is a law still beyond that of physical condition which affects the geographical distribution of species. In modern zoölogy, we have geographical centres of distribution; and we have zoölogical districts or areas both on land and in the sea. Here questions are involved which have not been heretofore fully discussed; but which nevertheless — so long as each geological period is marked by a fauna of its own, and is compared with a modern fauna — demand our serious consideration.

In pursuing this subject, let us for a moment review the recognized periods of American geology, or the geological systems and groups as at present accepted, and their application to the rocks as developed upon our continent. With true scientific spirit, as well as in respect to what has been done on the continent of Europe and in Great Britain, we endeavor to make the greater groups or systems harmonize with those adopted in Europe, while all the minor divisions must be wrought out and receive the expression due to them from the conditions of this continent, dependent upon the prior conditions of the great ocean in which they were deposited. In tracing the development and distribution in that part of our continent which we intimately know, I shall be able to show with more force the results at which I have arrived respecting certain *mountain* and *continental phenomena.*

The Potsdam sandstone, in its wide distribution, is everywhere marked by ripple marks, by broken and drifted shells of *Lingula,* diagonal lamination and all the evidences of a shallow sea. In the uniformity of the character of its materials and its even distribution over wide areas, it is only equalled by its associated member the Calciferous sandstone or Magnesian limestone which, in composition and the uniformity of physical conditions under which it was deposited, presents similar characters. Even long prior to the period of these depositions, the forces which had disturbed the preëxisting continent had ceased, and in contradiction to the generally entertained opinion that in more ancient times there was more disturbance of the crust, there has scarcely ever been, since this advent of life upon our globe, so wide an area or so long a period, indicating repose of all the disturbing forces. At no period has deposition been so uniformly diffused or animal life so widely distributed in the same forms over so large a proportion of our continent.

The second great period of the geological history is marked by the limestone groups, the Chazy, Birdseye, Black River and Trenton. The latter is the most important and gives character to the whole. This formation we know to be widely spread over the continent, indicating until near its close a period of repose, and it is only in some few points towards the northwest that we discover the deposit much attenuated and the influx of muddy sediments interfering in a great degree with the persistence of the fauna.

I might state here incidentally that the fauna of the Potsdam and Calciferous period is totally unlike that of the Trenton limestone period following. In the earliest of known periods when life has flourished, it has been widely distributed and has ceased without convulsions or cataclysms, leaving the area of ocean which it had occupied for the occupancy of new races. In the very first of our creations, therefore, we have evidence to show that life was terminated over wide areas while there are no evidences remaining of violent commotions.

The next succeeding or the Hudson River group appears to us with more features of interest in its physical characters than either of the preceding; and it was at this period, as I shall attempt to show, that the form and outline of our present continent were determined.

The Hudson River group presents us on the one hand with a series of soft shales becoming coarser and alternating with sandstones above, and on the other with irregular masses of limestone and finally immense masses of coarse sandstones or conglomerates with great bands of shale. This group attains in Canada a thickness of 2000 feet, and in Pennsylvania it has a thickness, according to Prof. Rogers, of 6000 feet. Its northeastern extremity stretches out to Gaspé, and on the south side of the St. Lawrence, and, throughout Canada and the Green mountains of Vermont, forms in its metamorphic and folded condition, a great feature of the country. In its continuation southward it forms a marked feature in the Appalachian chain gradually dying out as the range declines to the southward. Along this line, which I shall term its line of trend, it presents no striking variations—nothing but what might be expected in any sedimentary group of rocks. It is only in pursuing it to the westward that we find interesting and important changes. Tracing it through Canada, west by the islands of Lake Huron and by Green Bay to the Mississippi River and more southerly still by Cincinnati and the mouth of the Ohio, we learn that the coarser sediments gradually disappear, the finer mud alone having been transported, and finally that calcareous matter becomes an important ingredient in the formation and sandstone has become an exception. Or to give the best expression to the change, we here in New York and Canada speak of the shales and sandstones of the Hudson River group terminated by the Oneida or Shawangunk conglomerate. In Ohio the same group is known as the "Blue limestone." In Wisconsin and Illinois, this group, which in New York and the east is thousands of feet in thickness, has thinned so that it measures only from sixty to one hundred feet, while in Iowa I have not been able to obtain a thickness of fifty feet. Here we have an extensive group of strata changing from fine and coarse sedimentary materials and passing through all the intermediate phases until it becomes a shaly limestone.

Now in looking at the present continent as at that time an open sea, I conceive that the most rational explanation of these phenomena is that a powerful current has brought these sedimentary materials of sand, clay and pebbles from the northeast[3] and has distributed them along the line now marked by the Appalachian chain; or, if you please to assume that there has been a continent to the east of our own which has been subsequently submerged, then the materials have been distributed along its coast. On whatever ground we view the deposit, we must admit, I conceive, that these materials have been distributed by a current, and that the direction of this current has been in the line of greatest accumulation and coarser materials; and that towards the westward, where we find less accumulation and finer materials, but still a wide ocean, the current gradually diminished until it essentially ceased and the fine materials were slowly spread over the broad area which they occupy in their diminished thickness. Such I conceive to have been the physical conditions of this ancient ocean-bed, an ocean disturbed by currents as extensive and as powerful as any we can point to in modern oceans. Thus from an equable and comparatively quiet period when the Potsdam sandstone was spread widely over the bottom of the sea, we have come to one when powerful currents directed the distribution of the sediments in long lines of deposition, on the seaward side of which we find the gradual thinning-out of the beds for want of a sufficient transporting agency.

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[3] See note at the end of address.

Shall we here inquire, what influence this change in the character of sediments has had upon the fauna of the period; and if the fauna in any manner indicates the force of physical influences? In the east the Hudson River group presents an abrupt change from limestones to black and green shales containing few fossils. In the sandstones above and in the calcareous bands accompanying them are numerous plants, graptolites, some crinoids and acephalons, with gasteropods and cephalopods not known in the group below; while at the same time after being absent through the lower part of the group, many of the Trenton limestone species of brachiopods are here reintroduced midway in the series mingled with the new fauna.

In the west where the strata are calcareous and have a lithological similarity with the formation below we find, likewise, a greater similarity with the fossils of the preceding period. Nearly all the Trenton brachiopoda are found, together with a large proportion of new forms of this family. The acephalous shells, which are the distinguishing fossils of this group in New York and part of Canada, are rare and very subordinate in the west, forming indeed no notable portion of the fauna. At the same time the trilobites of the Trenton limestone occur abundantly in the Hudson River group of the west while they are extremely rare in its coarser sedimentary phase in the east. But even these phases do not hold good throughout, and in western Wisconsin, northern Illinois, and in Iowa towards the attenuated margins of the group, scarcely a single species of the group is identical either with those of Ohio or of New York or Canada; and, finally, on its attenuated northwestern margin it is marked by scarcely more than half a dozen species of fossils, not one of which could be recognized as identical with anything known in the group within two hundred miles from the centre of its greatest development. Such are the phases of the Hudson River group in the great eastern development and in its western and southwestern prolongations, and this was the extent of our knowledge of its phases until within a year past [1856]. During this time the investigations of the Canadian survey under the direction of Sir W. E. Logan have extended to the Island of Anticosti, hitherto almost a *terra incognita*, and the few fossils we had seen from there indicating very clearly a new and rich field. But the results are far in advance of all anticipation.

In order, however, to understand fully these results and the position of that Island, you must know that from this point to below Quebec and still farther down the St. Lawrence, the lower formations with the Hudson River group in its vastly developed physical aspect, together with the formations below, abut against the slopes of the Laurentian mountains; that to the eastward the trend of the Hudson River in its conglomerates, etc., is more easterly while the Laurentians trending to the northeast open a space between them in which lie the Island of Anticosti and the Mingan Islands. We might have expected to find on the Island of Anticosti the upper sedimentary deposits of the Hudson River group with some higher beds, but we were quite unprepared for the result which shows that here, as at the west, are large accumulations of calcareous beds with the recurrence of fossils similar to those along the area exposed by the Cincinnati axis. The same corals, the same brachiopods that mark the "blue limestone" formation of Ohio and Indiana and of the islands of Lake Huron are, here in Anticosti, characteristic forms. On the west the nearest points where these associations of fossils are known are the islands of Lake Huron, three hundred miles or more to the westward of where we stand; while on the other hand, seven hundred miles or more to the eastward, are the same associations of fossils. In Anticosti there is the well-marked coral *Favistella stellata* with *Orthis subquadrata*, marking the same horizon as one thousand miles farther to the west, while all the intermediate space is filled with the sedimentary deposits of the group, in which neither of these forms occurs. To the southward of Anticosti, there has been the great force of that current from the northeast bringing forward the sediments of the Hudson River group, while the more gentle action spread northward the finer material, to be mingled with the calcareous matter there formed from corals and shells and to become the habitation of another fauna. And not only do we find there many species which we know as characteristic of western localities, but many other similar forms both of corals, brachiopoda and gasteropoda, besides many new and remarkable forms. Instead therefore of this group of brachiopoda and other fossils forming, as we had supposed, a peculiar fauna marking the western extension of the Hudson River group, it has, in truth, its centre farther to the east, and the western apparition is an isolated offshoot, as it were, which after all has had but a meagre development when compared with that of Anticosti. The trend of the coarser materials, striking against the southern prolongation of the Laurentian range, cuts off the western extension of these more quiet sea-deposits which were only again reproduced and continued at the westward and southwestward.

The strata which we designate therefore as our Lower Silurian formations have been deposited under very unequal conditions: in the first instance an almost universal sediment of uniform character, ending with calcareous materials marked by a fauna as peculiar as any other; then an entire cessation of these conditions and a new fauna scarcely less universal in its distribution in the earlier epochs such as the Trenton limestone. In the later phases of the Hudson River group it exhibits evidence of violent physical action in the accumulation of coarser debris, powerful currents and unequal distribution of material, with a varied fauna. Certain parts of the group in its geographical distribution are characterized by fossils which are almost or quite unknown in other parts.

Almost coincident with the distribution of the sediments of the Hudson River group to the west and southwest are those of the Medina sandstone and Clinton groups. These later formations gradually die out in the line of the Great Lakes on the westward and have scarcely a vestige in the Ohio valley; while they are, like the Hudson River group, much more persistent in the direction of the Appalachian chain. Although we can only regard these products as the result of the same action which produced the sediments of the Hudson River group, yet we admit the culmination of that action and of that group in the Oneida conglomerate; while the Medina sandstone and Clinton groups in their sedimentary materials must be regarded as the result of the diminishing force which originated and continued in greater or less degree throughout the period. At the time the Medina sandstone (and perhaps the Oneida conglomerate) as well as the coarse materials of the Clinton group were being deposited in New York and westward, the sediments of Anticosti were made in a comparatively quiet sea, and the succession of forms goes on almost without interruption. You perceive that there were already at this early period vastly greater accumulations of sediment along what is now the eastern border of our continent than along the country to the westward and especially in that part now constituting the Mississippi valley.

We are now to contemplate a return to a more quiescent condition of the ancient ocean. The shales, sandstones and conglomerates are succeeded by limestone formations of wide extent. The Niagara group, stretching from the Hudson valley westward, can be traced through New York, Canada, by lake Huron and lake Michigan, thence across Wisconsin and Illinois, where it disappears beneath the Devonian and Carboniferous rocks of Iowa and Missouri. In its southwesterly direction from New York it gradually thins out, but is still known in Tennessee where it is lost beneath the more recent formations. The numerous fossils in the Canada Survey collections proclaim the existence of this group in the peninsula of Gaspé and at numerous other points on the northeast.

Without at this moment giving the deductions which arise from the facts, I may mention here that the Niagara group and succeeding limestones lie uncomformably upon the Hudson River group, in the valley of the Hudson; that, even before the deposition of this member of the series, those contractions which have finally produced the results of which we have been before informed, in the Green Mountain and Appalachian chain generally, had at this early period commenced.

The Medina sandstone and Clinton groups, which lie at the base of the Upper Silurian System and below the Niagara group, have had but a partial distribution of sediments in a westerly direction, while the succeeding limestones of the Niagara are as widely distributed as any Silurian formation upon our continent. Notwithstanding this disturbed condition during a large part of the time of the deposition of the formations at the base of what we term Upper Silurian, a new fauna was introduced, and as we should suppose under most unfavorable circumstances. Short periods of repose indicated by the nature of the strata, alternated with periods of disturbance from the influx of coarse sediments, which must have been disastrous to animal life. We might have presumed that the fauna we find in these rocks was a colony, as it were, from some more favorable centre, but we have not until now had the means of proving that in the same period the island of Anticosti was a quiet sea and there flourished the prototypes of our upper Silurian fauna.

The conditions which existed during the Niagara period, caused by the elevation of the eastern portion of the Hudson River group, thus producing a thinning of the easterly edge of the Niagara group, continued after the cessation of its fauna. Beginning with its thin edge in eastern New York it becomes conspicuous in western New York and Canada West and is thence recognized by the islands around the northern margins of the Great Lakes and becomes an important formation at the Mississippi River. Thus without any apparent disturbance and with a continuance of the same physical conditions which continued to cause the deposition of the same kind of sediments, we have a total change in the fauna marking this later stage, as it were, of the Niagara period. The conditions for its production becoming more and more favorable as we go westward to the Mississippi valley, and there, in its greatest mass and the highest development of its fauna, we leave it passing beneath the more recent formations of Iowa with a thickness of six hundred feet.

In the succeeding order, the Onondaga Salt group follows, parallel in its outcrop and so far as we know coëxtensive with the Niagara group through New York, Canada West and thence to the Mississippi valley; and again southwesterly parallel with the Appalachian chain. Although having this great geographical extent, it has generally a moderate thickness and in some places has not received the recognition of a distinct group. Generally the deposit is that of a shallow sea, while in New York and Canada West it presents the features of a deep basin deposit. While thinning out on the eastern limits of New York to a thickness of thirty feet or even less, and westerly to less than one hundred feet, it still has in central New York a thickness of 1000 or 1200 feet; showing that there was a deep depression, caused in all probability by the lateral pressure on the east, which elevated the rim of the basin on that side rather abruptly while it sloped more gradually to the west. This lateral pressure on the east is apparently caused by the gradual contraction which produced the more abrupt foldings of the crust in the region of New England and eastern New York, thus showing most conclusively that changes of level and modifications of the form of the ocean were going on throughout these periods and that librations extended much beyond the actual evidences of their action at this time.

I have chosen to speak of these formations where we know them fully and have trusted to no doubtful observations or evidence not fully sustained by facts. You will have perceived that thus far the Silurian sea has spread over all the known area of the older rocks on this side of the Rocky mountains. In the period of the Potsdam sandstone and lower limestones the depth was very uniform throughout, as would appear from the fauna. In the period of the Hudson River group we trace the deposits, as we believe, to greater depths than animal life of a molluscan type can flourish, while further to the east were shallower seas. In the Niagara period and the two succeeding ones, the ocean presents a pretty uniform character over a westerly extension of one thousand miles from one meridian, while we have evidence of libration on the east and particularly in that portion occupied by the sedimentary portions of the Hudson River group.

The lines or exposed areas of deposit mark the ancient ocean limits. When the conditions were favorable for sustaining life we look to the fossils for the indications of the changes of area and depth of this ancient ocean. In the absence of life, we seek an explanation of these changes of area and depth, in the direction and force of currents as indicated by the nature and extent of the sedimentary deposits.

If we now look to the next succeeding group of strata, which we term the Lower Helderberg group, we find the direction and extent of its area almost at right angles with those immediately preceding it, and parallel to that great line of sedimentary deposits marking the crest of the Hudson River group. From the investigations of the Canada Geological Survey, the Lower Helderberg group is known on the northeast at the extremity of Gaspé, resting unconformably upon the rocks below; thence to the southwest more or less interruptedly it pursues its course, here at our feet in the bed of the St. Lawrence appearing in outliers which rest unconformably upon the upturned edges of the lower beds of the Hudson River group. Again in the Hudson valley above the Highlands lies the same series resting upon the shales and shaly sandstones of the Hudson River group, while the continuation of that group to the northward in its high crest of the Shawangunk is cut off by the erosion of the Hudson valley. Tracing the group but a little way westward, it passes beneath the Upper Helderberg in apparent conformity to the nearly horizontal Hudson River rocks below and those which succeed it. Southward we trace this group of strata through Pennsylvania, Maryland and Virginia to Tennessee, where its horizon is well marked. But notwithstanding this immense extent, from Gaspé to Tennessee it rarely extends more than a hundred miles to the westward of the Hudson valley in New York though having a greater width to the southwest. It therefore presents a belt of fossiliferous beds of about fifteen hundred miles in length from northeast to southwest. Here then we have the entire area of the west and northwest, as far as ve know (with the exception of a very small area in Illinois and Missouri), entirely destitute of this formation or of any representative of it in the series. Was this vast area a deep sea or dry land? If a deep sea, we should expect to find the fine sediments of the shoreward formations drifted out into it to some extent; but these we nowhere find. In a few instances we have seen the lower beds apparently water-worn and broadly furrowed as if exposed to shore currents or even to littoral action, but of these conditions we have too little knowledge to speculate. But it still appears to me that we must admit the conclusion that at this period a large proportion of the area on the northwest of the line I have indicated must have been above water.

Thus while the trend of the limestones of the Niagara and the two succeeding calcareous groups has a general east and west direction, this limestone of the Lower Helderberg has a northeast and southwest trend. While the Lower Helderberg limestones by their fossil relations are Upper Silurian and intimately related to those below, the trend of greatest accumulation of the two is nearly at right angles.

The line of deposit of the Oriskany sandstone and Caudagalli grit was coincident with that of the preceding limestone deposit and overlies it. The sandstone, indeed, stretched somewhat farther to the west and lay beyond the edges of the limestone upon the upper members of the Onondaga salt group; still the same area is occupied, and the line that admitted of the formation of coral-reefs in the Lower Helderberg period has become again, as in the period of the Hudson river group, the line of currents and of sedimentary deposits. This sandstone formation which in Gaspé is more than one thousand feet thick, and in Pennsylvania according to Professor Rogers, seven hundred feet thick, soon thins out to the westward. In the Helderberg mountains it has a thickness of not more than thirty feet, and often less than three feet, while farther west in central New York it occurs in a few isolated patches of a few feet in extent and less ~~thickness,~~ filling some small depressions in the upper beds of the Onondaga salt group. Here we have apparently again returned to the conditions of northeast and southwest accumulation of sediments deposited in the line of the Appalachian chain.

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The Oriskany sandstone is the period in which acknowledged types of Devonian fossils are introduced. It lies parallel to the preceding Upper Silurian limestones and at right angles to the great mass of the succeeding fossiliferous Devonian beds. These types which were introduced in the period of a sedimentary rock which was deposited along a line from northeast to southwest, and cut off from the succeeding group by a non-fossiliferous belt (the Caudagalli grit), nevertheless appear and flourish in the next formation whose trend is nearly at right angles to the preceding, and the vital manifestations go on irrespective of the change in the physical conditions.

Succeeding the period which I have just described, where the sediments have a northeast and southwest direction, we have to contemplate a calcareous formation whose known trend along the line of outcrop is nearly east and west, stretching from the Hudson valley through New York and Canada West, thence across Michigan, Indiana and Illinois, to the Mississippi river and into Iowa, having an outcrop parallel and apparently conformable in position to the rocks of the Niagara, Galt [Guelph] and Onondaga salt group. The belt of formations just noticed as the Lower Helderberg limestone, the Oriskany sandstone and Caudagalli grit is an important one along the eastern outcrop, but in the west these formations are entirely absent, and what is more remarkable, their absence is not noticeable, and for all we can determine on the ground, the sequence is as perfect in the west as at any point in our palæozoic area.

The calcareous beds before alluded to, including the Schoharie grit, constitute the Upper Helderberg group. The Schoharie grit has very little western extension, but is known for at least two hundred miles in a southwest direction. The limestones of this group mark a line of terrace from the Hudson river westward throughout New York, and present one of the best marked and most extensive physical features of the entire state. It is true the attenuate easterly margin of this group may be traced from the Hudson valley to Tennessee, and the same is true of the Niagara and Onondaga salt groups, though the line of greatest force is from east to west, and the whole series presents but a meagre thickness in Tennessee.

Pursuing our investigations southward from this limestone terrace, we find in eastern New York and in Pennsylvania, first: a series of shales and shaly sandstones; then we have a series of sandstones and shaly beds; and above these, shales and sandstones all of which may with little difficulty be included in one group as has been done in the geological survey of the latter state. But in central and western New York we find the lower part of the series becoming more shaly, and finally more calcareous, so that in the western half of the state it is a group of calcareous shales with beds of limestone known as the Hamilton group, and containing a set of fossils peculiar to itself.

The central portion of the mass becomes better defined as a series of shales and flagstones, with some heavy beds of sandstone which are well marked, though nearly destitute of fossils. This group of strata has received the name of the Portage group.

At the same time the third member has become defined as a set of shales and shaly sandstones with a peculiar assemblage of fossils, and has received the name of Chemung group.

When we follow these successive groups of strata to the westward we find them gradually thinning out. The Hamilton group traced through Canada West, Michigan and Illinois to the Mississippi, there presents a thickness of some fifty or sixty feet, while in eastern New York it is scarcely less than fifteen hundred feet, and is a thousand feet thick in the central part of the state. The Portage group, which in its defined limits in central and western New York has a thickness of one thousand feet or more, is scarcely represented in the Mississippi valley.

The Chemung group, to which in New York I would not attribute a thickness of less than twelve or fifteen hundred feet, is often represented in the Mississippi valley by some thirty to fifty feet, and sometimes by one or two hundred feet of shaly sandstone and beds of limestone. These formations together in Pennsylvania have a thickness, according to Professor Rogers, of five thousand feet to the southwest.

Now regarding the formations in this aspect only, we should conclude that here were transverse currents carrying materials from the east to the west, and this conclusion was reached before we knew anything of the structure of the New England mountain series, and before we understood as we now do the geology of the northeast. The researches of Sir William E. Logan on the peninsula of Gaspé have shown that the *equivalents*, and in fact the *continuation* of these groups, as a part of the Devonian system, have an enormous development in an unaltered state, and in the condition of coarse sediments of at least seven thousand feet in thickness and with a southwesterly trend.

If we follow the line of trend we are taken along the direction of the White mountains, thence into Massachusetts and New York, where we are taught that higher metamorphic sediments overlie authentic Lower Silurian, until we find that the unaltered and undisturbed Hamilton, Portage and Chemung groups of New York are but the prolongation of the Gaspé sediments which have become folded and metamorphosed in the region of the White mountains, but which reappear on the west in their unaltered condition and highly fossiliferous character as soon as out of reach of this disorganizing action of metamorphism.

Instead, therefore, of finding here any *new* force or diverse transporting currents, we have simply a return of the great transporting agencies of the Hudson river period, from the same source apparently in the northeast, and prolonged in the same direction to the southwest, parallel and coëxtensive with the Appalachian chain. The finer materials were spread laterally through the more quiet ocean, till they nearly disappear by thinning out in the Mississippi valley. You will see, gentlemen, with what an enormous amount of sedimentary deposits I am loading the eastern side of the future incipient continent, an amount too great perhaps for credibility, but I have not yet done. From the same source and in the same direction has come the vast amount of sediment forming the Catskill mountain group, with the capping conglomerate, having a thickness of more than three thousand feet in New York and six thousand in Pennsylvania, while the group has entirely thinned out before reaching the western limits of the state, and no vestige is known of it in the Mississippi valley.

Beyond this are, according to Professor Rogers, three thousand feet of red shale at the base of the coal measures and extending parallel to the Appalachian chain. The origin of this shale has not been satisfactorily traced in the northeast, but there is no difficulty in finding a source for this and other sedimentary deposits of the Coal Measures, and an evidence of the same transporting force in the fourteen thousand feet of Lower Coal Measures found by Sir William E. Logan to exist in New Brunswick.

Now, gentlemen, I am satisfied I have wearied your patience too much in these details, but I could not present a simple conclusion without first giving you the data in full.

I have shown you incidentally, as we progressed, that the Hudson river group became disturbed, folded and upturned before the deposition of the Niagara, and still more before the deposition of the Lower Helderberg group which has its trend parallel to this great belt of formation, and which lies upon the upturned edges of the Hudson river group, from Gaspé to the Highlands of the Hudson.

Indeed, I believe the origin of the coarse materials of the Medina and Clinton groups may be due to disturbances among the preëxisting beds of the Hudson river group, and probably derived from the breaking up of the conglomerates of this group in its northern prolongation. Sir William E. Logan has likewise proved that with the commencement of the foldings and plications of these lower strata commenced the metamorphism of the same. And here I cannot forbear from alluding for a moment to the views of Mr. Hunt upon metamorphism. Laying aside all assumption of causes not known to exist, and ignoring the supposed effects of that imaginary nucleus, he has proceeded, from known and demonstrated facts derived from existing conditions, to explain most satisfactorily certain processes in metamorphism; and, instead of appealing to an unknown source for the ingredients of certain metamorphic strata, he has demonstrated the existence of the same elements in the unaltered beds which are known to be of the same age and prolongations of the same strata.

The system of folding and plication therefore, which has been attributed to later violent action, has been going on from the commencement of the Upper Silurian, and though its later phases include the Coal Measures, it has nevertheless been produced by continued action from a long anterior period. Nor has it ceased with the coal, but I believe has been continued in a less degree since the deposition of the sandstone of the Connecticut valley. But even for one moment admitting the supposition that these foldings and contortions have been for the greater part produced at a single period, let us inquire whether this has had anything to do with the production of the mountain chain, or whether this system of mountain-making by upheaval, overturning, etc., is a system of mountain-making, and whether rhetorical eloquence has not made us believe the language without testing the facts. I must assert that it has nothing at all to do with it—that so far at least as regards our mountain chains, the principle in its application is false.

You have seen how, age after age, by the simplest of all processes, the immense sedimentary deposits have been transported and distributed along the line of our great eastern mountain chains; and with the accumulation of sediments this line was depressed time after time, as new deposits were piled upon it; and now at the end of the Devonian period, or at the end of the Coal period, has the whole continent been raised to its present elevation. If there were no folding or contortion, and if the strata of the Alleghany mountains were as undisturbed as those of the great plateau of the west, what would have been the result? Simply that, instead of the Appalachian mountains of six or seven thousand feet in height, you would have had a simple stratified pile of more than twenty thousand feet from the base of the Potsdam to the summit of the Devonian.

But have folding and partial upheaval along mountain chains nothing to do with the grand elevation? I reply, nothing at all; but, on the contrary, every one of these disturbances lessens the effect and degrades the elevation. It is *original deposition* that not only gives direction to our mountain chains, but amount of deposition that determines their elevation; or, if you will reduce it to the simplest terms, we may state that *elevation is due to deposition*, and there can be no great degree of elevation without a corresponding amount of previous deposition of sediments.

Now I conceive this to be the simplest system of mountain-making that can be proposed, and it can be shown to be applicable not only to the Appalachian chain, but to other mountain ranges of our continent.

Let us see, moreover, whether the same simple principle, somewhat extended, may not be applied to continent-making. We admit that the Appalachian chain has given mainly the eastern outline to our continent, and that original deposition has given origin to this mountain chain. The currents of the palæozoic ocean have determined the directions of these sediments, and consequently of the mountain ranges and the outlines of the continents.

A series of strata similarly deposited by an ocean current, but nearly at right angles, mark the western outline of our continent; and though modified by subsequent depositions, they are nevertheless fundamentally of like origin, due to the same causes, giving origin to a mountain chain and the outline of a continent.

Now, if we extend our observation to the Mississippi valley, a thousand miles to the west, where we have essentially the same set of strata as those forming the Appalachians, we find no mountain ranges. I shall be told that it is because there are no upheavals, but this is not known to be true. Several axes and extensive faults cross the valley of the Mississippi nearly at right angles to the Appalachian chain, elevating all the strata from Lower Silurian to Devonian. Nevertheless, though the strata are elevated from ten to thirty degrees, they produce scarcely a perceptible feature in the prairie country.

Uplifting has not produced elevation, but has rendered the strata liable to degradation, by breaking or weakening them along the line of fracture or folding. I have already shown that the groups of sedimentary strata composing the Appalachian chain have thinned out to a few hundred feet at most on the Mississippi and there are consequently *no materials* of which to make mountains in the great Mississippi plateau. The general height of the country gives the entire elevation produced by the thickness of the beds, and there can be no more. We could have no mountain chains along a great plateau like this, however much the strata may be folded and contorted, unless there should take place a dislocation of the older Laurentian or Huronian rocks from below; and although such a phenomenon would be in accordance with geologic *theories*, respecting the upheaval of mountain chains, yet I suspect that careful search would prove that no such upheaved chain of mountains exists upon the surface of the globe. In fact, that the upheaval of mountain chains, except in the sense that the elevation of a continent is upheaved, has never taken place. In this connection let me offer an example in point in support of what I have said. It has been stated that the White mountains are composed of accumulations of sedimentary deposits of Silurian and Devonian age, having altogether in their undisturbed condition, a thickness of from fifteen to thirty or forty thousand feet (or more, if I follow authority), while at the same time the White mountain chain little exceeds six thousand and the Green mountains not more than four thousand feet in height. Now in tracing this mountain chain to the southward, we observe, on the west side of the Hudson river above the Highlands (which are produced by an area of older rocks), the Catskill mountains reaching an elevation of about four thousand feet, and stretching away to the westward in gradually declining isolated hills. These have been regarded as part of the Appalachian chain but they have nothing to do with it so far as being involved in the disturbance of the strata. They stand alone as an isolated group, of which the nearly horizontal pile of strata gives this great elevation. The disturbed Hudson River group makes one or two hundred feet of their base. *Deposition* has here given *elevation* without loss or degradation by any of those foldings and plications which are supposed to lie at the foundation of mountain-making. Had the strata of the Green mountains and White mountains remained undisturbed, and in the original condition of deposition, they would have overtopped the Catskills by several thousand feet.

Original deposition, then, directed by ancient currents, has unquestionably determined the direction and elevation of our mountain chains.

The time on this occasion will not allow me to extend the application of the same principle in unravelling and explaining complexities, that may often occur—when,during the depression of a continent or of a portion of it which may afterwards form a mountain chain, transverse or diagonal currents may cause such an amount of deposition as to produce a great complexity of structure, when the continent shall become elevated.

The simplicity of the North American continent is due to the originating causes producing the direction of the palæozoic currents; while a large part of the eastern side has not been subsequently submerged, to any great degree, and has thus escaped the causes of complexity. The wide extent of later deposits in the interior of our continent, which give contour to that portion, was in a great degree paralleled with the eastern deposits. The forces which we say have rent the eastern, the central and the western, are merely the vast accumulations of material which become elevated, perhaps simply from the great accumulation, thus rendering the escape of heat from the interior slow or impossible. And in this statement, it must not be forgotten, that the central portion of our continent, and some parts of the western portion, were not continental, but submerged areas, till after the Cretaceous period, and to some extent also, as late as the Tertiary period; and if these have greater elevation it is due to greater deposition.

Although it is not necessary to introduce the Coal Measure deposits into the Appalachian system for the purposes of explaining their elevation, they are nevertheless complicated with it, in a great degree. But all the phenomena, so far as regards folding and plication, and the production of the northern mountains as well as a large part of the Appalachian system, do not include the Coal Measures in the elevation. On this account I would call your attention to some facts connected with the deposition of the coal formation in the United States. But before proceeding to do so, I must dwell for a moment on the Carboniferous limestones, or those great calcareous formations which, in the valley of the Mississippi and the southwest, lie beneath the Coal Measures.

In tracing the origin of our sedimentary formations from the northeast, we do not find any cessation from the beginning of that period called Devonian till we come to the Coal Measures. The whole country in this direction would seem to have been submerged, and the course of these ancient currents to have continued with little interruption.

In the Hamilton, Portage and Chemung groups, we first find *land plants* and these remains increase towards the northeast, while they are rarely or never known in these formations to the west of the limits of New York. In Canada, however, similar plants are known, and the account of a specimen in the collections of the Survey, published by Mr. Dawson, shows that dry land may have been near, and we have every reason to believe that dry land capable of producing vegetation did first appear in that direction and in the early Devonian period. While in the east, the Catskill mountain rocks, and the red shale beneath the anthracite Coal Measures, were being deposited, we have in the west no representatives of those rocks; the Carboniferous limestones beginning their deposition directly upon the Chemung group.

The condition of the ocean in the west during these periods is worthy of attention. Upon the cessation of the sediments of the Chemung group, commences a limestone formation of great extent, and marked by numerous species of fossils. By a well-defined line, this lower limestone is separated from a succeeding one, marked by a wholly distinct set of fossils; while its greatest development lies to the south of the area of greatest development of the first. In the same manner a third, less strongly defined; and with an intermediate sandstone formation, still another limestone succeeds, and this with its northern limit far below or to the south of the first and second, and its greatest development is to the southward of the preceding members.

Now the simple explanation of this seems to be that the sea, in which limestone deposits could be made, was contracting on the north and gradually extending to the south and southwest, while we have evidence to prove that much dry land existed to the north,—evidence which I shall presently attempt to show. This fact of the gradual expansion of the ocean to the southwest, and its contraction on the north, is significant when taken in connection with the succeeding formation of the Coal Measures; for which reason I introduce here the notice of these facts.

It might appear unnecessary while so much has been written, and so well written, upon the subject of the Coal Measures of the United States, to offer anything in this place, but I believe I can present one or two points of interest in this connection.

I have already called your attention to the fact, that the sedimentary materials of the coal formation have their greatest expansion to the northeast, and that we find the earliest record of land plants in that direction. The Coal Measures, as a whole, though widely expanded, show a gradual thinning-out to the west and southwest, and in Iowa and Missouri have become greatly attenuated as compared with the same formations in Pennsylvania, Ohio and Kentucky.

I have just now reverted to the fact that on the north and west, we have evidence of dry land, or of land raised above the sea previous to the deposition of the coal. While in Pennsylvania, Ohio, etc., the Coal Measures apparently rest conformably upon the strata below them, in Iowa and Missouri they rest unconformably upon the rocks below. While the prevailing dip of the older rocks is to the southward, there are nevertheless numerous faults and axes which uplift the strata in a direction from N. W. to S. E. or at right angles to the Appalachian chain.

In the west these ancient rocks of Lower and Upper Silurian and of Devonian age have, previous to the deposition of the coal, presented numerous inequalities of surface, partially due to the broken strata produced by elevations and faults, and partially by subsequent denuding action.

In Illinois, Iowa and Missouri we find the Coal Measures spread horizontally over the uplifted edges of these older strata, sometimes barely filling the depressions and inequalities of original surface, giving origin to numerous little circumscribed basins, often of only a few yards in extent. And not only do we have the evidence of ancient worn and unequal surfaces of this ancient land, but we find the sand mud, and carbonaceous matter often filling fissures, and penetrating into ancient caverns, which must have been worn while these older strata were near to or above the level of the sea. The wearing down of the crests of low axes, and the broken margins of faults, prove the existence of a powerful eroding action, and as we know that at this period the sea was gradually retreating to the southward and the land rising on the north, the motion of the abraded materials would be in the same direction.

There are other phenomena connected with the coal period, which are of much interest. In Pennsylvania and Ohio, we observe several intercalated bands of calcareous shale or argillaceous limestone, charged with marine fossils, thus indicating that the area of the Coal Measures was sometimes depressed beneath the sea. These bands of limestone with marine fossils are known in the Coal Measures of Ohio, Illinois, Iowa and Missouri. The limestone of this period in Missouri has been usually referred to the Carboniferous limestone, not recognizing these beds as distinct from those below the coal; and it was not until the publication of the Missouri Report that Professor Swallow clearly indicated the position of the beds on the Missouri River as a part of the true Coal Measures.

In the meantime and for many successive years, fossils of Carboniferous age had been found in the far west along the Rocky Mountain range from as far north as Fort Laramie and the Great Salt lake, by Capt. Stansbury, and at numerous intermediate points by the Pacific Railroad Surveys and other United States explorations, and on the Southern Boundary line and in New Mexico by the Boundary Commission. Dr. Roemer also had discovered some fossils from the Carboniferous limestone in Texas. We had therefore a knowledge of the vast extent of a limestone along the Rocky Mountain range from latitude 43° north, to about 30° south, containing fossils referred to Carboniferous age, and the rock was almost by common consent referred to the age of the true carboniferous limestone and has been cited as Lower Carboniferous limestone.

Having had occasion during the two preceding years to give some attention to the rocks of Carboniferous age, and more particularly during the past year to a comparison of the fossils, I was surprised to find that some of these southwestern species were identical with those from the well-developed carboniferous limestone in the Mississippi valley, Iowa, Illinois and Missouri to Alabama. Furthermore a careful comparison of species collected by myself many years since from the limestones of the Ohio coal measures, with others collected by Mr. Nicollet, Capt. Stansbury and by Messrs. Meek and Hayden from the limestones of the Missouri river, showed a most positive identity of species. A farther comparison of all these with the carboniferous fossils from some of the Pacific Railroad Surveys and those of the Boundary Survey proved such positive identity of species as left no doubt that all the limestones of the southwest, that is, in the approaches to the Rocky Mountains and beyond them, are of the age of the coal measures. A later comparison of the collections of Marietta College has enabled me to demonstrate the identity of several of the species described by Dr. Hildreth with those of the southwest, and it may be unnecessary to add that these are Coal Measure fossils.

These limestones, then, of the southwest, spreading over an area many times as large as all the Coal Measures of the remainder of the United States, are the greater development of those thin bands of limestone with marine fossils that mark certain lines in the Coal Measures of Ohio, etc. These bands of limestone, from insignificant beds of a few feet in thickness, have extended to several thousand feet in thickness and mark great lines of mountain ranges, and, lying often nearly horizontal or dipping at various low angles, present vast cliffs along mountain passes towering thousands of feet above the plain below. And these, gentlemen, are limestones of the Coal Measures, while the productive Coal Measures are unknown over this vast region. With what propriety, then, can we say that the coal period is preëminently the period of vegetation, as is done to sustain a theory that it marks one day in the creation of the world, or to sustain another view that coal vegetation gives the character to the entire palæozoic series?

I regret, gentlemen, that I cannot bring you some very brilliant theoretical result from all these data; but the very simple explanation of all is, as I have shown you step by step, that the northeasterly portion of our continent was either already dry land, or was just rising from beneath the water at this period; that as low ground just emerging from the ocean, and at intervals deluged with sand and mud and lying little above the water level, it sustained a luxuriant growth of vegetation; while the gradually retreating ocean still spread over by far the largest proportion of the continent on the west and southwest. The accumulated reefs of limestone, three thousand feet in thickness, attest the long period of quiet, and freedom from irruptions of sedimentary matter upon these regions. At intervals the unstable position of the newly made dry land permitted the incursions of this southwestern ocean, in a shallow expansion over the great Coal Measure area, only to return again, upon the gradual elevation of the surface, and thus once more to become a low land fitted for the production of plants.

This subject followed out promises us some of the most interesting and beautiful results, and will show us how the librations of the surface have been connected with the destruction and reproduction of the successive floras of the coal period.

There are numerous interesting generalizations arising legitimately from the facts I have brought before you, but of which time will not permit a notice. In no direction, however, more intimately than in that of palæontology, do these facts and physical deductions point to interesting and important conclusions. There is one fact, which in this connection I cannot omit, in regard to the fossils in these intercalated beds of limestone in the Coal Measures, and this is—that they are species of the same genera and many of them of allied forms to those characteristic of the limestones below the Coal Measures, or of the Devonian and Silurian limestones; showing conclusively that the condition of the ocean did not essentially vary through this long period, and the only difference between the coal period and that of the preceding was that there was a larger area of dry land giving origin to the great growth of vegetation of that period. Moreover, the large area of this ocean formation, characterized by a marine fauna of the same period, compared with the relatively small extent of the areas of carbonaceous matter and terrestrial vegetation, shows that so far from being essential, they are rather incidental conditions of the Carboniferous period.

CONCLUSION.

In the progress of the earth from the beginning of animal life to the present time, we are but watching the progress of an ocean bed to its final development as a continent,—and for the most part, every step has been one of progress towards the present condition. Whenever any portion of this ocean bed has been raised above the surrounding sea, land plants have taken possession of that portion. If it has remained long enough above the surface, land animals have become its denizens. As a matter of course, the higher land animals did not first appear, for even now, similarly situated lands do not sustain the higher organizations. The occurrence of mammalia in the Jurassic period is no more irrelevant to that period, than the occurrence of plants in the coal period. It is a fact perfectly in accordance with all the rest of our legitimate information, that the organic conditions have always been coincident with the physical conditions.

Geology, if we would let alone grand theorizing, is a simple and beautiful study, in which we see everything evolved naturally and harmoniously, without at any time great and sudden changes. We remark those changes as one who having viewed a city in its progress, should fall asleep for a century and afterwards behold the difference. But to one who could have seen stone laid upon stone, and each edifice completed singly, it would have had but the aspect of natural and quiet progress.

NOTES.

PAGE 37. The gneiss cited as Laurentian and containing bowlders of preëxisting gneiss I have seen in the region of Lake Superior, and they were referred to the Laurentian age on the authority of Sir W. E. Logan. I am informed by Dr. T. Sterry Hunt that these masses are of Huronian age and carry boulders of stratified Laurentian gneiss. Although this shows that we have carried back the stratified gneiss only to the Laurentian period, it proves nothing in favor of a granitic nucleus nor of extruded granites of igneous origin.

PAGE 41. A comparatively quiet condition, during a large portion of the period of its accumulation, is indicated by the uniform fine-grained character of the quartzose sand, which extends over large areas. The tracks of Crustaceans, found by Sir William E. Logan upon certain surfaces of this rock in Canada, appear to have been at about the same level along the strike of the formation for 400 miles. The deposit was evidently made in shallow water, and from the mark of a wind-ripple found upon the surface in connection with these tracks at one locality, it is evident that the sand had been laid bare at the ebb of tide.

The formation in Canada has a thickness of from 70 to 600 feet, and an extent of a thousand miles flanking the Laurentides from Bellisle to its extreme southwestern extension in the Province. "The hills, at no very great distance from the outcrop of the Potsdam formation, rise to heights varying from 500 to 4000 feet; and while the sand at their base lay between the ebb and flood tide, the flank of the Laurentian mountains must have formed the coast of the Lower Silurian sea. As has already been stated, these hills extend from Labrador to the Arctic Ocean, and we can thus trace out the ancient limit of the ocean for 3500 miles."

This formation extends southward through New York,. Pennsylvania, Virginia, and notably through Tennessee, and occurs flanking the older metamorphic rocks in Texas. The formation is extensively developed in the States bordering the upper Mississippi and in the upper Lake Region. It likewise constitutes an important feature in the Geology of the Black Hills country.

The character and thickness of the formation in New York, along the eastern flank of the Adirondacks, is similar to that existing in Canada. In the Mohawk Valley at the southern extremity of the Laurentian gneiss it is often scarcely recognizable, and never attains more than a few feet in thickness in any of the exposures. In Pennsylvania the maximum thickness of the formation is from 2500 to 3000 feet, while in some localities its thickness does not exceed twenty-five feet: — this variation being due, in some degree at least, to the original inequalities of the sea bottom.

In Tennessee, according to Professor Safford, the Potsdam group has a thickness of more than 20,000 feet; but this includes the conglomerates below and the magnesian limestones above.

The source of the Sandstone flanking the Adirondacks has been attributed to the subjacent Laurentian gneiss, and it has been suggested that long continued trituration has broken up and separated the mica, hornblende, etc., which have been floated away, leaving only the siliceous grains to form the great deposit. It may, however, be questioned whether the gneiss has been the chief source of material. In the great conglomerate or breccia (for the fragments are mostly angular), at the base of the Potsdam on the southeastern flank of the Adirondacks, the fragments of gneiss are few and small, and the greenish material, which may have been derived from the mica and hornblende, is in small proportion. By far the largest constituent of the mass is of quartzite, corresponding with a similar material found in many places lying upon the gneiss, and sometimes referred to the Potsdam, while in some localities it appears to have more intimate relations with the gneiss below.

Fragments of feldspar are sometimes found embedded in this conglomerate, and, in the more interior localities, a larger proportion of fragments is evidently derived from the underlying gneiss rock. In Tennessee, Professor Safford represents the lower portions as "coarse gray conglomerates, talcose, chlorite and clay slates, repeatedly interstratified, all having a semi-metamorphic aspect." But it should be recollected that the adjacent metamorphic rocks may be of newer age than the Laurentian. Again "the pebbles are quartz and feldspar mainly, generally rounded, but sometimes more or less angular." The conglomerate "usually contains more or less chloritic and talcose matter in its interstices."

Along lines of the most active erosion, no large deposits are made, and those which are made are of a coarse character; the great accumulation is just outside of the limits of the most active forces.

The underlying conglomerate or breccia is widely disseminated, having a greater thickness at the south (in Tennessee) than in the north. This may be accounted for by the gradual subsidence of the entire continent, or the eastern portion thereof. The deposition of sediments apparently began at the south, coincident with the subsidence, and there was a recurrence of similar conditions during the whole period of depression, so that at the base we have a conglomerate which is followed by a sandstone; and in Tennessee, where the subsidence was greatest and farthest removed from the final source of sediments, we have the greatest thickness, and the uppermost beds are shales, indicating deep water and distant source of material.

In the original nomenclature the term Potsdam sandstone included conglomerates and quartzites resting on the Laurentian gneiss, with the sandstones above these, to the base of the calciferous sandstone (magnesian limestone). It has been found, however, in some localities, that there is a gradual passage from the Potsdam to the calciferous; and above the latter a repetition of the previous conditions in the deposition of a fine grained sandstone in the northwest known as the St. Peter's sandstone; while in Missouri there are several alternations of sandstone and magnesian limestone, showing that similar conditions were recurrent to the close of the series.

It seems pretty clear, therefore, that while the magnesian limestone was being deposited in mid-ocean there may have been a deposition of siliceous sand along the coast line or littoral zone, giving a continuous series of sandstone layers, or, as sometimes occurs, with thin calcareous interlaminations. We can conceive it possible that the Potsdam fauna may have lingered along the shore lines, even after the close of the deposition of the magnesian limestone.

In Tennessee, Professor Safford includes all these in the Potsdam group, recognizing three divisions; the lower division of conglomerates and slates; the middle, the Chilhowe sandstone, with intercalated beds of dolomite; the upper including the Knox shale and the Knox dolomite which closes the series. The whole constitutes a series of deposits most naturally grouped together, as belonging to a single geological period, and they are represented as conformable throughout, and are equally involved in the later movements which have folded and plicated the mass as shown in the mountain exposures. The series, wherever of considerable thickness and exposure, is susceptible of subdivision, and Dr. Owen suggested a triple division of the mass below the magnesian limestone in the upper Mississippi Valley, while a different subdivision is applicable to Missouri. Thus far no one has successfully attempted the correlation of the subdivisions recognized in different parts of the country.

The relations of the fossiliferous beds at Troy, N.Y., referred to the lower Potsdam, have never been satisfactorily determined. It is a matter of mere conjecture. The fossils are from calcareous masses embedded in slates whose age and relations are unknown and which in fact cannot now be separated from those of the adjacent Hudson River group. Therefore it is unsafe to attempt to correlate, with these beds, strata in other portions of the country.

(3) PAGE 42. The northeastern source of the mechanical sediments has been questioned by several writers. The greater thickness of many of the formations in Pennsylvania, than to the northward, has induced Professor Lesley to infer that the source of sediments has been in the southeast, instead of the northeast as I have suggested. By northeast I would be understood to mean northeast of the centre of the Appalachian chain. The rapid attenuation of all the sedimentary deposits above the Potsdam, to the south of Maryland, and the increasing proportions of the calcareous formations in the same direction, seem to me to indicate that the great source of sedimentary deposits was to the north and northeast. In the southwestern continuation of the Hudson River group we find it passing gradually into calcareous material. Taking the entire series from the base of the Hamilton to the top of the Catskill, including the upper member or Pocono sandstone, embracing altogether a thickness of more than 10,000 feet, we find the whole represented in Tennessee by a few hundred feet of strata. At the north the strata of the Hudson River, Medina, Clinton, Hamilton, Portage and Chemung groups are largely arenaceous; while in the south the corresponding formations except the upper ones are mostly calcareous. Had the source of the sediments been from the south, we should have to assume that the coarse deposits were made at a great distance from this source and that they were transported across a large area which now preserves only the evidence of gentle forces and open sea accumulation. It would seem, therefore, that this proposition is neither necessary nor satisfactory, and that the source of sediments was probably from the north or northeast.

PAGES 44, 45. In 1857 the Canadian Survey had recognized the entire belt of slates, sandstones and conglomerates on the southeast side of the St. Lawrence, including also a considerable proportion of the metamorphic rocks farther to the eastward, as of the Hudson River age, and the subject was thus treated in the address.* Subsequent investigations proved a large proportion of this area to be of older formation, but notwithstanding this fact, the reasoning regarding Anticosti and its relations still remains correct. The island is a mass of strata in which calcareous matter prevails to the exclusion of coarser sediments.

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* Prof. Rogers, and other geologists, had before this time recognized the metamorphic rocks of New England as of palæozoic age.

Owing to the more northerly trend of the Laurentides from the Pt. de Monts, a wide space to the northeast has been occupied by the ancient palæozoic ocean in which were deposited the several members of the series from the calciferous to the Niagara inclusive, in quiet succession.

The intermediate space between Anticosti and the Mingan Islands on the north has been occupied by the upper part of the Birdseye and Black River limestones, and by the Trenton limestone proper, the Utica slate and lower part of the Hudson River group; the upper members of the latter, to nearly 1000 feet in thickness, forming the northern shore of Anticosti. This gives to the lower formations, from their base to the top of the Hudson River group, a thickness of more than 2600 feet. A large part of the island, however, is composed of the succeeding formations. The Anticosti group consists of representatives of the Medina, Clinton and Niagara groups, but these are everywhere calcareous, having a maximum thickness of nearly 1400 feet.

The sequence of open sea deposits of this region, from the Trenton to the Niagara groups, has, therefore, apparently been more complete and less interrupted than at any other point within our knowledge. Unfortunately, the hiatus between the Mingan Islands and Anticosti leaves us without positive knowledge of the nature of the deposits in this interval. The coarser sediments of the Hudson River group, in its typical localities, occur in the upper part of the series, and since here we have the higher portion of the group represented by nearly 1000 feet of calcareous beds, it is fair to infer that it may have been calcareous throughout. In its physical aspect as well as in its fossil contents, the Hudson River group at Anticosti is comparable with the series of the same age in Ohio and Indiana, the northern part of Kentucky and in Tennessee.

Crossing from Anticosti to the peninsula on the south side of the St. Lawrence, we find the Hudson River group represented in its finer and coarser sedimentary deposits as known in its typical localities, and here as well as at Montmorenci near Quebec and elsewhere, complicated with the older slates, sandstones, etc., of the Quebec group. From Gaspé, therefore, by the south side of the St. Lawrence to Quebec, and thence to Montreal and by Lake Champlain and the Valley of the Hudson to the Highlands of New York and through New Jersey, and Pennsylvania to Virginia, we have this belt of sedimentary strata, usually more or less disturbed and complicated, and sometimes partially metamorphosed, but always well-marked and clearly distinguishable over an extent of outcrop of nearly 1500 miles.

PAGE 46, ¶¶ 3, 4. Of late the question has been raised regarding the unconformability of the Niagara group and subsequent formations, to the rocks of the Hudson River group below, and consequently the date of the foldings and plications of the latter have been controverted by geological writers. The original idea of unconformability was maintained by Rogers and Mather of the N. Y. Geological Survey while Emmons in one case explains the apparent want of conformity as due to faulting, while he gives another section showing conformability. Sir W. E. Logan everywhere maintained the nonconformability of these formations. I have held this view from my own investigations, though expressing some doubt in a single instance, but I have no longer any hesitation in my own mind regarding the matter, for wherever there may be *apparent conformability* it can only be delusive.

At St. Helen's island near Montreal, and elsewhere, the limestones of the Lower Helderberg rest unconformably upon the inclined and eroded edges of the Utica slate or other lower members of the Hudson River group, with an intervening conglomerate in which are found pebbles of Trenton limestone, Utica and Lorraine shales. The entire superficies of the valley had been eroded previous to the deposition of this Lower Helderberg limestone. Numerous localities along the Hudson River valley present evidences of want of conformity between beds of the Hudson River group and those of the succeeding formations. Those which have been chiefly cited are Rondout and Catskill or between these two places on the west side of the Hudson, and Becraft's mountain on the east side. The more conspicuous sections are near Rondout, but one of the most interesting occurs at Austin's Mill near Catskill. At this locality the general aspect of the section presents an appearance of conformability between the formations below and those above. A critical examination, of the line of contact, however, presents not only distinct discordance of the beds, but in one place the line of junction is marked by a thin stratum of grit with rounded pebbles separating the sandstone of the Hudson River group from some calcareous layers of the age of the Niagara group. This point is in the trough of a synclinal and the lines of stratification are essentially parallel. At another place in the same exposure near the edge of the synclinal are presented the upturned edges of the Hudson River sandstones and shales, succeeded by a thin band of grit and breccia; and upon this rests a soft magnesian limestone of the age of the Onondaga Salt group, succeeded by the haydraulic limestone and members of the Lower Helderberg series.

The apparent conformity in many cases is an illusion; for, although the successive beds may appear to be in accordance, so far as bedding is concerned, a more careful examination may show that the strike of the one crosses that of the other, as is seen in the exposure at Austin's Mill, where the divergence of strike between the Hudson River strata and superimposed formations is at least thirty degrees at one point on the edge of the synclinal.

It should be noted that in the greater number of junctions described, between the Lower Helderberg and Hudson River groups, the limestones of the former group are often found resting on the shales and slates which characterize the middle and lower members of the Hudson River series, while the upper arenaceous beds of the group are wanting. Nor would absolute contact and parallelism of beds in this case prove conformity of structure. It is quite impossible that complete conformity should exist between the beds of the Hudson River group and any portion of the Niagara or Onondaga group, in the absence of the entire series of the Medina sandstone marls and the shales of the Clinton group; and when it is proposed to bring the members of the Lower Helderberg into parallelism with the Hudson River group, the absence of several thousand feet from the base of Medina to the top of the Salt group shows the impossibility of such conformity. Unfortunately, the absolute junction of the two formations has rarely been carefully sought — where a few inches of conglomerate or breccia may represent the interval between the close of the Hudson River and the beginning of the Helderberg period — an interval of thousands of feet in thickness and ages in time.

The beginning of the disturbance was at the close of the Hudson River period and the great thickness and extent of the red marls of the Medina indicate that the deposit was not made in an open sea. A vast interval separates the Lower Helderberg from the Niagara. The whole Onondaga period with its marls and dolomites of one or two thousand feet in thickness is interposed. The open sea was shut off and a region with salt lakes spread over the formations of the preceding ocean area for a thousand miles to the westward (as now evidenced in its salt and gypsum beds), while thinning out on the east. Over this eastern outcrop and far eastward swept the Lower Helderberg Sea, which laid down the limestones in the valley of the St. Lawrence, on the flanks of the Beloeil mountains, east of Montreal and from Gaspé* by Maine, New Hampshire and Vermont, and from New York to Tennessee. The break, shown by all these phenomena, ending in the unconformable spread of Lower Helderberg, alike over the filled up Onondaga basins and uncovered Niagara and Hudson River groups in eastern New York; over eroded Utica slate in eastern Canada, and over older crystalline rocks from New England to Gaspé, marks the first great geological revolution from Potsdam time.

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* The limestone in Gaspé is 2000 feet thick.

PAGES 49-51. The term *trend* of the formations was used to indicate, as far as known, the line of greatest accumulation or the line of transport of the material. Since this knowledge is derived from the outcrops. and much intermediate ground remains unexplored beneath superincumbent formations, it has been urged that the inferences may not always be technically correct; but I do not conceive it probable that any erroneous conclusions of importance are likely to result from such statements of the fact, —at least the actual outcrops furnish more reliable information than any system of hypothesis.

The Oriskany sandstone, from Gaspé to Virginia in its trend or line of greatest deposition, is essentially parallel to the Lower Helderberg. In New York it extends, as a marked formation, farther west than the Lower Helderberg, and is also found in Canada West. Both the Oriskany sandstone and Lower Helderberg group are known in southwestern Illinois. The eastern extension of the Oriskany is overlaid by the Caudagalli and Schoharie grits. In central and western New York these formations are absent, and the Oriskany was exposed to eroding influences during the time of their deposition, and until the beginning of the deposition of the Corniferous limestone. Through the central and western part of New York the Oriskany sandstone is found filling scattered depressions of limited extent in the preceding formations. Sometimes the lower beds of the Upper Helderberg group contain rounded and angular fragments of this sandstone, showing conclusively that there was considerable erosion of the strata below, previous to the formation of the limestone.

The sandstone is unknown (except as small isolated patches or nodules, or as fragments embedded in the limestone above) between Seneca Lake, in central New York, and the Niagara River, and yet near Cayuga, in Ontario, after an interval of 200 miles, there are outliers of the rock yielding an abundance of the characteristic fossils of the formation. It is, therefore, fairly inferred that the absence of this rock in many places may be due to erosion, while the remaining evidences of its existence might readily be overlooked.

PAGE 55. The Address has been facetiously criticised as proposing a system of mountain making with the mountains left out. The Address was not intended to propose any system of mountain making, but to show that mountain ranges were coincident with lines of great sedimentary accumulation. That this accumulation of sediments with its subsidence and consequent folding and plication, and the subsequent elevation of the mass and erosion of the anticlinals, had shaped the mountains; that no mountain elevations could take place where the sediments composing the area were thin; and that the mountain elevations were never equal to the vertical thickness of the strata composing them. I intended to imply that mountain elevation was due to sedimentary accumulation and subsequent continental elevation—trusting to the intelligence of my hearers and readers to interpret my suggestions.

From various sources giving the thickness, I stated that the maximum of the palæozoic sediments, entering into the formation of the Appalachian chain, was 40,000 feet. Perhaps it would be more prudent, as a basis of argument, to accept a medium and place the thickness at 25,000 feet (though the aggregate is much greater), out of which have come mountains of 5000 feet in height. It may not be easy to account for the manner in which the enormous erosion has been accomplished, for this could not have taken place beneath the sea, and the most natural explanation is that the eastern part of the continent has at some subsequent time been greatly elevated to allow of such erosion. I did not pretend to offer any new theory of elevation, nor to propound any principle as involved beyond what had been suggested by Babbage and Herschel. I did not propose to discuss the theory of the contraction of the globe from cooling, or of the crumpling of the earth's crust from the gradual cooling and shrinking of the interior mass, because such arguments are not always philosophical for want of a basis in facts, and are always unsatisfactory as giving a very inadequate solution of the problem. This question cannot be properly discussed in a note.

I am satisfied that a region where the ocean bed, during palæozoic time, subsided so as to permit a deposition, under water, of more than 25,000 feet of strata, is sufficiently unstable in character to come up again to an elevation required for the erosion of the anticlinal valleys. This great subsidence alone may indicate that the area was one of weakness and liable to elevation or depression according to the action of the forces.

During the long palæozoic time the area of subsidence was in the Appalachian region, though clearly enough, during some portion of that time great uplifting occurred on the northeast, to be succeeded by subsidence which may have been equal to the elevation. Why could not the area of subsidence be changed from the Appalachian region to the ocean on the east? Subsidence in one locality means a corresponding, but not necessarily equal, elevation elsewhere; so while the ocean bed was subsiding may not the Appalachians have risen?

PAGE 57. See the introduction to Palæontology of New York, Vol. III.

PAGE 59. I have shown the existence of a similar condition in Iowa, where the erosion of the Devonian and Silurian surfaces has been covered by coal measures, preserving little evidence of the eroded material. In many instances of this kind deep depressions of considerable area were worn in the older formations, which were filled up by the underclays of the coal measures and often followed by the deposition of the coal above. (See Geological Report of Iowa, pp. 120–141, 1858.)

PAGE 61. Very recently Mr. Gilbert of the U. S. Geological Survey has discovered coal and coal measures in the far west, just enough to make an exception to the general condition here stated.

NOTES ON PROF. JAMES HALL'S ADDRESS.

BY T. STERRY HUNT, LL. D., F. R. S.

The present publication, after more than a quarter of a century has elapsed, of Prof. James Hall's Address before the American Association at the Montreal Meeting in 1857, is eminently fitting at the present time and the views therein expressed will be found for the most part to have lost none of their value, but to have been confirmed by the progress of geological science. It should here be mentioned that most of the important points in this Address were published by Prof. Hall in 1859, in the Introduction to the Third Volume of his great work on the Palæontology of New York, and were the subject of an extended notice by the present writer, in an Essay on Some Points in American Geology, in the American Journal of Science for May, 1861. It was here shown that the views of Prof. Hall, which when heard and read in 1857 and 1859 seemed to many untenable and evoked hostile criticism, were in harmony with those previously maintained by masters in the science of Geology. It is true that the elevation-hypothesis of Von Buch had been extended from volcanic mountains to ranges made up of aqueous sediments, like the Alps, the Pyrenees and the Alleghanies, and by the authority of Humboldt and Elie de Beaumont had been impressed upon the scientific world, but not without protest. Constant Prevost, Scrope and Lyell had opposed this famous hypothesis as applied to volcanic elevations, while de Montlosier in 1832, in the same spirit, declared that the great mountain chains of Europe are but the remains of continental elevations which have been cut away by denudation and that the foldings and inversions to be met with in the structure of mountains are to be looked upon only as local and accidental.

J. P. Lesley, again, in 1856, had asserted that continents are but congeries of mountains, or rather the latter are but fragments of continents, separated by valleys which represent the absence or removal of mountain-land; and again had said "mountains terminate where the rocks thin out."

Later, the present writer returned to the discussion of Prof. Hall's views in an Essay on Some Points in Dynamical Geology published in the American Journal of Science for April, 1873, and in part a criticism of Professor Joseph LeConte's paper which appeared in the same Journal in November and December, 1872, on the Formation of the Features of the Earth's Crust. I quote from my Essay the following passages, to show how far Hall's views were accepted by LeConte, and also for the reason that in reply to the adverse criticisms of LeConte and others, I have here stated what I conceive to be the great and distinguishing merits of Professor Hall's contribution to the theory of mountains first developed in his remarkable Address of 1857.

"The views of Professor Hall as to the relation between great accumulations of strata and mountain elevations, are cited with approval by LeConte, who, following him, asserts that 'mountain chains are masses of immensely thick sediments.' I venture, however, to remark in this connection that the views both of Mr. Hall, and of myself, as his expounder, have as yet been but imperfectly understood either by LeConte or our other critics. Thus, they have been defined as a theory of mountains with the origin of mountains left out, while LeConte says 'Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain-formation has taken place.' Now in fact neither Hall nor yet myself in my exposition of his views [in the Essay of 1861 cited above] has proposed any theory to explain this part of the process; that is to say, the uplifting of the deposited sediments, which LeConte calls 'the actual mountain-formation.' Hall's contribution to the problem, which, as LeConte well says, forms 'an era in geological science,' was to show the relation between mountain-chains and great accumulations of sediments; to illustrate this by the history of the palæozoic rocks of North America; and, moreover, to protest against the generally received theory that mountain elevations are due to local upthrusts; he, to use his own language 'going back to the theories long since entertained by geologists relative to continental elevations.' That mountains were the remains of eroded continental areas had already been taught by Lesley, and long before by de Montlosier. It was left for Hall through a new way to lead us back to these views, but the whole theory of the cause of continental elevation was left by him where he found it. In my exposition of his views, I have only endeavored in addition to show in what manner a contracting globe and a solid nucleus may be related to the great facts of local subsidence and accumulation."

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"Professor LeConte declares that an important problem in geological dynamics remains in his opinion, unsolved, namely the cause of the 'great and widespread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata,' the last being that of the post-pliocene period. *Now*, it is precisely the *upward* movements of this kind which constitute the continental elevations of de Montlosier, Hall and myself, giving rise to plateaus, and by the partial erosion and denudation of these, to mountains. The *cause* of these continental elevations was not discussed by Hall, and is by LeConte declared to be unexplained; while such is the case 'the actual mountain formation,' to use his words, is still unaccounted for. That these gentle and widespread movements of oscillation are, however, in some way not yet clearly explained, connected with the contracting of the nucleus, and the consequent conforming thereto of the envelope, we can scarcely doubt; or that the latter, from its nature and origin, must present great differences in constitution and in flexibility in its various parts. From this it might be expected that the movements imparted to the envelope alike by the process of secular cooling and contraction of the nucleus, and by the disturbance of the equilibrium of pressure consequent on the processes of erosion and sedimentation, would give rise to seemingly irregular oscillations, resulting in the depression or the elevation of considerable areas, constituting continental movements."

The two papers of the present writer will be found reproduced, the first in abstract, in his *Chemical and Geological Essays*, pp. 59–68, and 70–79. In the latter he has adverted to the view adopted by Hall in his Address, that the crystalline rocks of New England, including the Green Mountains and the White Mountains, are altered palæozoic strata. This view which had been advocated by Mather, the brothers Rogers (though subsequently rejected by them), by Logan and others, was at that time accepted by the present writer; who has, however, long since rejected it as untenable, and maintained the prepalæozoic age of these mountains, and generally of the crystalline rocks of the Atlantic belt, a view which is now shared by most geologists. It is hardly necessary to say that the acceptance or rejection of this hypothesis of the palæozoic age of the crystalline rocks in question in no way affects Professor Hall's argument. The present view of the writer, now maintained by LeConte and most American Geologists, makes the rocks in question a part of the great prepalæozoic continental area lying to the north and east which Professor Hall supposed to be the source of the palæozoic sediments of the Appalachian basin.

SECTION A.

MATHEMATICS AND ASTRONOMY.

ADDRESS

BY

WILLIAM HARKNESS,

VICE PRESIDENT, SECTION A.

MEMBERS OF THE SECTION OF MATHEMATICS AND ASTRONOMY:—

TRANSITS of Venus usually occur in pairs; the two transits of a pair being separated by only eight years, but between the nearest transits of consecutive pairs more than a century elapses. We are now on the eve of the second transit of a pair, after which there will be no other till the twenty-first century of our era has dawned upon the earth, and the June flowers are blooming in 2004. When the last transit season occurred the intellectual world was awakening from the slumber of ages, and that wondrous scientific activity which has led to our present advanced knowledge was just beginning. What will be the state of science when the next transit season arrives God only knows. Not even our children's children will live to take part in the astronomy of that day. As for ourselves, we have to do with the present, and it seems a fitting occasion for noticing briefly the scientific history of past transits, and the plans for observing the one soon to happen.

When the Ptolemaic theory of the solar system was in vogue, astronomers correctly believed Venus and Mercury to be situated between the Earth and the Sun, but as these planets were supposed to shine by their own light, there was no reason to anticipate that they would be visible during a transit, if indeed a transit should occur. Yet, singularly enough, so far back as April, 807, Mercury is recorded to have been seen as a dark spot upon the face of the Sun. We now know that it is much too small to be visible to the naked eye in that position, and the object observed could have been nothing else than a large sun spot. Upon the establishment of the Copernican theory it was immediately perceived that transits of the inferior planets across the face of the Sun must occur, and the recognition of the value of transits of Venus for determining the solar parallax was not long in following. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks; but Halley was first to work it out completely, and to him is usually assigned the honor of the invention. His paper, published in 1716, was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus of 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained.

When Kepler had finished his Rudolphine tables they furnished the means of predicting the places of the planets with some approach to accuracy; and in 1627 he announced that Mercury would cross the face of the Sun on November 7, 1631, and Venus on December 6 of the same year. The intense interest with which Gassendi prepared to observe these transits can be imagined when it is remembered that hitherto no such phenomena had ever greeted mortal eyes. He was destitute of what would now be regarded as the commonest instruments. The invention of telescopes was only twenty years old, and a reasonably good clock had never been constructed. His observatory was situated in Paris, and its appliances were of the most primitive kind. By admitting the solar rays into a darkened room through a small round hole, an image of the Sun nine or ten inches in diameter was obtained upon a white screen. For the measurement of position angles a carefully divided circle was traced upon this screen, and the whole was so arranged that the circle could be made to coincide accurately with the image of the Sun. To determine the times of ingress and egress, an assistant was stationed outside with a large quadrant, and he was instructed to observe the altitude of the sun whenever Gassendi stamped upon the floor. Modern astronomical predictions can be trusted within a minute or two, but so great did the uncertainty of Kepler's tables seem to Gassendi that he began to watch for the expected transit of Mercury two whole days before the time set for its occurrence. On the 5th of November it rained, and on the 6th clouds covered the sky almost all day. The morning of the 7th broke, and yet there was no respite from the gloomy pall. Gassendi continued his weary watch with the sickening dread that the transit might already be over. A little before eight o'clock the sun began to struggle through the clouds, but mist prevented any satisfactory observation for nearly another hour. Towards nine the sun became distinctly visible, and turning to its image on the screen, the astronomer observed a small black spot upon it. It was not half as large as he expected, and he could not believe it was Mercury. He took it for a sun spot, and carefully estimated its position at nine o'clock, so that he might use it as a point of reference for the planet, if indeed he should be fortunate enough to witness the transit. A little later he was surprised to see the spot had moved. Although the motion was too rapid for an ordinary sun spot, the small size of the object seemed to forbid the idea that it was Mercury. Besides, the predicted time of the transit had not yet arrived. Gassendi was still uncertain respecting the true nature of the phenomenon when the sun again burst through the clouds and it was apparent that the spot was steadily moving from its original position. All doubt vanished, and recognizing that the transit, so patiently watched for, was actually in progress, he stamped upon the floor as a signal for his assistant to note the sun's altitude. That faithless man, whose name has been forgotten by history, had deserted his post, and Gassendi continued his observations alone. Fortunately the assistant returned soon enough to aid in determining the instant of egress, and thus an important addition was made to our knowledge of the motions of the innermost planet of the solar system.

After this success in observing Mercury, Gassendi hoped he might be equally fortunate in observing the transit of Venus on December 6, 1631. He knew that Kepler had assigned a time near sunset for first contact, but the tables were not sufficiently exact to forbid the possibility of the whole transit being visible at Paris. Alas, alas! these hopes were doomed to disappointment. A severe storm of wind and rain prevailed on December 4th and 5th, and although the sun was visible at intervals on the 6th and 7th, not a trace of the planet could be seen. We now know that the transit happened in the night between the 6th and 7th, and was wholly invisible at Paris.

Transits of Venus can occur only in June and December, and as the two transits of a pair always happen in the same month, if we start from a June transit the intervals between consecutive transits will be 8 years, 105½ years, 8 years, 121½ years, 8 years, 105½ years, and so on. This is the order which exists now, and will continue for many centuries to come, but it is not always so. The path of Venus across the sun is not the same in the two transits of a pair. For a pair of June transits, the path at the second one is sensibly parallel to, and about twenty minutes north of, that at the first; while for a pair of December transits the parallelism still holds, but the path at the second one is about twenty-five minutes south of that at the first. Hence it happens that whenever Venus passes within about four minutes of the sun's centre at a June transit, or within about eight minutes at a December transit, she will pass just outside the sun's disk at the other transit of the pair, and it will fail. Thus the intervals between consecutive transits may be modified in various ways. If the first transit of a June pair fails, they will become 129½ years, 105½ years, 8 years, 129½ years, etc. If the second transit of a June pair fails, they will become 113½ years, 8 years, 121½ years, 113½ years, etc. If the first transit of a December pair fails, they will become 8 years, 113½ years, 121½ years, 8 years, etc. If the second transit of a December pair fails, they will become 8 years, 105½ years, 129½ years, 8 years, etc. And finally, if either the first or second transit of a pair fails both in June and December, they will become 113½ years, 129½ years, 113½ years, 129½ years, etc.

When Kepler predicted the transit of 1631, he found from his tables that at her inferior conjunction on December 4, 1639, Venus would pass just south of the sun, and therefore he believed the second transit of the pair would fail. On the other hand, the tables of the Belgian astronomer, Lansberg, indicated that the northern part of the sun's disk would be traversed by the planet. In the fall of 1639 this discrepancy was investigated by Jeremiah Horrocks, a young curate only twenty years old, living in the obscure village of Hoole, fifteen miles north of Liverpool, and he found, apparently from his own observations, that although

Kepler's tables were far more accurate than Lansberg's, the path of the planet would really be a little north of that assigned by Kepler, and a transit over the southern portion of the sun would occur. He communicated this discovery to his friend William Crabtree, and these two ardent astronomers were the only ones who had the good fortune to witness this, the first recorded transit of Venus.

Horrocks had great confidence in his corrected ephemeris of Venus, and it forbade him to expect the ingress of the planet upon the sun before three o'clock in the afternoon of Sunday, November 24, old style (December 4, new style); but as other astronomers assigned a date some hours earlier, he took the precaution to begin his observations on the 23rd. The 24th seems to have been partially cloudy, but he watched carefully from sunrise to nine o'clock; from a little before ten until noon; and at one o'clock in the afternoon; having been called away in the interval by business of the highest importance—presumably the celebration of divine service. About fifteen minutes past three he was again at liberty, and as the clouds had dispersed, he returned to his telescope and was rejoiced to find Venus upon the sun's disk, second contact having just happened. Only thirty-five minutes remained before sunset, but during these precious moments he made determinations of the position of Venus which are even yet of the highest value. Crabtree was less fortunate. At his station, near Manchester, there was but a momentary break in the clouds a quarter of an hour before sunset. This sufficed to give him a glimpse of the transit, and he afterwards made a sketch from memory.

The years sped swiftly by, and as the transit of 1761 approached, Halley's paper of 1716 was not forgotton, although he himself had long been gathered to his fathers. In deciding to what extent his plans could be followed, it was first of all necessary to know how nearly the real conditions would approximate to those he had anticipated. Passing over a paper by Trébuchet calling attention to errors in Halley's data, Delisle was first to point out the exact conditions of the transit, and the circumstances upon which the success of the observations would depend. In August, 1760, less than a year before the event, he published a chart showing that inaccurate tables of Venus had misled Halley, both as to the availability of his method, and in the selection of stations. The occasion could be more effectively utilized by a change of plan, and Delisle considered it best to observe at suitably selected localities from many of which only the ingress, or only the egress, would be visible. Ferguson, in England, seems to have arrived independently at similar conclusions.

The two methods proposed respectively by Halley and Delisle have played so important a part in the history of physical astronomy that it will not be amiss to state briefly the distinction between them. The sun causes Venus to cast a shadow which has the form of a gigantic cone, its apex resting upon the planet, and its diameter continually increasing as it recedes into space. All the phenomena of transits are produced by the passage of this shadow cone over the earth, and as each point of the cone corresponds to a particular phase of a transit, any given phase will encounter the earth, and first become visible, at some point where the sun is just setting; and will leave the earth, and therefore be last visible, at some point where the sun is just rising. Between these two points it will traverse nearly half the earth's circumference and in so doing will consume about twenty minutes. The only phases dealt with by either Halley's or Delisle's method are the external and internal contacts, both at ingress and at egress. Delisle's method consists in observing the times of contact at stations grouped about the regions where either ingress or egress is soonest and latest visible. The longitudes of the stations must be well determined, and then by combining them with the observed times of contact the rate at which the shadow cone sweeps over the earth becomes known, and from it the solar parallax results. At many of the stations best suited for Delisle's method, only the beginning or only the ending of the transit will be visible; but for the application of Halley's method, both the beginning and the ending must be seen. The theory of the latter method is so complicated that it is difficult to explain it briefly and at the same time accurately; but the following considerations will suffice to indicate its nature. The duration of a transit at any point on the earth's surface depends partly upon the length of path, and partly upon the velocity, of that point while within the shadow cone. The length of path is affected by the latitude of the point, and the velocity by the earth's diurnal motion, which in some regions accelerates, and in others retards, the progress of the shadow. The result is that throughout one-half the earth's surface the duration of the transit is lengthened, while throughout the other half it is shortened; the maximum lengthening and shortening occurring at the respective poles of the hemispheres in question. Although these poles are not situated at the extremities of the earth's axis, it usually happens that one of them is shrouded in night; but upon the sunlit side of the earth, from which alone observations can be made, localities may exist at some of which the duration of the transit will be twenty minutes or more greater than at others. This inequality is the condition upon which Halley's method depends, and when such localities are accessible it may be advantageously applied. Briefly then, Halley's method consists in observing the duration of a transit at two or more stations so selected as to give durations of widely different lengths; while Delisle's method consists in employing a common standard time to note the instant when the transit begins, or ends, at two or more stations so chosen as to give very different values for that instant.

The transit of 1761 was visible throughout Europe and was well observed by astronomers in all parts of that continent. Besides this, England sent expeditions to St. Helena and to the Cape of Good Hope; and English astronomers observed at Madras and Calcutta; French astronomers were sent to Tobolsk, Rodriguez, and Pondicherry; Russians to the confines of Tartary and China; and Swedes to Lapland. No less than 117 stations were occupied by 176 observers; and of these, 137 published their observations. When this mass of data was submitted to computation, the result was far from satisfactory. Values of the solar parallax were obtained ranging from 8.49 seconds to 10.10 seconds; and in their disappointment the astronomers of the eighteenth century concluded that too much reliance had been placed upon Delisle's method.

The transit of 1769 drew on apace; and, to avoid a repetition of the fancied mistake of 1761, attention was directed almost exclusively to Halley's method. The conditions of the transit were carefully discussed by Hornsby in England, and by Lalande and Pingré in France; and it was found that its duration would be greatest in Lapland and Kamschatka, and least in the Pacific Ocean, California and Mexico. Astronomers were dispatched to all these regions. England sent the famous Capt. Cook to Otaheite; France sent Chappe to California; the King of Denmark sent Father Hell to Lapland; and in addition numerous observations were made in Europe, North America, China, and the East Indies.

The preparations were most elaborate, and the result better than in 1761, but still not satisfactory. The black drop and other distortions disturbed the contacts in this transit as they had done in the previous one, and the values of the parallax deduced by the best computers ranged from 8.43 seconds to 8.85 seconds.

Thus the matter rested till 1825 and 1827 when Encke published abstracts of his discussion of the transits of 1761 and 1769, from which he deduced a parallax of 8.58 seconds. This discussion was not printed in full till 1835, when it immediately commanded the attention of astronomers, and its result, which Encke had modified to 8.57 seconds, was universally accepted for more than a quarter of a century. As time wore on, certain gravitational investigations led to a strong suspicion that the sun's distance had been overestimated by at least three million miles, and the observations of Mars at its opposition in 1862 converted this suspicion into a conviction. The eighteenth century transits were again rediscussed and a parallax of 8.83 seconds was found from them by Powalky in 1865, and 8.91 seconds by Mr. E. J. Stone in 1868. Newcomb's paper, in 1867, also produced a marked impression.

The transit of 1874 was then approaching, and in the discussion as to how it should be utilized Halley's and Delisle's methods once more played a prominent part. It was recognized that the uncertainty in the observed times of contact of the eighteenth century transits was largely due to the black drop, and the causes of that phenomenon were carefully considered. Among them, most astronomers believed that irradiation played an important, if not the principal, part; but at the same time there was a general feeling that the telescopes of a century ago were bad, and that the magnificent instruments of the present day would give better results. In view of all the circumstances it was determined that the contacts should be observed with equatorially-mounted achromatic telescopes of from 4 to 6 inches' aperture, or with reflectors of not less than 7 inches' aperture, and that magnifying powers of from 150 to 200 diameters should be employed. The Germans and Russians adopted heliometers of about three inches' aperture for making exact determinations of the positions of Venus during transit, but other nations did not follow their example.

Photography, an agency undreamed of in the eighteenth century, was also available, and all saw the desirability of employing it; but there was much difference of opinion as to how this should be done. The European astronomers preferred instruments modelled upon the Kew photoheliograph, whose objective has 3.4 inches aperture and 50 inches focus, giving an image of the sun 0.482 of an inch in diameter, which is enlarged by a secondary magnifier to 3.93 inches. On the other hand, the American astronomers contended that photographs taken with such instruments would be affected by troublesome errors due to the secondary magnifier, that position angles could not be measured from them accurately enough to be of any use, and that it would be exceedingly difficult to determine the exact linear value of a second of arc. They advocated the use of horizontal photoheliographs, which are free from all these disadvantages; and the instruments which they adopted had apertures of 5 inches, and focal distances of 38½ feet, giving images of the sun slightly more than 4 inches in diameter. Notwithstanding this radical difference of opinion respecting the best form of photoheliograph, the astronomers of the old and new worlds were in perfect accord as to how the instruments should be employed. Between the first and second contacts, and again between the third and fourth contacts, photographs about five minutes square, showing the indentation cut by the planet into the sun's limb, were to be taken at intervals of a few seconds; and from these it was hoped the true times of contact could be deduced with great accuracy. Between the second and third contacts, pictures of the entire sun were to be taken at short intervals, and the positions of Venus relatively to the sun's centre were to be obtained from them by subsequent measurements. In the latter case, the photoheliograph took the place of a heliometer, and was superior to that instrument in its power of rapidly accumulating data.

The question of instrumental outfit having been disposed of, stations were selected, and parties dispatched to almost every available point. The United States, England, France, Germany, Russia, Holland,—in short, nearly all the nations of the civilized world,—took part in the operations. The weather was not altogether propitious on the day of the transit, but nevertheless a mass of data was accumulated which will require years for its thorough discussion. When the parties returned home the contact observations were first attacked, but it was soon found that they were little better than those of the eighteenth century. The black drop, and the atmospheres of Venus and the Earth, had again produced a series of complicated phenomena, extending over many seconds of time, from among which it was extremely difficult to pick out the true contact. It was uncertain whether or not different observers had really recorded the same phase, and in every case that question had to be decided before the observations could be used. Thus it came about that within certain rather wide limits the resulting parallax was unavoidably dependent upon the judgment of the computer, and to that extent was mere guesswork. Attention was next directed to the photographs, and soon it began to be whispered about that those taken by European astronomers were a failure. Even yet I am not aware that the Germans have published anything official on the subject; but the English official report has appeared, and it frankly declares that "after laborious measures and calculations it was thought best to abstain from publishing the results of the photographic measures as comparable with those deduced from telescopic view." From the way in which these photographs were taken, Sir George Airy saw that they could not yield position angles of any value, and therefore differences of right ascension and declination could not be determined from them; but they did seem capable of giving the distance between the centres of Venus and the sun with considerable accuracy. Upon trial this proved not to be the case. No two persons could measure them alike, because "however well the sun's limb on the photograph appeared to the naked eye to be defined, yet on applying to it a microscope it became indistinct and untraceable, and when the sharp wire of the micrometer was placed on it, it entirely disappeared." In short, the British photographs are useless for the present, but Sir George Airy hopes that in the future some astronomer may be found who will be capable of dealing with them.

We turn now to the American photographs. They present a well defined image of the sun about 4.4 inches in diameter, and are intended to give both the position angle and distahce of Venus from the sun's centre. A special engine was at hand for measuring them, but when they were placed under the microscope only an indistinct blur could be seen. Here again was the same difficulty which had baffled the English, but fortunately its cause was soon discovered. The magnifying power of the microscope was only 37½ diameters, which seemed moderate enough, but was it really so? The photographic image of the sun was about 4.4 inches in diameter, and this was magnified 3.31 times by the objective of the microscope, thus giving an image 14.56 inches in diameter.

To yield an image of the same size, a telescopic objective would require a focus of about 1563 inches, and if the eye-piece of the microscope, which had an equivalent focus of 0.886 of an inch, were applied to it, a power of 1764 diameters would be produced. This then was the utterly preposterous power under which the image of the sun was seen when the photograph was viewed through the microscope,and no useful result could be expected from it. Means were immediately provided for reducing the power of the microscope to 5.41 diameters, and then the photograph seen through it appeared as the sun does when viewed through a telescope magnifying 255 diameters. After this change all difficulty vanished, and the photographs yielded excellent results. The measurements made upon them seem free from both constant and systematic errors, and the probable accidental error of a position of Venus depending upon two sets of readings made upon a single photograph is only 0.553 of a second of arc. To prevent misunderstanding it should be remarked that this statement applies only to pictures taken between second and third contact, and showing the entire sun. The small photographs taken between first and second contact, and again between third and fourth contact, proved of no value.

These investigations consumed much time, and before the result from the American photographs was generally known, an international convention of astronomers was held in Paris to consider how the transit of 1882 should be observed. The United States was not represented at this conference, and guided only by their own experience, the European astronomers declared that photography was a failure and should not be tried again. They knew that the contact methods are attended by difficulties which have hitherto proved insurmountable, but under the merciless pressure of necessity, they decided to try them once more. Unfettered by the action of the Paris Conference, the United States Transit of Venus Commission took a very different view of the case. Its members knew that the probable error of a contact observation is 0.15 of a second of arc, that there may always be a doubt as to the phase observed, and that a passing cloud may cause the loss of the transit. They also knew that the photographic method cannot be defeated by passing clouds, is not liable to any uncertainty of interpretation, seems to be free from systematic errors, and is so accurate that the result from a single negative has a probable error of only 0.55 of a second of arc. If the sun is visible for so much as six minutes between the second and third contacts, by using dry plates thirty-six negatives can be taken, and they will give as accurate a result as the observation of both internal contacts. These were the reasons which led the American Commission to regard photography as the most hopeful means of observation, and thus it happens that the astronomers of the old and new worlds differ radically respecting the best means of utilizing one of the most important astronomical events of the century. The Europeans condemn photography, and trust only to contacts and heliometers; the Americans observe contacts because it costs nothing to do so, but look to photography for the most valuable results.

In 1716, Halley thought that by the application of his method to the transit of 1761, the solar parallax could certainly be determined within the five hundredth part of its whole amount. Since then, three transits have come and gone, and the contact methods have failed to give half that accuracy. From the photographic method, as developed by the U. S. Transit of Venus Commission, we hope better things, and perhaps fifty years hence its results may be regarded as the most valuable of the present transit season. In 1874, as in 1761, exaggerated views prevailed respecting the value of transits of Venus, but no competent authority now supposes that the solar parallax can be settled by them alone. The masses of the Earth and Moon, the moon's parallactic inequality, the lunar equation of the earth, the constants of nutation and aberration, the velocity of light, and the light equation, must all be taken into account in determining the solar parallax, and it cannot be regarded as exactly known until the results obtained from trigonometrical, gravitational, and photo-tachymetrical, methods are in perfect harmony. It may be many years before this is attained, but meanwhile practical astronomy is not suffering. Its use of the solar parallax is mainly confined to the reduction of observations made at the surface of the earth to what they would have been if made at the Earth's centre; and for that, our present knowledge suffices. The real argument for expending so much money upon transits of Venus is that being an important factor in determining the solar parallax, their extreme rarity renders it unpardonable to neglect any opportunity of observing them. Let us do our whole duty in this matter that posterity may benefit by it, even as we have benefited by the labors of our predecessors.

PAPERS READ.

NEW VIEWS OF MR. GEORGE H. DARWIN'S THEORY OF THE EVOLUTION OF THE EARTH-MOON SYSTEM, CONSIDERED AS TO ITS BEARING ON THE QUESTION OF THE DURATION OF GEOLOGICAL TIME.

By SAMUEL HAUGHTON, of Dublin, Ireland.

IT has been tacitly assumed, even so far back as the times of Newton and Clairaut, that the earth and planets have passed through a liquid condition (owing to former great heat) before assuming the solid condition, which some of them, at least, now possess.

Laplace, in his nebular hypothesis, also assumes the former existence of this liquid condition, and it is openly asserted by all geologists who believe that the earth consists of a solid crust (more or less thick), reposing upon a fluid or viscous nucleus.

It has been proved by Sir William Thomson, following out the views of the late Mr. Hopkins, that the present condition of the earth, taken as a whole, is such that it must be regarded as being more rigid than glass or steel, possibly more rigid than any terrestrial substance under the surface conditions of pressure.

The following considerations show that it may be fairly doubted whether the earth or any other planet ever existed in a fluid condition.

1. The possibility of the equilibrium of the rings of Saturn, on the supposition that they are either solid or liquid, has been more than doubted, and the most probable hypothesis respecting them is, that they consist of swarms of discrete meteoric stones.

2. It is difficult to understand the low specific gravity of Jupiter and the other outer planets, on the supposition that they are either solid or liquid, for we know of no substance light enough to form them.[1] If the outer planets consist of discrete meteoric

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[1] The force of this argument could not be felt before the revelations of the spectroscope, because at that time there was no proof that the whole universe was composed of the same simple substances, and those very limited in number.

stones moving around a solid or liquid nucleus, the difficulty respecting their specific gravity would disappear.

3. The recent researches connecting the November, the August, and other periodic swarms of shooting stars with comets tend in the direction of showing that comets, in cooling, break up into discrete solid particles (each no doubt having passed through the liquid condition); and that probably the solar nebula cooled in like manner into separate fiery tears, which soon solidified by radiation into the cold of space.

4. Mr. Huggins's recent comparisons of the spectroscopic appearances of comets and incandescent portions of meteoric stones, showing the presence in both of hydrocarbon and nitrogen compounds, confirm the conclusions drawn from the identity of the paths of comets and meteoric periodic shooting stars.

5. Mr. H. A. Newton, in a remarkable paper read before the Sheffield Meeting of the British Association (1879), showed the possibility (if not probability) of the asteroids being extinct comets, captured and brought into the solar system by the attraction of some one or other of the outer large planets, and permanently confined in the space between Mars and Jupiter, which is the only prison cell in the solar system large enough to hold permanently such disorderly wanderers.

In the same paper, Professor Newton threw out the idea that some of the satellites of the large planets might also be of cometary origin.

From all these and other considerations, it is therefore allowable to suppose that the earth and moon, when they separated from the solar nebula, did so as a swarm of solid meteoric stones, each of them having the temperature of interstellar space; *i. e.*, something not much warmer than 460° F. below the freezing point of water.

Mr. George H. Darwin has shown, admirably, how the earth-moon system may have been developed from the time when the earth-moon formed one planet revolving on its axis in a few hours, to the present time, when the earth and moon (in consequence of tidal friction) have pushed each other asunder to a distance of sixty times the radius of the earth.[2]

In his paper on the tidal friction of a planet[3] (supposed viscous and under the influence of bodily tides caused in it by an external body such as the moon), Mr. Darwin has found a remarkable equation of condition, which may be thus expressed:

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[2] Proceedings of Royal Society, 19th June, 1879.

[3] Phil. Trans., 1881, Part ii, p. 494.

$$d(\sqrt{r}) \propto \frac{\Psi dt}{r^6} \qquad (1)$$

where r = distance between centres of earth and moon.
t = time elapsed from a fixed epoch.

$$\Psi = \frac{p(n-\Omega)}{1+p^2(n-\Omega)^2} \qquad (2)$$

n = angular velocity of earth's rotation.
Ω = angular velocity of moon's orbital revolution.
p = quantity varying inversely as the viscosity of the planet.

The extreme interest of equation (1) consists in the appearance of the inverse sixth power of the distance.

As the function Ψ varies very slowly, we find by integration, for any portion of time during which Ψ may be regarded as constant

$$t = \mathrm{A}r^{\frac{13}{2}} + \mathrm{B}, \qquad (3)$$

a most unexpected and remarkable result.

Upon reading Mr. Darwin's papers, my mind turned to a problem with which I was familiar, viz.: the retardation of the earth's rotation produced by the lunisolar tide exerted upon the ocean supposed collected in an equatorial canal, the moon and sun having no declination, and I readily found an equation to express the evolution of the earth-moon system, on the foregoing hypothesis as to friction.

This equation is the following:

$$d(\sqrt{r}) \propto \frac{f\Phi dt}{r^6} \qquad (4)$$

where

$$\Phi = \frac{\mathrm{V}_0(n-\Omega)}{\{4\mathrm{V}_0^2(n-\Omega)^2-k^2\}\sqrt{4(n-\Omega)^2+f^2}} \qquad (5)$$

f = coëfficient of friction supposed proportional to relative velocity.
k varies inversely as r^3.
V_0 = velocity at earth's equator.

This leads, as in Mr. Darwin's hypothesis of viscous earth, to the integral

$$t = \mathrm{A}'r^{\frac{13}{2}} + \mathrm{B}' \qquad (6)$$

The form of the functions Ψ and Φ is similar, as both ascend by odd powers of $(n-\Omega)$ and vanish when $n=\Omega$, that is to say, at the beginning and end of the evolution by friction of the earth-moon system.

It is quite clear, therefore, that the remarkable expression (1) found by Mr. Darwin, is not peculiar to his special hypothesis of a viscous earth, but can be deduced equally well from the totally distinct hypothesis of an absolutely rigid earth retarded by the tidal action of a liquid ocean.

I was led by this result to consider the case of the earth-moon separating (as I believe they did) from the central solar mass, in the form of a swarm of discrete masses of meteoric iron and stone, each one having the temperature of the cold of interstellar space, or not much above it. Translating this conception into mathematical language, I find that the equation of continuity belonging to the hydrodynamical theory applies equally well to the meteoric theory, viz.:

$$vy=v'y' \tag{7}$$

where v, v' are the velocities at any two points, and y, y' are the depths of the ocean or meteoric swarm at the same points.

The depth of the swarm or ocean, without jostling or friction will be least under the moon, and greatest at right angles to the moon, and the velocities will be inversely. Hence the chances of jostling among the meteorites, when disturbed by the moon's tidal action will be proportional to the velocity, being greatest where the velocity is greatest and the area of passage least, and *vice versa*.

This consideration reduces the meteoric problem to that of the hydrodynamical problem, with a friction proportional to the velocity, and gives equations, in all respects similar to those derived by Mr. Darwin, from the hypothesis of a viscous earth.

On the meteoric hypothesis, if the jostling of the stones be slow they may cool almost as fast as they are heated and the result will be a cool earth and almost indefinite time at the disposal of geologists.

THE PARALLAX OF α LYRÆ AND 61 CYGNI. By ASAPH HALL, of Washington, D. C.

THE question of the parallaxes of the fixed stars, or their distances from the earth, was for a long time a subject of speculation among the ancient astronomers. Very little, however, could be done towards answering this question until the true theory of our solar system had been discovered, and the invention of the telescope had made possible more accurate astronomical observations. Even when these things had been done there remained other questions to be solved, such as those of aberration and refraction, before astronomers could reduce their observations correctly and bring the problem of stellar parallax to the test of exact measurement. All these reductions are easy to us now, but the astronomer who lived two hundred years ago found himself in a maze of perplexities, and nearly a century passed away before these difficulties were overcome so far as to make feasible the determination of the distances of the stars.

When it was seen that the motion of the planets around the sun is undoubtedly the true theory of our solar system, and that we have a long base line to measure from, astronomers were not slow to undertake observations for stellar parallax. One of the earliest observers was Rothmann, astronomer for William of Hesse, in Germany. Rothmann found a difference of 2′ in the latitude of his observatory from observations made in summer and in winter, and hence he inferred that the stars have an annual parallax, which confirmed the Copernican theory that the earth is in motion around the sun. But Tycho Brahe, the best observer of those times, declared that his own observations, made with the best instruments and with the greatest care, showed no such change of latitude; and hence Tycho drew the conclusion that the stars have no parallax, and that the theory of the motion of the earth around the sun is false. One of the most ingenious early attempts to investigate stellar parallax was that of Dr. Robert Hooke of London in 1669; but Hooke seems to have been a man of unsteady purpose, and failed to carry out his plan of observation, so that his work came to nothing. The next remarkable attempt was by the English astronomer, Flamsteed, in 1689–1697. Flamsteed made very careful determinations of the zenith distances of Polaris at its upper and lower culminations at different times of the year, and correcting these for precession he found that the results showed a periodical change, which he ascribed to parallax. But as soon as Flamsteed published his observations, it was pointed out by Cassini and others that the periodical changes could not be explained by a parallax of the star. It is curious, however, that no serious attempt was made to account for Flamsteed's results, the most careful and accurate that had been obtained up to his time. These results contain, in fact, the data for a very good determination of the constant of aberration, as was shown by C. A. F. Peters in 1846, and the value of this constant found from Flamsteed's observations is in error only a quarter of a second of arc.

Another generation of astronomers continued their attempts at the determination of stellar parallax, from observations of the zenith distances of the stars, but with no results. Many of these observations show periodical changes, which gave rise to various theories; but probably all these changes may be explained by variations in the instruments, or by incomplete reduction of the observations. In fact, no trustworthy determinations of parallax were possible until after the discovery of the aberration of light; and Bradley's discovery of this phenomenon, in 1728, put an end to most of the crude attempts. The coëfficient of aberration is 20.″4; and although the correction for aberration is zero at the time of the maximum of parallax, yet the motion of the earth in a single day will introduce a correction for aberration amounting to one-third of a second of arc, a quantity comparable with the parallaxes of the stars. Bradley was one of the few astronomers of the past century who knew the value of accurate and definite observations, and from his own work he drew the conclusion that the parallaxes of the stars must be less than a second of arc. Bradley's labors seem to have shown astronomers the great practical difficulty of this problem of stellar parallax, and few further attempts were made until toward the beginning of the present century when the instruments had been much improved.

The Italian astronomer, Piazzi, in 1792–1804, found parallaxes of several of the bright stars, amounting in some cases to 3″ or 4″, but his results are certainly wrong. About this time Brinkley, the astronomer Royal of Ireland, devoted fourteen years of careful and intelligent observation to the question of stellar parallax. This is one of the most remarkable researches of the kind ever made. The values of parallax found by Brinkley amount in some cases to 2″; and his work led to a long dispute between himself and Pond, the astronomer Royal at Greenwich. Although Brinkley was a skilful astronomer, and a man whom no amount of labor could deter, yet we must say that his results for parallax are mostly erroneous. However, the great difficulty of dealing with this question, by means of absolute measures made with divided circles, seems to have been brought out clearly by Brinkley's observations and their subsequent criticism and discussion.

The most elaborate and successful attempt that I know of to determine the parallaxes of the stars by such absolute measurements is that made by C. A. F. Peters at Pulkowa in 1842–'43. In the care and skill of observation, and completeness of reduction, this work has never been surpassed. Peters finds positive values of the parallaxes of seven of the eight stars observed, and in the case of the single negative parallax the value is less than a tenth of a second. The parallax of 1830 Groombridge found by Peters is $0''.226 \pm 0''.141$; a value much less than that found by a differential method, and subsequent investigations have proved the correctness of Peters' result.

The absolute determination of so small a quantity as a stellar parallax is an extremely difficult matter, since the observations must be extended over a year, during which the instrument is exposed to great changes of temperature which act on the circle, and which may influence the reductions. The divided circle may be avoided by using a transit instrument in the prime vertical, although the field of observation is in this way restricted to fewer stars. If Struve's method of reversing the instrument in each vertical be used, and stars are chosen that pass near the zenith, two of the instrumental corrections are almost wholly eliminated; but the third, or level correction, comes in with nearly its full value. With the excellent spirit levels made by Repsold, it was found by Struve that the probable error from this source was only a few hundredths of a second of arc. For stars of the first and second magnitudes that can be observed at all seasons of the year, this instrument promises good results, and in the absolute determinations of parallax, and in the investigations of the constants of aberration and nutation, it may yet fulfil the expectations of Struve.

It was long ago pointed out by Galileo, that if two stars are near each other, and one of them is bright and the other faint, probably these stars are at greatly different distances from us; and that differential observations of such stars would lead to a knowledge of their difference of parallax. Huygens applied this method to some of the binary systems, and of course found no parallax. A century afterwards Herschel also applied this method to some of the binary stars, and again no parallax was found; but by continuing his observations Herschel found that many of these stars were in motion around each other, and recognizing that such stars must be at nearly the same distance from us, he saw that no parallax could be found from such observations. In some respects, however, this method of Galileo presents great advantages. The observations being merely differential they can be made with micrometers of various kinds, and with greater accuracy and ease than absolute determinations. The reductions are also far simpler and more easily computed, and we avoid in a good degree the effects produced by changes of temperature. It is true that by this method we get only the difference of the parallaxes of the stars, and there will always remain some doubt concerning the value of the real parallax. But if a number of different stars be used for comparison, and the same value of the parallax be found from the various stars, we may fairly conclude that the parallax belongs to the principal star. A practical difficulty is met with here in the fact that most of the stars that have large proper motions, and those that for other reasons are such as we would observe for parallax, have no suitable comparison stars within reach of the micrometers commonly in use; that is, if we follow the method of measuring angles of position and distance, which is perhaps the best. In some cases this difficulty may be avoided by observing only the difference of declination, when the coëfficient of parallax in declination is not too small, although by doing this we give up the advantage of an independent determination of the parallax from the angle of position.

In 1862 a series of observations of α Lyræ was begun at the U. S. Naval Observatory by Professor J. S. Hubbard, and this series was continued by Professor S. Newcomb and his assistants until April, 1867. Each complete observation consists of four transits over seven wires, the instrument being reversed in each vertical. This star passing only a quarter of a degree south of our zenith, the correction for azimuth is nearly zero, and the collimation being eliminated by the method of observing, there remained only the correction for the level, which was determined by an excellent instrument made by Würdemann of Washington. This long and careful series of observations proved, however, of no value for determining the parallax of this star. Some unknown disturbance acted in such a way that the parallax comes out negative. This unfortunate result led me to make an attempt to find the parallax of this star by the differential method, while our observatory remains on its present site. These observations were begun May 24, 1880, and were finished July 2, 1881. As the famous star 61 Cygni could be observed without spending much more time, observations on this star were begun Oct. 24, 1880, and ended Dec. 7, 1881. These observations were the simplest possible, being measures of the difference of declination from a comparison star of the tenth magnitude. My observations of α Lyræ consist of two series, one with a bright field and dark wires, and the other with a dark field and bright wires; those of 61 Cygni were made with the dark wires only. Before beginning these measurements an arrangement was attached to the tube of the micrometer for adjusting the stellar focus, so that this adjustment could be deliberately and carefully made, and during the work care was taken that this adjustment should be always exact. In the case of our 26-inch refractor the tube of the micrometer is shifted one-thirtieth of an inch during the year, it being pushed in during the cold weather and drawn out in summer. It has been assumed in all our previous reductions that the coëfficient of temperature for the screw of our micrometer is zero. To test this assumption, which would have an important influence on the reductions, at the same time that the observations for parallax were going on a series of measurements was made on a pair of stars in the cluster *h* Persei. These measurements were not reduced until the observations for parallax were finished, and on account of the previous result of a zero coëfficient for temperature, the measurements were not so numerous as they might otherwise have been; still they show a decided correction for change of temperature.

The observations for parallax were arranged so that the micrometer and the wires were reversed on each night, and at the end of an observation the wires were restored to their first position. Each of these observations being reduced to a chosen epoch by correcting for proper motion, precession, nutation and aberration, gives an equation of condition of the form

$$x + by + cz + du + n = o,$$

where x is the correction to the assumed difference of declination, y the correction to the assumed value of the annual proper motion, z the difference of the constants of aberration for the two stars, u the difference of the parallaxes of the stars, and n is the residual between the observed difference of declination and an assumed value. The assignment of weights to such equations is a delicate matter, and whenever possible I have preferred to give the weight unity to each of them, notwithstanding the fact that the observations were made under very different conditions of the images. The solution of each set of equations by the method of least squares gives the following values of the parallax:

α Lyræ.

Dark wires, $\pi = 0''.1556 \pm 0''.00764.$
Bright " $\pi = 0.2080 \pm 0.00827.$

Taking the mean of these results by weight, we have the final result:

$\pi = 0''.1797 \pm 0''.00561.$
Time of light = 18.11 Julian years.

61 Cygni.

Dark wires, $\pi = 0''.4783 \pm 0''.01381.$
Time of light = 6.803 Julian years.

The substitution of these values in the equations of condition shows that the greatest residual in the case of α Lyræ is for the dark wires 0''.120; and for the bright wires it is 0''.160, there being 69 equations in the first case and 59 in the second. The observations of 61 Cygni are not quite so good, the greatest residual here among the 66 equations being 0''.209.

The star 61 Cygni has one of the largest and best determined parallaxes that we know of, since a recent investigation by Dr. Elkin has reduced that of α Centauri to nearly the same value. This quantity being half a second of arc, it can be seen easily with a good telescope. But a tenth of a second is so small that one can hardly distinguish it even with the highest magnifying powers, except with remarkably steady and distinct images. To determine a parallax of this amount we must trust therefore that in a large series of measurements the influence of the parallax will be shown in the mean result. And my own experience has convinced me that such will be the case, if the observer be careful, and has practised so long that his habit of observing will not change during the period of observation.

ON A METHOD OF REDUCING DIFFERENT CATALOGUES OF STARS TO A HOMOGENEOUS SYSTEM. By WILLIAM A. ROGERS, of Cambridge, Mass.

[ABSTRACT.]

WHEN two catalogues of stars which have been independently observed are compared, it will be found that there are certain systematic deviations *inter se* which are functions either of the right ascension or of the declination, or of both the right ascension and the declination. If all the stars in the two catalogues are compared for each hour of right ascension and the mean of the deviations for each hour be taken,' it will be found that this mean is never a constant. So also the mean of the residuals obtained by combining the stars in groups, arranged in the order of declination, will be found to vary with the declination.

Now our knowledge of the motion of the solar system in space depends upon our knowledge of the proper motions of the stars composing the stellar system. Since all the catalogues of stars which have ever been made, have variations from any normal system assumed, it is necessary to reduce each catalogue to a common system before the observations can be employed in the determination of the proper motion.

It is the common practice to arrange the stars in the order of right ascension and to take the mean of the residuals from the normal system chosen for each hour. Since these residuals involve the accidental errors of observation, it is then necessary to obtain a system of mean corrections which shall be as nearly free as possible from accidental errors. There are two methods of doing this—

I. The Analytical Method.

If we have a series of residuals $\Delta\alpha$ arranged in the order of right ascension, which are strictly circular functions, we may always assume:

$$\Delta\alpha = \text{a constant} + m \sin \alpha + n \cos \alpha + m' \sin 2\alpha + n' \cos 2\alpha \text{ etc.}$$

From the solution of a series of equations of this form the values of m n m' and n' can be found by the process of least squares. Hence the value of $\Delta\alpha$ becomes known for any right ascension.

II. The Graphical Method.

If we assume any given unit in right ascension as a horizontal argument, and an aliquot part of $\Delta\alpha$ as a vertical argument, it is obvious that points representing $\Delta\alpha$ may be laid off which will bear a definite relation to a fixed horizontal line. If a smooth curve be drawn through these points values of $\Delta\alpha$ nearly representing observation may be derived for any right ascension by reading off the vertical coördinate passing through the curve at this point.

Now when the systematic deviations of any catalogue from the normal system have been found by either of these methods and have been applied to the positions of the catalogue, it will still nearly always be found that there are remaining residuals which are functions of the declination. These residuals can be treated either analytically or graphically by the methods just described.

But when the chosen catalogue has been reduced to the normal system by the application of both of these classes of systematic corrections, *i. e.*, those depending on the right ascension, and those depending on the declination, obtained independently, there are several catalogues which still show a system of residuals which are functions of both the right ascension and of the declination.

It is easy to see why this must be so, to a certain extent. Nearly all modern catalogues depend upon Bessel's constants of precession. A few, however, depend upon Struve's constants.

In order to reduce the annual variation for Bessel's constants to those of Struve, we have:—

$$\text{Struve} = \text{Bessel} + .00112\overset{s}{3} + 00012\overset{s}{9} - \text{tang}\ \delta \sin \alpha$$

It will be seen that the variable part of this reduction depends upon both α and δ.

Both of the methods described for comparing different catalogues with a normal catalogue are open to the objection that the coördinates of any given star when referred in this way to a normal system, may differ by a small amount from the values of the coördinates derived by a direct reduction of the instrumental constants from the fundamental stars of the system. I estimate the uncertainty arising in this way to be about $\pm 0.015^s$ in $\Delta\alpha$ and $\pm 0.20''$ in $\Delta\delta$.

In the reduction of the zone stars observed at the Harvard College Observatory, the instrumental constants from 1871 to 1876 have been recomputed from the final positions of the fundamental catalogue upon which it is to be based, viz., upon the catalogue of fundamental stars in publication, the primary reductions having been made with the provisional places of these stars. When, therefore, the final catalogue is completed, the data will be at hand for a definite determination of the amount of this uncertainty. But since this reduction is entirely impracticable in the case of the catalogues already published finally, some method of reducing different catalogues to a homogenous system is an absolute necessity. Admitting the necessity—*that method is to be preferred which will reduce the residuals $\Delta\alpha$ and $\Delta\delta$ to a minimum, whatever the order or the limits of the groups into which they may be divided.*

The method proposed is as follows:—

(*a*) The residuals are first arranged in the order of declination. The mean values for any group represent nearly the corrections for the mean declination of that group, and for twelve hours of right ascension.

(*b*) The residuals for each group minus the mean value for that group, are then arranged in the order of right ascension, and a graphic curve is drawn through the points representing these values. Each curve will give a series of values for the even hours of right ascension, which are arranged in vertical columns, the horizontal argument being the mean declination. It will be seen that the values in the vertical columns are derived from the same curve, while those in the horizontal columns are derived from different curves. In order to connect the different groups in declination a curve is drawn through the points represented by the residuals in the horizontal columns. This will, of course, disturb to a slight extent, the values already found in the vertical columns, but they can in turn be rectified in the way already described. A second approximation will ordinarily give smooth curves for both the vertical and the horizontal arguments.

(*c*) On account of the limited number of residuals which usually compose the groups arranged in the order of declination, there is danger of introducing systematic errors in drawing the graphic curves. Hence, after the sum of the two residuals already found has been subtracted from the original residuals, the new values are arranged in the order of right ascension and a smooth curve is drawn through the points thus found, giving the means of obtaining the slight corrections which still remain.

This method has been followed in a paper printed in Vol. X of the Memoirs of the American Academy of Arts and Sciences on "A comparison of the Harvard College Observatory Catalogue of Stars for 1875.0 with the fundamental systems of Anwers, Safford, Boss and Newcomb."

ON THE PERFORMANCE OF A NEW FORM OF LEVEL INVENTED BY MR. JOHN CLARK OF THE U. S. COAST SURVEY. By WILLIAM A. ROGERS, of Cambridge, Mass.

[ABSTRACT.]

EXPERIENCED observers have often expressed the opinion that the level as ordinarily constructed cannot be regarded as an instrument of precision. It will be admitted on every hand that it is not made by a scientific method. In fact, the method of construction is of the most crude kind. The maker chooses a glass cylinder as uniform in thickness and density as he can obtain and proceeds to grind an interior curve of unknown radius with an emery and a polishing tool. He has no means of determining the exact radius by observation. He is unable to test the equality of the curvature on each side of the centre, until after the cylinder has been filled with a fluid and after the ends have been hermetically sealed. Even then the test is only a relative one. It is of the same kind as the observations which are made with the completed level.

As the result of many years of experience I have reached the conclusion that an astronomical level can be regarded as an instrument of precision, only when it is subjected to adequate tests during the series of observations in which it is employed. A given level may measure small angles with great precision, but the same level under different conditions may give untrustworthy results.

Even with an instrument which has no graduated circle, an adequate level trier can be constructed by placing a graduated scale at a known focal distance. In the case of the Meridian Circle of Harvard College Observatory the level is placed in Y's attached to the cube of the instrument placed parallel with the optical axis.

In order to show the necessity for repeated and continuous observations for the determination of the value of one division of the level, I record here my experience with the companion level of the instrument known as the Russian Transit. It was made in the workshop of the Pulkowa Observatory.

I began the observations about eight o'clock in the morning of Nov. 13, by comparing the readings of the circle with bubble first at the middle of the tube and then at the extreme end. Proceeding in this way with each five divisions in succession I was surprised to find, not only a continued diminution of the value of one division, but a well defined shifting of the zero of the level. By noon I had nearly completed the examination for the first half of the divisions. I then opened the shutters for an observation of the sun. After an interval of ten minutes, observations with the level were resumed when it was found that the value of one division, determined from the same space as before, had increased by one-fourth of its mean value. It will be sufficient to give in illustration the results of the observations on three days.

	1881 Nov. 13.	Nov. 14.	Nov. 15.
Shutters closed.	1 div = 1.76″ Th. = 38°	1 div = 1.20″ Th. = 40°	1 div = 2.06″ Th = 47°
Shutters open.	1 div = 2.08 Th. = 65°	1 div = 2.06″ Th. = 70°	1 div = 1.78″ Th = 36°

In these observations the level was held in position in the Y's with a light spring. A second series of observations was made with the tube mounted as follows: At the suggestion of Mr. George B. Clark, oval rings of brass were fitted loosely upon the tube and held in position with wax. These rings were then placed in

Y's one end being fastened with a spring clip while the other end was free. A third series was made with the tube fastened directly to the cube of the telescope at its neutral points by means of a hard cement. The results are given below.

SERIES I.		SERIES II.		SERIES III.	
1881 May 22	1 div = 2.90″	1881 Nov. 23	1 div = 1.63″	1881 Oct. 7	1 div = 2.25″
" 22	= 2.68			" 7	= 2.25
Sept. 1	= 2.60	" 25	= 2.48	" 8	= 1.64
" 7	= 1.83			" 8	= 1.62
" 7	= 1.92	" 29	= 2.18	" 8	= 1.65
" 8	= 2.83			" 9	= 1.62
" 9	= 2.74	1882 Jan. 8	= 1.80	" 9	= 1.59
" 12	= 3.42			" 12	= 1.71
" 13	= 2.89	" 12	= 1.94	" 13	= 0.83
				" 13	= 1.28
				" 13	= 1.17
				" 20	= 1.23
Mean	2.65	Mean	2.01	Mean	1.57

It is hardly necessary to say that this level has been discarded as worthless. On the other hand the mounted level furnished with the Russian Transit proves to be an excellent one.

A similar test with a level invented by Mr. John Clark has given good results. The level tube is supported upon centres which are attached to a plate which revolves freely upon another plate described by the inventor as a "reference plate." This "reference plate" is attached to the cube of the telescope by three adjusting screws. The following are the steps of an observation.

The telescope pointing north, the bubble is read for position east and position west. The telescope pointing south, the level is now on the under side of the cube. Revolving the tube upon its centres 180°, the bubble is read as before. This level therefore gives not only the inclination of the axis, but the ellipticity of the pivots.

This form of level is especially adapted to the easy determination of the value of one division. I give below the results of the determinations thus far made. The separate results are the results of observations on different days. They therefore involve the accidental errors of observation.

													Means.
1881.	Sept.	2.78″	2.71″	2.79″	2.80″	2.96″	2.87″	2.87″	3.07″	2.63″			2.83″
	Oct.	2.61	2.90	2.79	2.53								2.73
	Nov.	2.82	2.88	3.11	3.46	2.85	2.84	2.91	2.95	2.69	2.71	2.98	2.74
	Dec.	2.85	2.96	3.13									2.98
1882.	Feb.	3.21	2.97	2.69	2.77	2.99	2.62						2.87
	Mar.	2.73	3.21										2.97
	Apr.	2.92	2.80	3.01									2.91
	May	2.85	2.93										2.89
	June	2.87	3.01										2.94

The steadiness of the level of the instrument is something remarkable as is shown by the following separate observations which are here published by the permission of Professor Pickering the Director of the Observatory.

1882 Feb.	3	$b = +.78^{s}$	Apr.	3	$b = +.77^{s}$	June	25	$b = +.74^{s}$
"	5	$b = +.81$	"	14	$b = +.80$	July	3	$b = +.63$
"	14	$b = +.82$	"	24	$b = +.81$	"	10	$b = +.72$
"	23	$b = +.80$	May	1	$b = +.79$	"	31	$b = +.61$
"	26	$b = +.80$	"	15	$b = +.75$	Aug.	16	$b = +.71$
Mar.	5	$b = +.78$	"	29	$b = +.72$	"	25	$b = +.68$
"	14	$b = +.73$	June	11	$b = +.61$	Sept.	7	$b = +.66$
"	20	$b = +.74$	"	19	$b = +.66$	"	25	$b = +.61$
						Oct.	2	$b = +.65$

ON THE ARITHMETIC OF CHORDS. By C. P. HART, of Wyoming, Ohio.

[ABSTRACT.]

ANCIENT trigonometry, which gave birth to what is known as the "Arithmetic of Signs or Chords," though long superseded by the much more comprehensive and convenient processes and formulæ derived from the circular functions, developed by modern trigonometry, is nevertheless of considerable practical value in some trigonometrical calculations, especially when the usual logarithmic tables do not happen to be at hand. Indeed, in the solution of certain cases of plane oblique-angled triangles, the arithmetical solution by chords is as simple and expeditious as is that of plane right-angled triangles by the Pythagorian problem, from which the arithmetical relations were originally derived.

The author, in developing these relations, discussed the following formulæ, beginning with the most simple, and obtained definite expressions for both acute and obtuse-angled plane triangles — first, for every fifteen degrees, and finally for all angles whatsoever, the latter being the equivalent of the well-known trigonometrical expression, $AC^2 = BC^2 \pm 2 \cos. B \times BC. BA.$

FORMULÆ:

θ 60°. $BC^2 = AB^2 + AC^2 - AB. AC.$

θ 120°. $BC^2 = AB^2 + AC^2 + AB. AC.$

θ 45°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{2}.$

θ 135°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{2}.$

θ 30°. $BC^2 = AB^2 + AC^2 - AB. AC \times \sqrt{3}.$

θ 150°. $BC^2 = AB^2 + AC^2 + AB. AC \times \sqrt{3}.$

θ 15°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 165°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 + 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 75°. $BC^2 = AB^2 + AC^2 - AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

θ 105°. $BC^2 = AB^2 + AC^2 + AB. AC \times (2 - 3^{\frac{1}{2}})^{\frac{1}{2}}.$

GENERAL FORMULA

For all oblique-angled plane triangles: $BC^2 = AB^2 + AC^2 \pm AB. AC \times BB'$ (the chord of the *difference* between the contained angle and its supplement) of a circle whose radius is one.

CIRCULAR COÖRDINATES, AND COMPLEX ANHARMONIC RATIOS. By WM. WOOLSEY JOHNSON, of Annapolis, Maryland.

[ABSTRACT.]

THE *position ratio* of a point referred to two fixed points A and B being defined as the ratio AP : BP, AP and BP being *directed lines*, is a complex quantity which may be written

$$\rho e^{i\theta} = \rho (\cos \theta + i \sin \theta) = x + iy$$

ρ and θ, or x and y, may be taken as real coördinates of P; and it is shown that the loci of $\rho =$ constant, $\theta =$ constant, $x =$ constant, $y =$ constant are all circles. When B is removed to infinity, we have ordinary polar and rectangular coördinates. Substituting x for $1-x$ we have a symmetrical system of xy coördinates, in which it is shown that the equation of a curve is the same as that of its inverse in ordinary rectangular coördinates.

P may also be determined by the ratio of its position ratio to that of an assumed fixed point Q not on the line A B. This is a complex anharmonic ratio; when put in the form $\rho e^{i\theta} = x + iy$ the loci of $\rho =$ constant, etc., are still all circles.

The anharmonic ratio is real for four points situated on the same circle. In particular the case of harmonic ratio is considered and the general construction of harmonic conjugates is given. If two circles cut orthogonally, harmonic conjugates on one of them with respect to the points of intersection are on the same diameter of the other, and if three circles are mutually orthogonal their intersection forms three mutually conjugate pairs of points. The same construction determines points whose anharmonic ratio is i.

INVERSE ELLIPTIC FUNCTIONS AND THE IMAGINARY PERIOD. By WM. WOOLSEY JOHNSON, of Annapolis, Md.

[ABSTRACT.]

THE elliptic functions sn u cn u dn u, with their reciprocals and ratios, constitute twelve elliptic functions which, according to a notation proposed by Mr. J. W. L. Glaisher, are denoted by sn, cn, dn, ns, nc, nd, sc, sd, cs, cd, ds, dc. These may be regarded as the ratios of four quantities s, c, d, n, of which only the ratios are dealt with in homogeneous equations. The derivatives of the functions are expressed in this notation, and from them are deduced the values of the inverse functions expressed as integrals.

From the latter the usual expressions for the direct elliptic functions of an argument of the form $i\,v$ in terms of functions to the modulus k' are readily derived, and these are used to deduce the imaginary period of the elliptic functions.

Conservation of Solar Energy. By Pliny Earle Chase, of Haverford College, Pa.

[ABSTRACT.]

All forms of solar energy are due to solar radiation. The maintenance of the energy depends on the maintenance of the radiations. In investigating the relations of centripetal and centrifugal action and reaction, it seems desirable to consider the following hypotheses and conclusions:

1. Laplace's estimate that the velocity of transmission, in gravitating acceleration, if finite, is at least 100,000,000 times as great as the velocity of light.
2. Le Sage's hypothesis that gravitation and luminous radiation represent equal actions and reactions.
3. Faraday's search for a gravitating constant.
4. Herschel's comparison of the mean *vis viva* of light with that of sound.
5. Weber's identification of the velocity of light (v_λ), with the "electromagnetic ratio (v_e).
6. Berthelot's "explosive waves" and their action on waves of sound.
7. Siemens's inquiries into the combined influence of rotation, centrifugal action, gravitating fall, and chemical affinity.

To these considerations the following may be added:

8. If there is a natural unit of force, we may look for a natural unit of velocity.
9. Oscillations may be orbital, pendulous, or wave.
10. Different transformations of similar oscillations are harmonic.
11. Rotation may be regarded as a pendulous motion due to retarded and modified revolution.
12. The resemblance of Le Sage's theory to the kinetic theory of gases points to a probability that the natural unit of velocity is oscillatory. This probability is strengthened if we assume the existence of molecular and intermolecular elasticity.
13. In looking to the activities of the principal mass in our system, for indications of a natural unit of velocity, we find that gravitating velocities may be represented by gt.
14. In order that gt may be constant, t must vary inversely as g and, therefore, directly as r^2. This variation is found in the rota-

tion of a nebulous sphere, where it holds good for all stages of expansion or contraction which are not affected by external influence.

15. Gravitating acceleration should do its whole work in stellar rotation, as well as in planetary revolution.

16. Particles exposed to solar superficial gravitating acceleration during a single oscillation of half-rotation, would acquire a constant velocity which is equivalent to the velocity of light. If we designate this velocity by v_γ we have $v_\gamma = gt = v_\lambda$ as a gravitating constant, which gives the following extension to Weber's analogy; $v_\lambda = v_\epsilon = v_\gamma$. In other words, the unit of velocity which is indicated by solar rotation, is the same as is indicated by light and by electricity.

17. The velocity of light, like the velocity of sound, thus represents an elastic atmosphere whose height, if homogeneous, would be twice the virtual fall which would give the velocity in question, and whose elasticity is in harmonic accordance with solar rotation and planetary revolution.

18. Subsidence, from Laplace's limit of synchronous rotation and revolution to the poles, gives a mechanical equivalent of 76,000,000 J for each pound of subsiding matter. The spiral character of the subsidence produces solenoidal currents which may help to explain the equality of v_λ, v_ϵ and v_γ.

CORRECTION OF A PROBLEM IN "THE SYSTEM OF THE WORLD" BY SIR ISAAC NEWTON. By DE VOLSON WOOD, of Hoboken, N. J.

[ABSTRACT.]

It is stated "that two spheres of the same material as the earth, each one foot in diameter, placed in void space, ¼ of an inch from each other, would not come together by the force of their mutual attraction in less than a month's time."

I find that they would come together in 385 seconds, or in less than 6½ minutes.[1]

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[1] Some eminent scholars are of the opinion that the above named work was not written by Sir Isaac Newton, although his name is on the title page.

ON AN EXPERIMENTAL SOLUTION OF A PROBLEM IN THE DOCTRINE OF CHANCES. By T. C. MENDENHALL, of Columbus, Ohio.

[ABSTRACT.]

AT the meeting of the Association in 1879, the author had communicated a note upon a problem in chances attributed to Buffon, which contained the results of 12,000 trials. The problem is: If a thin stick of given length be thrown at random upon a plane surface covered with a system of equidistant parallel lines, what is the probability that it will lie across one of them? In the Proceedings of the Association for that year, a description of the apparatus and mode of making the experiment will be found.

This series of 12,000 trials was made by different individuals, and the author had resolved to carry out a more extended series alone in order to avoid a "mixing" of personal equations, if such should be found to exist. The number of trials made was 20,000. No reductions were made until the entire series was completed. At the meeting of the Association in 1881, the author had communicated the results of a reduction of about half of the total number of experiments and the results of the complete reduction were now placed before the Section. The theoretical probability involved as one of its factors π, the ratio of the circumference to the diameter of a circle. It was therefore possible to obtain a value of this constant from an experimental solution of the problem. The results of the reduction were, for convenience, put in this form showing the value of π at various stages of the experiment.

Reductions were made at the end of every one hundred throws thus giving two hundred values of this constant.

A chart was shown upon which these values were plotted together with a straight line drawn to show the true value.

The experimental curve for a time fluctuated above and below this line crossing it nine times. At the end of 7200 throws the experimental value approximates very closely to the true value. After 7900 it rises above the true value to which it does not again return. From this point until the end it gradually departs, with more or less irregularity, from the true value, and with a tendency apparently towards a constant value somewhat too great.

The final value of π is 3.180, a point which is almost constantly held during the last two thousand trials. This result seems to indicate something of the nature of a "personal equation" or bias. The only explanation for it seems to exist in the decision of doubtful cases. All cases were decided, no doubtful ones being recorded as such. The deviation from the theoretical law might be accounted for by supposing that in settling the doubtful cases there was an unconscious tendency to favor one side.

BELL ATTACHMENT FOR TELESCOPE CIRCLES. By HENRY M. PARKHURST, of New York, N. Y.

[ABSTRACT.]

THE bell is attached to the circles, and so arranged as to indicate when the telescope passes a designated right ascension or declination. The plan for a right ascension circle is described in the Science Observer. I have now constructed one for the declination circle, which I have here, together with photographs of the whole.

The main point is the mode of avoiding the uncertainty of position of the striking. A compound lever is so arranged that whichever way the telescope is moved, the acting portion of the lever will stand at an angle of 45°, resting upon and drawn over an arc, falling into depressions in that arc, causing the bell to sound when passing the centre of the depression, and consequently always at the same position.

The method proposed allows two strokes to come within 1° of each other, so that in sweeping in either direction the stroke will come exactly at the right point.

THE TWENTY-THREE INCH TELESCOPE OF THE HALSTED OBSERVATORY AT PRINCETON. By C. A. YOUNG, of Princeton, N. J.

THIS instrument, which has been erected in the Halsted Observatory at Princeton within the last few months, was made by Alvan Clark & Sons, of Cambridgeport, Mass. The contract, bearing date March 3, 1880, and providing that the telescope should be completed within two years after the receipt of the disks of glass from the makers, has been carried out fully and promptly in all respects.

The disks were cast by Feil of Paris; one of them (the flint) was on exhibition at the Philadelphia centennial.

The material is unusually excellent, almost perfectly free from air-bubbles and striæ, and much more transparent than the disks (by Chance) used in the construction of the object glasses of the Washington and McCormick telescopes.

The clear aperture of the lens is twenty-three inches, and the curves are approximately as follows, according to data furnished by the makers:

Crown,	radius of outside surface	+ 265.8	inches
	" " inner	— 81.9	"
Flint	" " surface next crown	— 73.4	"
	" " " " focus	+ 222.2	"

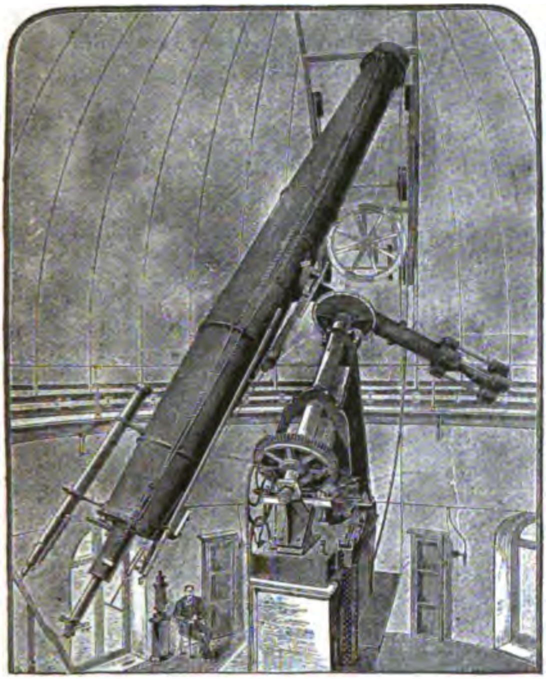
The + sign denotes that the centre of curvature is on the side of the lens next the eye, the crown being a double convex lens, with the external surface nearly flat, while the flint is a double concave, with the flatter surface next the eye.

The thickness of the crown lens is 1.35 inches
" " flint " " 0.62 "
The distance between the lenses is 7.61.

The effective focus of the lens, obtained by observing star transits over a scale ruled on glass, has been found to be 361.26 inches. This is nearly a foot greater than the distance from the outside surface of the object glass to the focus, showing that the *nodepunkt* is some distance in front of the lens and not between the two glasses.

The definition is admirable, the corrections both for spherical and chromatic aberration being excellent. The "color-curve" obtained by spectroscopic observation is substantially the same as that of Fraunhofer's glasses:— not quite so good as that of the Gauss object glass. The extreme range of focus between H and E amounts to just two inches.

The separation of the lenses effectually prevents the formation of any "ghosts" by reflection from the surfaces, and as the space between the lenses can easily be thrown open to the external air by removing the shutters which close the openings in the cell, the glass assumes the temperature of the external air very promptly. Another advantage gained is in making the aperture of the lens half an inch larger than it otherwise could be with the flint disk of given size.



THE PRINCETON TELESCOPE.

There has not yet been time to test the telescope very thoroughly by actual work, but on one or two nights of good seeing, such double stars as β Delphini, and ζ Sagittæ have been satisfactorily resolved;

and Mimas, the smallest of Saturn's satellites, has been seen not only at elongation, but very near conjunction also.

The mounting presents very few new features. It is simple and solid, considerably heavier, and better planned with respect to vibrations, than that of the Washington telescope.

The tube is cigar-shaped, of steel about $\frac{1}{16}$ of an inch thick.

At the lower end a heavy brass tube, nearly six feet long and seven inches in diameter, projects about two feet, and can be moved in and out over a range of two feet by a steel screw an inch in diameter which is concealed in the lower section of the steel tube between it and the brass tube. The screw acts upon a nut attached to the brass tube, and one revolution of the hand wheel, which turns the screw, slides the brass tube $\frac{1}{10}$ of an inch. At the end of the brass tube the tail-piece proper screws on, for carrying the micrometer, or eyepieces.

The diameter of the steel tube where it is attached to the declination axis is thirty inches. From the declination axis to the object-glass extremity is sixteen feet six inches: to the lower extremity of the steel tube, ten feet nine inches, and here the diameter is twenty inches.

The total length of the right ascension axis is about nine and one-half feet. Its diameter at the lower bearing is six inches; at the upper, eight inches where it rests on friction rollers. There are two hour-circles. One, at the upper end of the axis, thirty inches in diameter, carries a coarse graduation both on the face and edge. There are three pointers, so that by means of one or other of the three, the circle can be read from any point in the room.

The fine circle, twenty-eight inches in diameter, is at the lower end of the axis, and is read to single seconds of time by two opposite verniers, which are easily accessible from a platform upon the pier.

The declination axis is nine feet long; seven inches in diameter at the bearing nearest the telescope, and five and one-half at the other bearing. The declination circle, thirty inches in diameter, is at the end of the axis next the telescope. It carries a fine graduation (to 10′ of arc) on the face next the telescope, and a coarse graduation on the edge. The coarse reading is by means of a simple pointer, and is easily made from the floor, to within 10′,— quite accurately enough to bring any object into the finder.

The fine graduation is read by means of two long microscopes which come down to the eye end. The telescope tube carries two indices, each consisting simply of a fine line like the zero line of a vernier, and the micrometer at the eye end of the microscope gives the means of measuring the distance between this zero line and the nearest graduation mark; so that the accuracy of the reading is independent of the stability of the long microscopes. The reading is directly to 10″, and by estimation to single seconds.

The field of view of the microscope covers about 2½° of the graduation, and each degree is numbered. The illumination of the scale is on the principle of the collimating eyepiece, and is so excellent that the reading is very easy and accurate.

The clamps and tangent screws of both the right ascension and declination movements are managed from the eye end: the former by endless cords, the latter by Hooke's joint-handles.

There is an excellent apparatus for rapid motion in right ascension, by which the instrument can be reversed in less than half a minute.

The clockwork is very powerful, driven by a weight of about three hundred pounds with a fall of twelve feet in two hours. The regulator is on the same principle as that used by the Clarks on their chronographs, differing only in this respect that it is made very heavy (weighing over twenty pounds) and the weight is taken off the lower pivot of the vertical shaft by floating it in mercury. It runs very steadily, but seems to be somewhat affected by changes in temperature.

The instrument has two finders, one of five, the other of two inches diameter.

The tail piece of the telescope and of the five inch finder are both of the same size as that of the tail piece of the 9½ inch equatorial of the School of Science Observatory, so that all the eyepieces and minor apparatus belonging to either instrument can be used on any one of the three.

The sliding tubes of all the tail pieces are graduated, a great convenience in setting spectroscopic and other apparatus to focus.

The instrument is provided with a filar position micrometer, fitted with Burnham's illuminating apparatus.

It has also a double image micrometer, and a square-bar micrometer for comet observations. There is a large helioscope with polarizing reflectors, and an ordinary Herschel solar-eyepiece for the finder.

A very large and powerful direct-vision spectroscope by Hilger of London has been provided for stellar work.

It is upon the same plan as the Greenwich spectroscope (except that it is a little larger), containing three so-called "half prisms," and was constructed under the kind personal superintendence of Mr. Christie, the present Astronomer Royal, who first devised this peculiar form of prism. The spectroscope collimator is 2¼ inches aperture and 38 inches long. The view-telescope has the same aperture, but its focal length is much less. The instrument is over six feet long, and weighs about 150 lbs. with its appendages.

The telescope with its mounting weighs a little more than seven tons. It stands upon the top of a heavy stone pier of Quincy granite, the upper surface of which is 14 feet above the floor. The centre of motion of the telescope (at the intersection of the Right Ascension and Declination axes) is 20 feet, 6 inches above the floor.

The dome under which it is erected is of iron, 39 feet in internal diameter. It was built some fifteen years ago, and is not just what would be constructed now for the same purpose, requiring a good deal more power to turn it around, and to manage the shutter, than is the case with some built more recently:—the Vienna dome, for instance. All difficulties, however, have been overcome by placing a four-horse power gas-engine in the basement and making it do the work by means of proper gearing. It is possible to raise the shutter, turn the dome to any part of the sky, and have the telescope pointed upon any designated object within five minutes after entering the building—this includes the time required for starting the engine. The same engine drives a small Edison dynamo-machine, which supplies electric light to all parts of the building, and furnishes electric currents for spectroscopic work.

The cost of the telescope and spectroscope was $26,000, subscribed by friends of the College of New Jersey; Robert Bonner, of N. Y., R. L. Stuart, and the trustees of the John C. Green estate being the largest contributors.

ON THE LAW OF DISTRIBUTION FOR CERTAIN PLANT-NUMBERS. By JAMES EDWARD OLIVER, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

THE dimensions of any part of a plant; and also the number of flowers to a spike, of rays to a head in Compositæ, etc., when variable; have, each of them, for each species, under any given circumstances, some mean value; and I investigate, in a few typical cases, the law whereby those values of the number in question which are above or below the mean value are distributed.

I find that the number of flowers to the spike follows a law of distribution that differs but little from the normal law of distribution of errors of observation: *i. e.*, any value x has a frequency nearly proportional to the antilogarithm of $-Ax^2 \pm Bx^3 - Cx^4 \pm$ etc., wherein A, C are positive and the series convergent; as if the plant-number's deviation from the mean value were a sum of many small but sensible numbers having ambiguous signs independent of one another but dependent upon accidents of environment, ancestral history, etc.

I get a similar result concerning the *logarithm* of the length of a grass-head: indicating that the ratio of the length to a mean value is a product of many factors, each factor being a little more or less than unity as accident has determined independently of the others.

In the case of one Composita (*Leucanthemum vulgare*) I find that the distribution of the ray-numbers is strongly influenced by the "phyllotactic numbers" 13, 21, 34, . . . ; and this influence presumably combines with that of the above-mentioned law in such wise that, whenever the conditions are approximately uniform, the probability of the occurrence of a given ray-number shall differ but little from the product of the probability got as above, into a function of the ray-number's position in a cycle whose zero-points are the phyllotactic numbers. My observations, however, are too few to settle this point at present. The smaller phyllotactic numbers, 3, 5, 8, are less influential than the doubles 6, 10.

I regard the investigation as only preliminary, requiring far more varied observations to complete it; but I offer it in the hope that it may set other observers at work in the same direction.

A METHOD OF FINDING THE LAW OF LINEAR ELASTICITY IN A METAL. By JAMES EDWARD OLIVER, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

ASSUMING that the different strength of cast iron (for instance) for tension and for compression should be accompanied by a corresponding difference as to elasticity, due to the occurrence of even powers in the formula

Compressive stress $=$ A. strain $+$ B. strain^2 $+$ C. strain^3 $+$ etc., I get the first two or three coëfficients from experiments on the deflection of a triangular bar resting alternately on its edge and on its face.

A METHOD OF ELIMINATING THE PERSONAL EQUATION IN TRANSIT OBSERVATIONS. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

IF, during the transit, the movable wire of a micrometer could be made to move over the field at the same velocity as the star and if it could at the same time be made to bisect the latter, then a simultaneous observation of time and the position of the wire would furnish a transit observation unaffected by personal equation; we may express this otherwise by supposing the personal equation to be divided by infinity.

It would be difficult to carry this out in practice, but if, instead of dividing by infinity, we are content with a moderately large number, there would seem to be no great difficulty in thus reducing the personal equation to an inappreciable quantity.

In considering what would be necessary to effect this result there are two paths: we must either admit the usual imperfections of such apparatus and plan the mechanism in accordance therewith, or we must attempt to eliminate or control them.

An apparatus constructed in accordance with the first of these methods has already been proposed by Prof. C. A. Young, and as it is exceedingly well contrived for giving a result free from error we will notice the main features of it. In this method the ordinary micrometer remains untouched and another horizontal slide, carrying a vertical wire, is made to move over the field by clock-work. To effect this properly a second or vertical slide is introduced (at right angles to the former) and upon it is fixed an adjustable incline, in contact with which the horizontal slide is kept by a spring. This vertical slide is driven by the clock with a velocity a certain per cent greater than that of an equatorial star, and the incline being set at an angle therewith, whose tangent equals the cosine of the declination, the vertical wire will move the same per cent faster than any other star, and will therefore creep slowly over the same. At the moment of bisection the key of the chronograph is pressed, and the current not only records the time but clamps the moving slide fast in its position, which is then determined by the ordinary micrometer. This being done the slide is released and springs up against the incline; the wire is now ahead of the star, but if the connection between the screw and the clock be made by means of a friction collar, the former can then be adjusted independently until the wire is again behind the star and ready for another observation. It is evident that this method would be affected by only the before-mentioned percentage of personal equation.

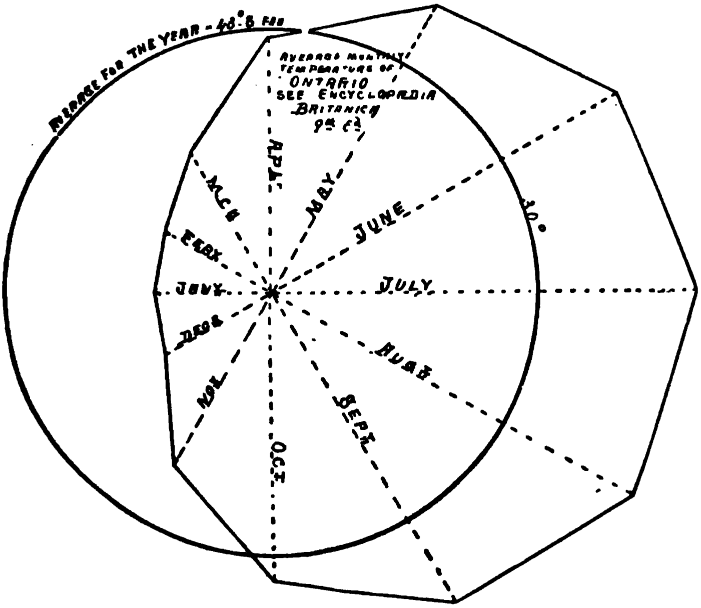
If the second method is chosen we might proceed as follows:—dispense with the ordinary micrometer and substitute therefor one of different construction. This micrometer should have a V-threaded screw as nearly perfect as possible, and it should run in two journals, one of which should be cylindrical and the other a V screw thread of zero pitch—*i. e.*, a grooved journal. The nut should be long and should embrace the screw through 90° only; it should be cut away except at its four corners, so that it would stand on the screw as by four legs. A single spring giving a thrust at right angles to the screw would now suffice to hold the nut against the screw and the latter in its journals. Such a screw would have no back-lash, and a moderate amount of wear would introduce no error in the position of the nut. The frame for holding the movable wire should be a simple extension of this nut, and would not slide in guides, the spring being so applied to the nut as to prevent its revolving about the screw axis. The screw would carry the usual graduated head provided with an arrangement, by which its position could be registered (on a strip of paper) without stopping it. If this micrometer could be so accurately made that the reading could always be depended on for the position of the wire, this method could be made a success, and this reading could be at any time and often tested by means of the fixed wires of the reticule. A uniform motion could be supplied to the apparatus in various ways—it could be obtained from the chronograph by means of an interrupted current, made to drive a small electro-magnetic engine on the eye end of the transit. To reduce the motion for declination, a uniformly revolving sphere could be made to drive, by friction, a wheel so adjusted as to be in contact with a small circle of the same declination as the star,—a device which has already been proposed. A simpler method would be to cause one cylindrical surface to drive another by friction and to place the axis of the second at the declination angle with the first. With only the friction of the micrometer screw to overcome, either method might give good results. In order further to allow of single observations succeeding each other rapidly during a transit, the current which records the time and micrometer position should also check the latter until the star is again ahead of the wire, when the release of the key should start it again for another observation. A better way might be to have a separate key by which the motion could be altered from a certain per cent faster to about the same per cent slower than the star, so that the wire could be made to oscillate slowly over it. As the number of single observations during a transit would be at least equal to the usual number, an intercomparison might give a reliable value for the weight of the whole. The addition of an arrangement for reversing the motion when the telescope was reversed in its Ys would furnish the means of determining the error of the micrometer zero, *i. e.*, of collimation, and make this equally complete with the ordinary method.

NOTE ON A NEW METHOD OF GRAPHICALLY EXPRESSING ANY CYCLICAL FACT IN METEOROLOGY. By W. E. HAMILTON[1], of Chatham, Ontario.

(1) *Definition:*—By a cyclical fact, I mean, the fact of the continuous recurrence of certain quantities, in the same order, and separated by the same intervals of time.

(2) *Illustrations:*—The average lengths of sunlight for the successive months of the year give an illustration of quantities

FIG. 1.



Polar Coördinates. Invariable shape for any scale. Month = 30°.

which would reproduce themselves in the manner above described. Any monthly averages, deduced by statistical enquiry, such as the amounts of immigration in the consecutive months, for a given port, so far as they are assumed to be permanent and not accidental averages, would form a cycle of the kind referred to. The cycle which is graphically illustrated in the accompanying diagrams, is the averages of temperature for the months of the year, in the Province of Ontario, as given in the Encyclopedia Britannica (Fig. 1). So far as the observations go, on which the cycle is based, it is assumed that the average monthly temperatures will recur continuously, for Ontario, from year to year, and we therefore have a cycle falling within the definition (1).

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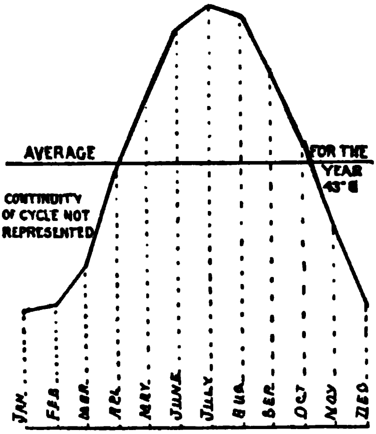
[1] Editor of the late Sir W. R. Hamilton's Elements of Quaternions.

(3) *Problem:*— To express this sequence of averages graphically. It may be conceded, without argument, that no reason exists for going into three dimensions of space, and that, therefore, our diagram should be contained within a plane.

(4) *Old Solution:*—The old and usual method is to represent the element of time by an assumed arbitrary base, on which the intervals of months are represented, by arbitrary but equal spaces, separating the consecutive ordinates, proportional to the average temperatures, which are plotted on an arbitrary scale, bearing no definite relation to the arbitrary scale of months (Fig. 2). The summits of these temperature-ordinates are connected by right lines, and the unclosed rectilinear figure so constructed represents the closed and self-repeating cycle of temperatures. Temporal sequence is indicated by progression from left to right as shown by an arrow to point out the annual motion of the temperature-point. The average for the whole year is represented by a right line parallel to the base.

FIG. 2.



Ordinary method. Rectangular Coördinates. Arbitrary. Same scale as Fig. 1.

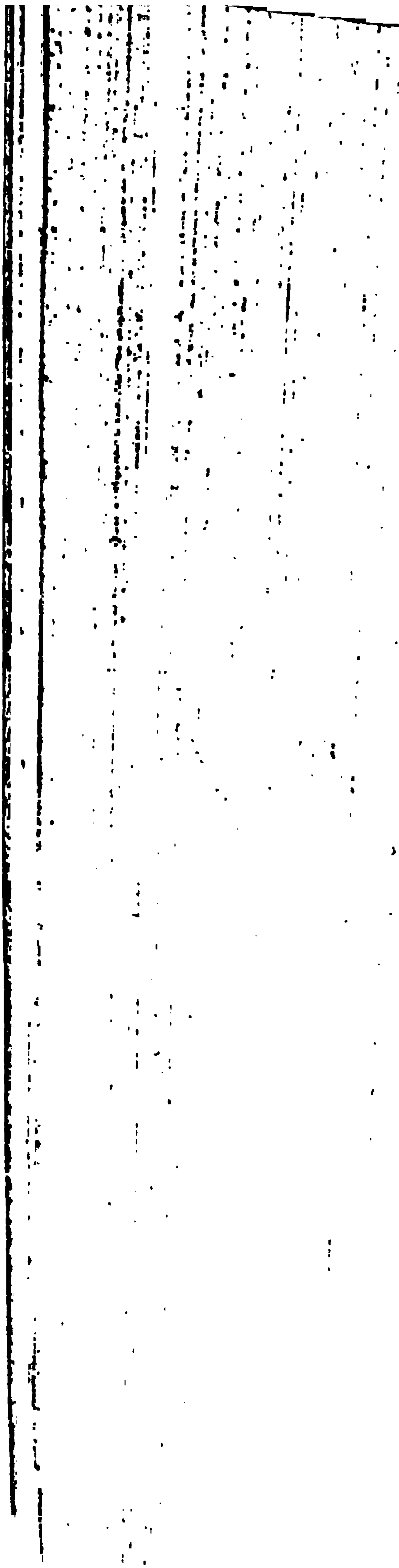
There are two objections to this method:—

(*a*) The visible continuity of the cycle cannot be shown, since the middle of the initial January and that of the January of the next year, are separated as the two most distant ordinates of the series.

(*β*) The broken line of temperatures has no invariable shape, but varies according to the ratio of the arbitrary vertical to the arbitrary horizontal scale.

(5) *New Solution:*—In my method I use polar instead of rectangular coördinates. I take a circle with an initial diameter to represent January, and successive months are separated by angles of thirty degrees from each other. On these radii, the temperatures

r proper proportions, so that the next January
coincides with the initial one, in length and di-
s of the radii are connected by right lines, and
ormed by their terminal points, represents the
emperature-cycle for the year, and is visibly a
hile the polygon, as the simplest consideration
s will show, is of invariable shape, no matter to
mperatures may be plotted. In fact, by the
coördinates, and the consequent constancy of
l for the interval of a month, only one variable
ely, the scale of the temperature-radii. It will
the cycle of time is patent to the eye, as a geo-
nvariable shape, and that thus the objections (*a*)
ply to the new method, in which, of course, the
a circle with the origin of polar coördinates for
us equal to the annual average on the assumed
It may be added here, that for popular illus-
are assumed of equal length, but that of course,
onth-angles should be proportioned to the slightly
the months. It would be interesting did space
e trigonometrical formulæ connecting the inter-
temperature-polygon, with the lengths of the
The temperature-points might also be connected
, but by the curve of most probable curvature,
its evolute and other attributes of geometrical
e in shape by alteration of the scale of temper-
on of the temperature-point, in my method, is
m left to right, passing through the upper part
e spectator faces it, or in other words, the point
way from the reader's body, and not toward it,
is extended before him. For monthly average
e extreme north, which may fall below zero, it
der to avoid confusion, to take a sufficiently low
–30 for the origin of the scale of radii. In my
e zero of Fahrenheit, as the origin. Some minor
spicable advantages of the new method may be
pode, in the annual cycle, of any given month,
iametrically opposite point. The facts of Janu-
g, roughly speaking, the minimum and maximum,
ate equality of the fall and spring temperatures,



are patent to the scrutineer of the diagram, as is the approximate diametral opposition of the intersections of the circle of annual mean temperature with the polygon of monthly means.

(6) I submitted my method, now published for the first time, but invented more than thirteen years ago, to Prof. Kingston of Toronto, who highly approved of it, while Prof. Tait of Edinburgh thought that it was virtually anticipated by the Hodograph of my father, the late Sir W. R. Hamilton of Dublin. It is difficult in these days of general intellectual ferment to ensure priority of invention, where the energies of so many minds are converging on the same problems, and should the system have been anticipated, I can only give the right hand of greeting to my rival whoever he may be. In conclusion, I may be allowed to add that I have tried to make the explanation as full and simple as I possibly could, knowing that while a single sentence might be sufficient to give the key to the mathematical reader, there are many others deeply interested in meteorology, who would find difficulty in following a curt geometrical abstract of the system.

NOTE.—To exhibit the recurrence of the cycle, in the usual method the ordinate for January should be repeated to the right hand of the December ordinate as the representative of the mean Jan., temperature of the following year. These ordinates are replaced in my method by one and the same radius.

TITLES OF OTHER PAPERS READ IN SECTION A.

SUGGESTIONS ON THE NATURE OF COMETS, IN CONNECTION WITH RECENT ASTRONOMICAL AND ELECTRICAL DISCOVERIES. By P. H. Van der Weyde, of New York, N. Y.

SCHEME FOR OBSERVING THE GREAT ECLIPSE OF MAY, 1883. By Charles H. Rockwell, of Tarrytown, N. Y.

SECTION B.

PHYSICS.

ADDRESS

BY

T. C. MENDENHALL,

VICE PRESIDENT, SECTION B.

MEMBERS OF THE SECTION:—

NOTWITHSTANDING the many changes which have been wrought in the internal machinery of this Association by the new organization which it has put on within the past two years, and under which it now begins a sort of new life, the rule requiring an address from a Vice President has survived, and this rule will itself, I trust, render it unnecessary for me to make further apology for asking your attention briefly on this occasion.

Although this duty still lies before one who may be honored by an election to this position, yet, happily at least in the present instance, it carries with it much less responsibility under the new organization than under the old. The chairman of this section is no longer one of two, but one of nearly a half score. A Vice President is no longer expected to marshall the deeds done by half the world of Science during the past year and make them pass in orderly procession before his hearers.

We have, in fact, been shorn of even our alphabetical supremacy, an appeal to which was a last resort in friendly contests with our young and vigorous rivals of the other side. Out of respect for sciences as old as science itself we have freely accorded the first rank, as far as a designating letter may be able to imply it, to our veteran co-laborers the mathematicians and astronomers.

As re-organizers and constitution builders we may draw lines and carve out areas for which we have little respect as students of science, and no arbitrary classification can diminish the strength of the ties which bind us to Astronomy, into whose treasure-vaults we are constantly pouring rich contributions, and Mathematics, upon whose varied and wonderful resources we are obliged to draw for almost every important advance we make.

Indeed this separation of Physics from Astronomy and Mathematics as well as from Chemistry, Mechanics, etc., must be regarded as temporarily convenient for the purpose of specializing the powers of the Association.

Our sympathies, our traditions and, more than all, the constantly overlapping, intermingling currents of thought with which we are all concerned, give us the assurance that the old section, A, must continue to exist; not weaker, but stronger as a result of the digitation which has taken place, just as the fingers unite to make the hand more powerful, more cunning and more useful.

Setting out as we are upon a new and, I believe, a more efficient scheme of work, I will venture to ask the attention of this Section to a subject which may, at first, seem to many to be not quite fit and appropriate to be presented to such a body as this.

I find my justification, however, in the broad title which the general Association bears, as well as in the events of its past history. This is an Association for the Advancement of Science. The forces which have to do with aiding or retarding this advancement are so various that we are in danger of losing sight of some of them.

We are mistaken if we suppose that science is advanced only through contributions which are the result of original research in our laboratories and libraries. Even if so narrow a view be taken, it will be admitted that the talent for research is fostered and encouraged, if not, indeed, created by an atmosphere of recognition and appreciation. The existence of such an atmosphere is in itself a blessing and its production is certainly worthy of our highest efforts.

To this end it is desirable and necessary to bring about a more general diffusion of accurate knowledge concerning the elementary principles and propositions of the science of physics, as well as some degree of familiarity with the methods of physical investigation. I do not refer, of course, to the demands or the necessities of those who expect to undergo a course of training for the purpose of becoming themselves physicists; but rather to the diffusion of this knowledge among the masses of educated people in general.

That this diffusion is not taking place to any great extent and will not, according to natural laws alone, is patent to any observing physicist, who cannot fail to have come in contact with prevailing and pernicious errors, which often carry the weight of repetition, and now and then of recognized authority.

I am aware that this is not an association of educators, and that pedagogics is not, as yet, one of the sciences specifically indicated as worthy of advancement at our hands; but if the growth of a tree is to be made healthy and permanent it is not safe to neglect the soil into which its roots penetrate. Train it and prune it as you will, to grow into vigor and strength it must spring from a rich and generous earth which, though beneath it and below it, must be in harmony with it in order to supply the proper and necessary materials for its sustenance.

It seems, therefore, not improper to raise the question, what can this Association do, or, more specifically, what can this section do to increase the efficiency of instruction in physics?

I do this the more willingly for the reason that a considerable majority of the members of the Section are engaged in this instruction during the greater portion of the year. In America a few only are privileged to devote themselves to original research. Here instruction and investigation, to a great extent, go hand in hand, and it is generally admitted that it is better so. The teacher does not reach his greatest efficiency, indeed he must fail completely unless he continues a student, not of the works of men alone but of Nature herself. On the other hand, some of the best and most fruitful inspirations of the investigator spring from his contact with those to whom he is communicating the finished products of his work. The history of science goes to show that many, perhaps most of those who have contributed to its advancement, have been great teachers. We neglect our duty, then, when we fail to give attention at proper times and under proper circumstances to the improvement of methods of instruction.

Perhaps in no other department of science has a greater change in these methods been wrought during the last ten years than in Physics. And yet this change has been going on in an irregular, unmethodical sort of a way. There has been little or no concerted action among those interested and engaged in the work. Although all have had practically the same end in view, each individual has, in the main, worked out his own solution of the problem in accordance with his own views, modified and often largely controlled by the conditions and restrictions to which he was subjected.

It is not surprising, therefore, that results attained should in many cases be widely different and, on the whole, not entirely satisfactory.

In this new instruction many things have been attempted that could not be, and some things that ought not to be, accomplished. That over-burdened and somewhat obnoxious word *practical* has found a place in our vocabulary and we hear much of practical instruction in physics, whatever that may mean.

The subject, considered as a whole, naturally divides itself into two parts, pertaining respectively to higher instruction and elementary instruction: instruction in the colleges and instruction in the schools. Let us briefly consider each of these.

In referring to higher or collegiate instruction, it will be remembered that I do not include that of the post-graduate course in the University, properly so called, for which laboratories for research are equipped and maintained, and to which students are admitted only when thoroughly prepared by previous training. Fortunately for American students a few such courses in physics are now open in this country and it goes without saying that those who are conducting them do not need advice from us. It is for the large class of under-graduates who pursue the study of physics for a greater or less time that we may be concerned.

Contemporary with the recognition of the possibility of greatly improved methods of instruction, was the recognition of the value of the more thorough study of physics as an element in what is called a liberal education.

These were alike the results of the tremendous strides made by physical science, beginning twenty-five or thirty years ago. Grand and beautiful generalizations commanded the admiration of men skilled in other departments of human knowledge, and equally wonderful applications of principles to practice, touched our every day existence upon so many sides, as to draw forth applause from the millions who are without the "inner court." Physics thus found or forced its way upon the college curriculum to an extent much greater than had previously been thought possible or desirable.

A few keen-sighted men, combining in themselves, happily, the student and the teacher, recognized the fact that thorough instruction in physics implied and demanded the use of laboratory methods, such as had been utilized for some years in chemistry and were rapidly coming into prominence in every other department of natural science.

Among these was, notably, Professor Pickering, whose establishment of a working, physical laboratory for purposes of instruction in the Institute of Technology at Boston, must be regarded as an epoch in the history of this progress; and with this, also, might be linked, although following at a little later date, the widely known establishment made by Professor Mayer at Hoboken.

These were quickly followed by others in the East and in the West, and at the present time there are many institutions of learning in which the laboratory methods of instruction are in use and whose equipment includes a so-called physical laboratory.

To all interested in the study of the present condition of this work I would especially recommend the very valuable Report on the Teaching of Chemistry and Physics in the United States, prepared by Professor Clarke of the University of Cincinnati, and issued about a year ago by the Bureau of Education. This Report is full of facts of great value and doubtless fairly represents the relative standing of collegiate instruction in these two important subjects at the present time.

Professor Clarke has classified the various courses of instruction in physics as follows:—

1st. Full course, including higher mathematical physics, advanced laboratory work and research.

2nd. Full course, with mathematical physics and elementary laboratory work.

3rd. Course in general physics, involving a previous knowledge of trigonometry and including laboratory work.

The other courses, up to ten in number, are elementary in their character, and do not concern the present investigation.

The report contains statistics gathered from nearly four hundred universities and colleges, agricultural colleges and scientific schools. In nearly all of these the study of physics is pursued to a greater or less extent, although it appears that in some instances no report upon physics was forthcoming on account of ignorance as to what was meant by the word. Out of the whole number there were thirty-three institutions in which the instruction in physics fell within the limits established above. Of these there were four of the first rank, two of the second, and twenty-seven of the third.

In chemistry, however, laboratory instruction is to be found in at least one hundred and fifty institutions, the opportunities for instruction in this subject, thus outnumbering those offered for similar instruction in physics in about the ratio of five to one. But it will be remembered that in this contest chemistry has many things in its favor, and that physics is handicapped by the great cost, relatively, of the first establishment as well as by the lack of well defined and systematic courses of instruction.

Taking it as a whole it will be admitted that there has been a rapid, and, I believe, a permanent growth, and that the work has already become so extensive that it appears to be worth while to subject it to criticism and to determine by conference and consultation what improvements, if any, might be suggested.

Admitting the necessity of the laboratory as a means of instruction in physics, two important questions present themselves: first, of the total amount of time given to the subject, what proportion should be spent in the laboratory? and second, what should be the character of the work done there?

I shall not undertake to answer these questions, but will submit one or two conclusions which have been thrust upon me by observation and experience.

Concerning the first, something ought to be said. It will be remembered that the new instruction began at a time which was characterized not only by unusual scientific activity, but as well by what almost amounted to a revolution in educational processes. A great teacher had told us that we studied Nature in books, and when we met her face to face she passed unrecognized. There sprang up a new method, the essence of which was that the mysteries of nature could not be known at second-hand; that a knowledge of things could only be obtained by a contact with things themselves. The use of the text-book fell into disrepute and the student was encouraged to become his own authority. It was as if all men were to cast aside their maps, globes, histories, books of travel, etc., and start out to obtain a knowledge of the world by visiting its different portions.

But it was soon found that many never succeeded in getting far away from home. It is true that there were those who, untrammelled by tradition, precedent or authority, made bold excursions into the regions of the unknown and returned richly laden with spoils, but these were the few; the many were found to require guidance and support for some time before they became able to carry on explorations on their own account.

The underlying principle of the new method was correct and must survive, but it was a mistake to give it universal and unrestricted application. The earlier and indeed much of the later instruction in physical laboratories was tinctured with this error.

By many we were advised that the proper course to pursue was to put into the hands of the student who, in many instances, had little or no previous knowledge of the subject, a few pieces of simple apparatus and expect him to rediscover for himself principles of physical science which although now commonplace, were at one time as completely surrounded by difficulties to the human mind as are now, for instance, the principles of the dissipation of energy and the vortex theory of atoms.

The result of the crude experiments of the student was often to disprove the law which he was expected to establish; for he lacked that knowledge and training which would enable him to take into consideration the influence of secondary causes and conditions and to determine or properly interpret the errors of experiment. Something was gained, it is true, in the way of familiarity with the methods of manipulation, but very little in the acquisition of real knowledge.

Even if this method of instruction be made reasonably successful, the actual information concerning natural laws which the student obtains must be largely superficial, often erroneous, and the rate of acquisition extremely slow. Far better would it be for him to begin his so-called practical study of the subject after becoming tolerably familiar with its general outlines and prominent features, through the study of some reliable text book and especially after having armed and equipped himself with such a training in mathematics as will enable him to discuss understandingly the results which he obtains, to consider the limitations to which they are subjected and the influence which has been exerted upon them by errors of various kinds.

In this matter, as with most others, we are likely to fall into extreme views. Some of us maintain that experiment alone is the key, by the use of which Nature's mysteries are to be explored, and we fortify our belief by pointing to Faraday, the greatest experimental philosopher the world has yet produced. We forget that Faraday was ignorant only of the outward, conventional symbols of mathematical reasoning and that one of the greatest works on mathematical physics, by one of the greatest mathematical physicists of modern times is confessedly but little more than his interpretation.

Dazzled by the success of the leaders and representatives of another school, we proclaim that true success will depend on mathematical attainment, and that mathematical physics is the only physics worthy of the name. Here, again, the exceptionally brilliant few, who have succeeded under this training, stand as its exponents and we fail to consider that, if adopted to the exclusion of the first, its results may be disastrous in the extreme. No better evidence of this need be furnished than is found in the remarks recently made by Mr. G. H. Darwin, concerning a contest for honors, in what is generally admitted to be the greatest school of mathematics and mathematical Physics in existence. Mr. Darwin, who was one of the examiners, says, "The subject which exhibited the average weakness of grasp most flagrantly was thermodynamics. A great many men had read something of it, but very few really understood what they attempted to explain. Extraordinary muddle and confusion was sent up in answer to a question on the absolute scale of temperature. On another question, while the very elements of the subject were unknown to those who answered, the same men reproduced faultlessly the algebraic calculation of the thermodynamic function for a perfect gas."

Mr. Darwin also strongly recommends such a change in the style of questions as that half intelligence may be more stringently treated and men induced to read less and master more, and to gain a comprehension of physical principles.

There can be little doubt but that the experimental and mathematical study of the subject should go on together, assuming, of course, a sufficient preliminary training in pure mathematics.

What seems desirable, therefore, at least in some instances, is less experimental work on the part of the student and more thorough and exhaustive discussion and examination of what is done.

This leads at once to the consideration of what ought to be the nature of the work done in the laboratory. The limits to which I am confined will not allow me to enter into any lengthy discussion of this important question.

I will remark, however, that in my opinion there is much done which is neither desirable nor necessary. As a rule quantitative work alone, and that the best possible under the circumstances, should occupy the time of the student. I would relegate to the lecture table of the instructor all illustrative experiments and qualitative work necessary to a good understanding of the underlying principles of the subject, which every student should possess when he enters the laboratory. That which he gets which is of most worth in his course in a physical laboratory is not a familiarity with the principles of the science, but a training in the methods of investigation in use among physicists, including a knowledge of the use and abuse of experiment and the necessary limits of our knowledge derived therefrom. The study which he ought to make of errors, instrumental and accidental, will be of great value to him in other fields than this.

As an illustration of the lack of this sort of training, I may be allowed to mention a lecture to which I listened recently, delivered by the chief engineer of one of the leading railroads in the country. The subject was the "Great Pyramid," and in speaking of certain measurements taken in the interior he declared the results, which were given in feet, inches, and thousandths of an inch, to be *absolutely accurate*, taking especial care to disclaim anything in the nature of an approximation.

I need hardly explain that he was declaiming against the introduction and adoption of a system of metrology which has done and will continue to do much to increase the simplicity and accuracy of all measurements.

I have said that this quantitative work should be of the best quality possible. It is better for the laboratory to contain a few instruments of real precision than a large number of inferior performance and accuracy. It is not a matter of great importance upon which particular department of physics a student shall spend his time and strength. The underlying principles of this method of study are common to all and it is a matter of experience that when a student has successfully accomplished a tolerably exhaustive investigation of one topic, involving, it may be, but a single instrument with its accessories, he is upon his feet for the remainder of the course.

To sum up, the course of study in physics for the undergraduate collegian, which I have tried to indicate, should include a sufficient training in mathematics to enable him to apply his knowledge with ease and facility to the more common physical problems; a thorough and exacting course of text book and lecture work, in which the application of his mathematical knowledge would be made, and during which all illustrative experiments necessary to a complete understanding of the text should be exhibited by the instructor from the lecture table; and, finally, this to be supplemented by a course in the laboratory in which more attention is paid to the quality than to the quantity of work done; during which every problem is discussed, as far as possible, both mathematically and experimentally and especial attention is given to the discussion of the results of experiment and of the more elementary portions of the theory of errors.

Considering the work as thus divided into three parts, I am unable to see which is the least essential.

I desire to say a few words in regard to instruction in physics in the school, about which we are, apparently, more remotely concerned. Even greater reform is demanded in this direction than in the other. Although there are numerous American text-books, I venture the remark that none have properly combined, in their making, the experience of the class room with the critical knowledge of the scholar. We may import them from Europe, it is true, as we must, also, our more advanced text books, but, although in the main vastly superior to our own, they are still not entirely suited to the wants of American schools and American pupils. These books, in the hands of teachers who know little or nothing outside of the books they use and often falling far short of that, serve to put the instruction in elementary physics in this country in a condition over which no one can grow very enthusiastic; and this in spite of the prominent place which has been given it and the considerable attention it has received. Unsound doctrines and absurd theories are promulgated because of an inability to distinguish the ring of genuine metal. These become so deeply rooted that it is difficult and often impossible in after years to clear them away.

I believe it to be possible for this Association to exert a strong influence in favor of an improvement in the character of the instruction in physics in both elementary and higher institutions of learning in America. Much of it at present does us no credit and must eventually do us much harm. At a meeting of this Association in Nashville, five years ago, a committee was appointed to report upon Science teaching in the Public Schools. At the Boston meeting, two years ago, this committee presented a report which embodied much thought upon the subject and was replete with just and keen criticisms of present systems. It is greatly to be regretted that this report cannot have found its way into the hands of those whom it would most benefit. A wide distribution ought to have been secured, and I am convinced that it is not yet too late to remedy this error.

The same gentlemen were continued as a committee to report upon the Best Method of Science Teaching in Public Schools, and it is to be hoped that a scheme may be presented at no distant day. I will venture the opinion, however, that the best results will not be obtained until this, or a similar, committee shall work in coöperation with representatives of the Public Schools themselves, and I would suggest the feasibility of securing such coöperation through the National Educational Association.

No such difficulty is in the way of securing an improvement in the teaching of physics in Colleges and Universities, for those most interested are, in the main, a part of this Association and of this Section.

I will not venture to suggest in what manner the Association might best make itself felt in this matter, although I think that would not be difficult to ascertain. I have only endeavored to direct attention to some of the salient features of the problem and to ask its consideration at the hands of many members of the Section who come in almost daily contact with it and who will, I am convinced, sustain me in the belief that it is not unworthy to be brought before this body.

In preparing this discussion of the subject, I have not had access to such information as would have been desirable, concerning the work which is being done in many institutions, nor have I been able to consult with others who are especially engaged in its management. Indeed it was this very lack of accessible information, this very impossibility of personal consultation, with which I met in the outset, that convinced me of the importance of directing the attention of the Section to the subject.

What the Section *could* do in the direction indicated seems to be tolerably clear and certain. Whether it is wise or desirable that it should undertake to do anything is a matter which I willingly leave for it to determine.

PAPERS READ.

ON THE APPARENT SIZE OF MAGNIFIED OBJECTS. By WM. H. BREWER, of New Haven, Ct.

[ABSTRACT.]

AN object seen magnified in a compound microscope by different persons impresses them very differently as to its apparent size. It is well known that such magnified object seems very much larger to some persons than to others, and a few data pertaining to this fact have been published.

Some years ago, when I had classes annually in microscopy, it was my custom to illustrate this difference of mental impression by having each member of the class in turn mark his estimate of the apparent size of some object, and when all had done, exhibit to them the estimates. This always provoked discussion, reëxaminations of the object, and wonderment on the part of some that others saw it so much larger or smaller than it appeared to themselves.

Inasmuch as I knew of no publication of specific experiments pertaining to this matter, I finally determined to keep a record of such estimates, and see what relation the average bore to the standard adopted by microscopists. Such record was begun with the estimates of my class in May, 1873. To this were added the estimates of succeeding classes. In 1876 I ceased giving instruction in microscopy, but these experiments have been continued upon such other persons as I have had opportunity to examine, until now I have a number of observations sufficiently large to give some results.

At one time or another I had used various objects for this experiment: a circular diatom, a ruled scale (as has since been used by other experimenters), an insect, and other objects; but for reasons to be mentioned later, I finally chose a small insect as best suited to my purposes.

The insect chosen was a head-louse (*Pediculus capitis*), mounted in glycerine jelly, an unusually fine specimen. I used a binocular stand, a 1½ inch objective, which, with the ocular used gave a magnifying power of about thirty-nine diameters. The estimates recorded were given in answer to a question stated somewhat thus:—"About how long, from snout to tail, does this insect *appear* to you as you see it magnified in the instrument?" If further explanation was asked, as it very often was, it was given, that the object of the inquiry might be clearly understood. Some of the observations were made with a 3-inch, a 6-inch, a 12-inch and a 24-inch rule lying on the table for convenient comparison; most of the other observations with a foot-rule similarly at hand.

The image projected with camera lucida upon a sheet of paper ten inches from the axis of the instrument, gives the following data:—length of image "from snout to tail," 4.66 inches (118.4 mm.); greatest dimensions including antennæ (or as some observers expressed it, "horns and all") 4.87 inches (123.7 mm.); diameter of field 5.85 inches (148.6 mm.). The insect was therefore entirely within the field with some room on all sides and yet extending far enough (about ⅘ across the field) so as not to appear as a minute object in a large circle of light. The instrument was always inclined at a suitable angle for comfortable observation, the object viewed by transmitted light, and nearly all the observations were by daylight.

The number of persons whose answers I have recorded is 443. About 230 of these were students in the Sheffield Scientific School of Yale College, many of whom were accustomed to use the microscope. Of the remainder, thirteen were children under twelve years of age, a few were ladies, over a hundred were mechanics and workmen, others were professional men, etc., some of whom were experts in the use of the instrument. A large house is in course of building near my own, and one day I took the microscope to where the workmen were taking their "nooning," and with the instrument set on a pile of bricks as a table, they took turns in viewing the object and making their estimates; at another time the workmen on a bridge were tried; other days I visited the machine shops near the college, and thus in a variety of ways the subjects for examination were caught.

The persons experimented upon thus embrace a great variety as to age, intelligence and vocation, from veterans in the use of the microscope to those who were looking into it for the first time; in age from less than five years to upward of eighty-five; in intelligence from eminent scientists to Irish hod-carriers and negro laborers.

Very many of the estimates were sharp and specific, as "4½ inches," "about 6 inches" or "just about 2 inches." Others were less definite, as "5 or 6 inches," "3½ or 4 inches. In such cases I have used the mean in my tables, so it must be understood that many of the estimates were not so sharply stated as the figures in the table would imply.

Of the 443 recorded numbers, two are discarded because the question was obviously misunderstood; another said the object conveyed no idea to him of size whatever, merely form, and he could give it no size. The estimates of the remaining 440, without giving the number for each fraction of an inch, may be tabulated as follows: —

Less than one inch	2
1 inch and less than 2	37
2 inches " " " 3	64
3 " " " " 4	88
4 " " " " 5	91
5 " " " " 6	56
6 " " " " 7	49
7 " " " " 8	14
8 " " " " 9	10
9 " " " " 10	7
10 " " " " 12	8
12 "	7
14 "	2
18 "	1
2 to 5 feet	4
Total number of persons	440

As the image projected at 10 inches, measures 4.66 inches, we may say that to all those whose estimates are 4½, 4⅝ and 4¾ inches, the object appeared at its theoretical magnitude. These number. forty-one, about nine per cent. There were 241 estimates, or about fifty-five per cent below 4½ inches, and 158, or about thirty-six per cent above 4¾ inches. Eighty-two persons, or nearly nineteen per cent estimated from 4¼ to 5 inches inclusive, and it is fair to say that all these saw the object at about its theoretical magnitude. There were 236, or nearly fifty-four per cent lower and only about half as many, 122 or less than twenty-eight per cent higher. There are sixty-five estimates of 2 inches or less and twenty-two of 10 inches or more; there are ten of an inch or less and fourteen of a foot or more.

This enormous difference of mental conception, as to size, immediately suggests comparisons with the impressions different people have of the size of the moon, which is really a very different matter.

The moon is seen as a bright, round, structureless object suspended against a darker sky which spreads out on every side into a vast and practically unlimited field, and the object is at an unknown but evidently considerable distance. With the object seen in the microscope, all this is reversed. We have a dark object on a bright and limited field. We look into a small dark hole through which we see the open light, and just beyond the faintly visible circling edge which limits the field, we see the magnified object. It never seems more than a very few feet distant at most. Most persons whom I have questioned on this matter say that the object appears at about the distance they would hold a book to read or any object to be plainly seen; many say less than this. It never appears at a vast and indefinite distance from us, as the moon does; it always appears in near view, as a large insect might against a circular window near, or upon a limited white surface near us. Its apparent distance from us may be, and usually is, indefinite, but it is never great.

When we see any object by natural vision so plainly and apparently so near, we always form a reasonably correct idea of its size; it is only when the distance becomes very much greater, that the judgment becomes seriously perverted and then, the greater the distance the greater the liability to such perversion.

Further, comparing this phenomenon with that of the apparent size of the moon, that is a structureless disk at an indefinite distance in a measureless field; probably if it had legs, and head, and body, or was of irregular shape like a house or even a cloud, no one would say that it looked no larger to them than a dinner-plate, as many now do. As it is, there is nothing in itself, even if it were near, to suggest any size whatever. On the other hand, the object I chose for these estimates was an organism which when magnified was not so very unlike creatures with which we are familiar. Many compared it to a cockroach, a turtle, a lobster, or some other familiar animal. For example: two students were discussing their respective estimates which differed widely; one said earnestly to the other, after again looking into the instrument, "How is it possible that you have marked eight inches? It looks to me scarcely larger than a big cockroach." The other answered, as he too again looked into the instrument, "it seems as large to my eyes as a lobster would lying on the table before me;" and similar comparisons I have heard a score of times.

As far as I have been able to see, this mental impression as to size is reasonably definite with each individual and usually does not materially change, unless the impression at first is far from the theoretical size, and is changed later by practice in drawing with the camera-lucida. To one person the object will always appear smaller than to another. For example: a graduate student in my class estimated the length 2 inches; seven years later (during which interval he had been engaged in scientific work, and had made considerable use of the instrument) I tested him again, and his estimate was 1½ inches, he having forgotten entirely that he had ever estimated it before. Another, a professor in college who constantly uses a microscope in his professional work (to get the form of objects, but who has no occasion to draw them), estimated 9 inches; two or three years later I tested him again; he answered, "6 or 8 inches." Again, after several more years had passed, as he happened to be in my room when the instrument was on the table, I tried him again as if it was merely a casual question; he answered, "about 7 inches;" the interval between the first and last observation was more than eight years. I have kept records of duplicate estimates made by more than thirty persons, the intervals ranging from a few months to nine years, and as a rule, those who see the object as of large size first, always see it so; this is true even of children. I experimented with one of my own children, a boy in his sixth year, by having him point out on a stick how long the object appeared to him, and the length he indicated was 6 inches. Six months later I tested him again, using a very different stick and he indicated 6½ inches.

Microscopists as a class do not estimate any nearer the theoretical standard than persons unaccustomed to the use of the instrument. I have run over the list and taken the estimates of the twenty-nine persons whom I know to be microscopists of some considerable skill, and twenty-one of them, or three-fourths of their number, estimated below the theoretical size; seventeen of their estimates were less than 3 inches and six less than 2 inches. The college professor who showed me the first good compound microscope I ever saw, now more than thirty-three years ago, and who has used the instrument more or less ever since, estimated the length at but 1¼ inches.

While the majority of microscopists underestimate the length, mechanics and artisans more often overestimate. They usually mentally divide the object into parts and from their sum make the total, and I think that their estimates average nearer the theoretical value than would those of skilled microscopists not accustomed to draw with a camera-lucida.

Regarding the few persons who estimated several feet, one was a grocer who said that the moon always seemed very much larger to him than it did to most people,—his estimate was "2 to 2½ feet. The highest estimate was by a mechanic who was accustomed to draw and plot his work. His estimate was 5 feet. The instrument stood upon a high desk in his shop, inclined highly, he examined the object leisurely and was entirely ignorant of the estimates of any one else. Upon questioning, he compared it to a picture thrown upon a screen by a stereopticon.

With most persons, if the estimate is made quickly, the impression as to size is reasonably definite, but if one stops and tries to reason upon it, it grows less so. We can illustrate this to ourselves by looking into the instrument with one eye and at a scale with the other. If the scale is fixed, the object seems of a definite length upon it, but if the scale is retreated from or approached to the eye the object covers more or less space and the impression of definite size is soon lost. One reason for choosing the object I did for this investigation was that the interest in the species, with most observers, drew attention away from conscious calculation.

Occasionally a person is found who can picture it of any size. A professor of physics, accustomed to optical instruments, says "I can see it at will of any size from three inches to three feet, but it appears best at about six inches." And I have credited him with that estimate in my list.

I do not purpose to enter here the disputed fields of psychology into which the study of visual perception naturally leads us, but structive things brought out by these experiments. curious has been the antagonism between the ed from the visual angle of the magnified object, distance from the observer. We commonly say on of size is determined by the two factors, visual listance of the object. But in these estimates it s to see in how many cases the impression as to e has been antagonistic. Thus a person, who had pparent size at 1¼ inches, was asked how far off seemed, he held an object up before his eye to r off the magnified object appeared. At the d and with the visual angle actually subtended, have appeared about a foot long. I have noticed trating the same class of phenomena, where the arent size and apparent distance were made inde- h other and without calculation, and when if one other should have been many (even ten or more) small.

es only to the mental impression of size when aified in the compound microscope. With the e there are no such great differences of impression Of various other phenomena connected with these not the purpose of this paper to speak.

PIC RAIN-BAND. By WINSLOW UPTON, of Wash-
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[ABSTRACT.]

attention has been bestowed upon the variable rum due to atmospheric causes, especially to those ce of aqueous vapor, which have been studied by Smyth and others. These vapor lines vary with ueous vapor and hence can be taken as furnishing e amount present at different times. The lines ially suited for this purpose and can be observed pectroscope directed to different portions of the

[The changes in the lines themselves which cause the variability of the D rain-band illustrated by chart.]

Regular observations have been made by myself with a Browning pocket spectroscope, the results of which are illustrated by a diagram giving the rain-band curve and also the amount of rainfall noted and the psychometric curve obtained from dry and wet-bulb readings.

An improvement of the instrument is suggested by introducing a shaded band for comparison.

The results obtained may be summarized:

1. The rain-band is of value in estimating the amount of aqueous vapor present in the atmosphere.
2. Its indications follow in general those of the psychrometer but have the advantage of showing the vapor present in a large portion of the atmosphere, which advantage is occasionally of use.
3. It is of some use in foretelling rain, but must be used as any hygrometer and not as an infallible indicator independent of other conditions.
4. Improvements in the instrument used are needed to increase its efficiency, before it can be regarded as a meteorological instrument.

ON THE DURATION OF COLOR-IMPRESSIONS UPON THE RETINA.

By E. L. NICHOLS, of Richmond, Ky.

[ABSTRACT.]

BY a modification of the method of Plateau the author has determined for six different portions of the spectrum, the rate of revolution which must be imparted to a disk, with several narrow open sectors, in order to produce an unbroken visual image of the spectral region viewed through the disk.

The rate of revolution, which may be taken as a measure of the persistence of vision, was found to be a function of the wave length and of the intensity of the ray. The results of the experiments described may be represented by a series of curves, with wave-lengths as abscissæ and duration of the impression upon the retina as ordinates. These curves show a certain resemblance to the curves for the relative luminosity of the different spectral regions, as measured by Frauenhofer, Vierordt and Rood. The maximum is in the same position — between D and E but nearer D than E. It seems highly probable that the duration of impression is inversely proportional to the luminosity. The curves for the duration of the visual image exhibit, it is true, relatively larger values for the more refrangible rays, but these curves correspond of necessity to a very faint spectrum. The change which the curve for luminosity would undergo, were the intensity of the ray decreased, would, owing to the greater activity of the violet nerves of the eye at low intensities, cause that curve to approach much more nearly, if it did not coincide with the curves for duration of impression.

It was also found that the curve differed greatly in the case of different observers, and to a less degree from time to time, for the same eye.

The interval of darkness which may be allowed to intervene between exposures of a given duration, without interfering with the apparent continuity of vision, and its variation when the length of exposure varied, were also determined. The exposure varied from $\frac{12}{1000}$ second, to $\frac{756}{1000}$ second, and the interval of darkness was found to vary, but in less degree.

ON A MEAN-DIRECTION INTEGRATING ANEMOMETER. By CHAS. K. WEAD, of Ann Arbor, Mich.

[ABSTRACT.]

MANY forms of anemographs have been devised that record the direction of the wind at each instant as in Beckley's, or to eight points from time to time, as in the signal service record. But it is a tedious problem to obtain from these records the "mean direction" of the wind for any time by the aid of a traverse table. The instrument now described performs at each instant the resolution into an N and E component that is effected by the traverse table, and takes the sums of all similar components for any length of time.

Two equal gear wheels A and B are connected to one of the same size on the shaft of the wind-vane: on each of them a stud works in a slot in the head of the familiar yoke-headed connecting-rod; the two studs being set "at quarters," the distant end of the connecting rod will be displaced from its mean position by an amount proportional to the sine or cosine of the angle through which A and B and the vane have been turned from the meridian. On these ends friction-wheels C and D are pivoted which turn freely in a plane perpendicular to the rod. C and D slide over the faces of wheels underneath them and are driven by friction on them: these wheels, E and F, have the same velocity, and are driven either by clock-work to get a time-integration as in Coffin's tables, or by a "step by step mechanism" (as in a dial telegraph) actuated whenever a mile of wind has passed, to obtain a distance-integration. The velocity and direction in which C and D are driven will depend on their distance from the centres of E or F; that is, on the position of the studs in A and B, and so ultimately on the direction of the wind.

C and D are connected with counters or with type-wheels for printing; and the instrument is easily modified (as in the drawing exhibited), so as to find the mean direction from such old records as give the actual direction at every instant.

To find the mean direction: obviously the reading of the counter of C divided by the reading on the counter of D gives the tangent of the angle from the meridian (or the co-tangent, according to the original setting of A and B).

ON VISION BY THE LIGHT OF THE ELECTRIC SPARK. By W. LE CONTE STEVENS, of New York, N. Y.[1]

[ABSTRACT.]

THIS paper is merely the continuation of an investigation, published lately in the Am. Journal of Science, in which the current theory of binocular perspective, on the principle of visual triangulation, has been tested in various ways. This theory still has its place in most of our text-books, being due mainly to the surviving authority of Sir David Brewster and Brücke.

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[1] Published in full in the Am. Journal of Science for Oct., 1882; also full abstracts in the London Philosophical Magazine for Oct., 1882.

The writer has shown that it fails to take into account some important physiological conditions and is wholly inadequate in many cases. He has shown that the unconscious interpretation of muscular sensation explains all that Brewster's theory explains, and much more. In studying certain mistaken observations by Brewster, he has been enabled to devise a new mode of stereoscopy, in which the perception of binocular relief, reversible at will, is secured in the examination of a pair of perfectly similar figures, by properly adjusting these in position and taking advantage of the almost spherical form of the retina in each eye. A partial explanation of this has been published, in which it was assumed that the eyes have perfect freedom of motion. Believing, however that such motion is not essential, however important it may be in completing the perception, he deemed it advisable to study this mode of stereoscopy, with visual lines parallel or divergent, by the light of the electric spark. This has been done, with the aid of Mr. W. W. Share, of Columbia College, and with apparatus loaned by Prof. O. N. Rood.

The result has been to show that the clear perception of binocular relief is attainable, not only without motion of the eyes, as first shown by Dove, but without convergence of visual lines and without the perception of double images in any part of the binocular field of view.

THE BINOCULAR UNION OF SPECTRAL IMAGES. By W. LE CONTE STEVENS, of New York, N. Y.[1]

[ABSTRACT.]

IN studying the theory of vision the writer has had occasion to test certain experiments by Professors Wheatstone, Rogers, and Wundt, on the binocular union of images obtained by fatiguing the two retinas with the light from illuminated diagrams, so arranged as to produce the appearance of relief, and then noting whether this appearance is recognizable in the subsequent spectral images that remain visible with more or less distinctness after the actual pictures are removed.

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[1] Published in full in the Am. Journal of Science for Nov., 1882, and London Philosophical Magazine for Oct., 1882.

The source of possible error in the experiments hitherto made consists in the fact that the subsequent perception is vitiated by the observer's knowledge of what he has just seen, or of what ought to be the binocular resultant under usual conditions. This objection is applicable even in cases like that detailed by Professor Wm. B. Rogers, in which the two dissimilar diagrams were presented, each to the appropriate eye, in succession rather than at the same time. This method has been adopted in the present investigation, but with the additional important precaution, that the person tested shall not know what is the nature of the relief that should result if the two diagrams were simultaneously viewed. This was then ascertained, under varied conditions *by means of* the spectral images.

The result is a full confirmation and an extension of the conclusion reached by Professor Rogers.

These experiments, like those with the electric spark, show that muscular action is by no means necessary to the perception of binocular relief, though often an important aid; also that the perception of double images, whether consciously or unconsciously, is not necessary. The writer believes that difference in the degree of attention given to different points, at the same moment seen by direct and by indirect vision respectively, is the only indispensable condition to the perception of relief, and that the interpretation of this is a product of individual experience and not, in any demonstrable degree, of intuition. In this he accords with Helmholtz in opposition to many others who have written on this subject.

UPON THE ELECTRICAL EXPERIMENTS TO DETERMINE THE LOCATION OF THE BULLET IN THE BODY OF THE LATE PRESIDENT GARFIELD; AND UPON A SUCCESSFUL FORM OF INDUCTION BALANCE FOR THE PAINLESS DETECTION OF METALLIC MASSES IN THE HUMAN BODY.[1]

By ALEXANDER GRAHAM BELL, of Washington, D. C.

THE subject of my present paper recalls a time of intense excitement and painful suspense. The long, weary struggle with the untimely death-wound—the prolonged suffering borne so bravely and well by the lamented President Garfield—must still be fresh in every recollection. The whole world watched by his bedside, and hopes and fears filled every passing hour. No one could venture to predict the end so long as the position of the bullet remained unknown. The bullet might become safely encysted, but, on the other hand, recovery might depend upon its extraction. The search with knife and probe among vital and sensitive tissues could not be otherwise than painful and dangerous; and the thought naturally arose that science should be able to discover some less barbarous method of exploration.

Among other ideas[2] the thought occurred that the bullet might produce some sensible effect in modifying the field of induction of a coil brought near the body of the President, and that the locality of the bullet might thus be determined without danger to the patient and without pain; for it is well known that induction can be powerfully exerted through the human body without producing any sensation whatever.

Upon the balancing of Induction.

The influence that is exercised upon induction by metallic masses has formed the subject of numerous experiments by different investigators; and the principle of balancing the effects of induction on one portion of a circuit by equal and opposite effects produced upon another portion has been utilized in nearly all such investigations.

The earliest form of induction balance for this purpose appears to have been devised in Germany by Prof. Dove,[3] about the year

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[1] A preliminary notice relating to this paper was published in the *Comptes Rendus* of the French Academy of Sciences, Oct. 24, 1881.

[2] See Appendix, note 1.

[3] Pogg. Ann. vol. liv, pp. 305-335.

1841, and a good description of it in the English language may be found in De La Rive's "Treatise on Electricity," (1853 edition, vol. I, pp. 418–433)[4].

Another and superior arrangement for the same purpose is the well-known induction balance of Prof. D. E. Hughes[5].

The Static Induction Balance of J. E. H. Gordon[6] though primarily intended for experiments upon specific inductive capacity, might also, perhaps, be employed in the same class of investigations.

My own attention was directed to the balancing of induction a number of years ago by the disturbing noises produced in the telephone by the operation of telegraphic instruments upon lines running near the telephone conductor.

The difficulty was remedied by using two conductors instead of one, and by so arranging them with reference to the disturbing wires that the currents induced in one of the telephone conductors were exactly equal and opposite to those induced in the other. In this way an induction balance was produced and a quiet circuit secured for telephonic purposes. This method was patented in England in November, 1877, and during the whole winter of 1877–8 I was engaged in London upon experiments relating to the subject.

In the course of these researches I made frequent use of flat spirals of insulated wire, like those employed by the late Professor Henry[7] in his experiments upon induction.

My method was to pass a rapidly interrupted voltaic current through one flat spiral while I examined its field of induction by means of another flat spiral connected with a telephone. The currents induced in the latter coil produced a musical tone from the telephone.

At every point in the field of induction it was found that by turning the plane of the exploring coil a position of silence could be obtained, and another of maximum sound, the two positions making a right angle with one another.

It was also noticed that when a position of silence was established a piece of metal brought within the field of induction caused the telephone to sound. This effect was most marked when the two flat spirals were in close proximity, and were arranged with their planes parallel, as shown in fig. 1.

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[4] A similar apparatus was independently devised in America a number of years ago by Professor Rowland, of Johns Hopkins University. It is to be regretted that his discovery of the fact that he had been anticipated by Dove prevented Professor Rowland from completing and publishing his researches.

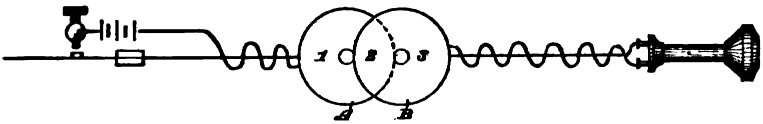
[5] Phil. Mag., July, 1879, vol. ii, p. 50.

[6] Phil. Trans. for 1879, p. 417.

[7] Silliman's Journal, xxviii, 329; xxxviii, 209; xli, 117.

When a silver coin, such as a half-crown or florin, was passed across the face of the two coils, the silence of the telephone was broken three times. The instrument emitted a musical tone when the metallic disk passed the points marked 1, 2, and 3 in the illustration, but the loudest effect was produced when the coin crossed the area marked " 2," where the two coils overlappped.

Fig. 1.



After my return to America I embodied these and other results in a paper " *Upon New Methods of Exploring the Field of Induction of Flat Spirals*," which was read before this association at the Saratoga meeting in August, 1879.

Practical Application.

While brooding over the problem of the detection of the bullet in the body of President Garfield, these experiments made in England returned vividly to my mind. It seemed to me that if the overlapping area " 2 " of the two coils shown in fig. 1 could be brought over the seat of the bullet without disturbing the relative positions of the coils, the telephone would probably announce the presence of the bullet by an audible sound.

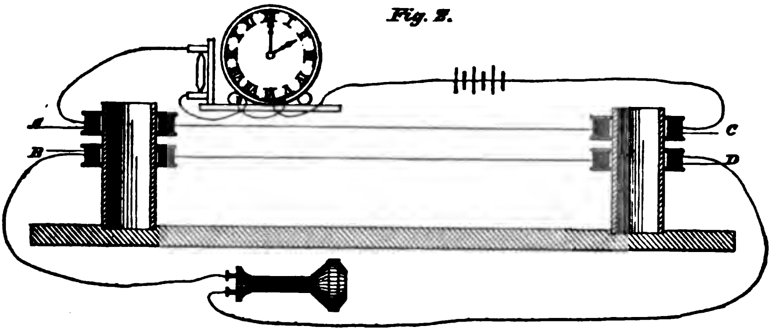
A crude experiment was at once made to test the idea. A large, single-pole electro-magnet (the core of which was composed of a bundle of fine iron wires) was used in place of coil A (fig. 1 ;) and a small coil of fine wire taken from a hand telephone was arranged a little to one side of the pole to represent coil B. The small coil being connected with a telephone, a battery current was passed through the coil of the electro-magnet, and the battery circuit was made and broken by an assistant.

Under these circumstances a much better balance was obtained than could possibly have been anticipated. Upon now bringing a leaden bullet near the small coil, a distinct ticking sound could be heard from the telephone each time the battery circuit was made and broken.

Being absent from my laboratory, and without facilities for proper experiment, I communicated my ideas to Mr. Charles Williams, jr., of Boston, manufacturer of electrical and telephonic apparatus, who kindly placed the resources of his large establishment at my service; and, at great personal inconvenience, delegated his best workmen to attend to my experiments.

Upon attempting to devise an appropriate form of apparatus for the special purpose in view, I saw that there were great practical difficulties in the way of utilizing the arrangement shown in fig. 1, and it occurred to me that the apparatus of Professor Hughes might perhaps be employed with more advantage as the basis of my experiments. In the ordinary form of Hughes' induction balance four coils are used, as shown in fig. 2. Through the agency of a Hughes microphone the ticking of a clock is made to create an electrical disturbance in the voltaic circuit containing the two primary coils (A C) and a corresponding disturbance is produced by induction in the two secondary coils (B D) connected with the telephone. If the connections are so arranged that the currents induced in the telephone circuit by the coils A C are in the same direction, the ticking of the clock is heard very plainly, but if they are in opposite directions no sound is perceived.

Fig. 2.



In the latter case the action of one primary coil (A) opposes that of the other (C), and an electrical balance results. If now a piece of metal is brought near one pair of coils (say A B) the balance is disturbed and the ticking of the clock is audible at the telephone. The arrangement of the coils (A, B, C, D) was the point to be studied, the microphone attachment being of no importance in the combination; for it is well known that a rheotome to break the primary circuit completely at intervals can be substituted for the microphone with advantage.

It seemed to me that two of the coils (A B) in the Hughes induction balance might be attached rigidly to a wooden handle, so as to be moved over the seat of the bullet without changing their relative positions, and that all the adjustments necessary might be made on the other pair of coils, which need not be moved from their place, and would not therefore be liable to disarrangement. If a single pair of coils were to be used as in fig. 1, they must be adjustable one upon the other. But if during the course of exploration the coil B (fig. 1) should be moved from its proper position even to the extent only of a small fraction of a millimetre, the balance would be disturbed and the exploration might have to be stopped in order to adjust the apparatus. These considerations led me to the conclusion that some modification of the Hughes induction balance was most suitable for my purpose, and I immediately commenced the construction of such an apparatus.

Suggestions Tested.

Just at this time I learned from the newspapers that Prof. Simon Newcomb, of Washington, had the idea of using a magnetic needle to indicate by retardation of its rotation the proximity of the bullet in the body of the President, and I telegraphed to Professor Newcomb the offer of my assistance in carrying on experiments, knowing the comparative difficulty he would experience in having apparatus made in Washington.

At his suggestion I tested the point whether the rotation of a leaden disk and of a leaden bullet underneath a delicately suspended magnetic needle would cause a deflection of the needle.

The disk occasioned a deflection, but the bullet produced no sensible effect. I telegraphed the result to Professor Newcomb, and at the same time took occasion to inform him of the hopeful results I had obtained with the crudely constructed induction balance referred to above.

I was much gratified by his immediate appreciation of the experiment. He telegraphed that he thought an induction balance promised a much more hopeful solution of the problem than his own method, and encouraged me in every way to continue my experiments.

This appreciation determined me to proceed to my laboratory at Washington, where I was accompanied by Mr. Sumner Tainter, who was anxious to assist in such a cause. I learned from Professor Newcomb that Mr. Geo. M. Hopkins, of Brooklyn, had independently suggested the use of Hughes' induction balance, and had made experiments in Brooklyn, the results of which were published in the New York Tribune on the 11th of July, 1881.[8] Mr. J. Stanley Brown (private secretary of President Garfield) kindly handed to me the letters he had received from Mr. Hopkins,[9] and also a Hughes induction balance like that shown in fig. 2, which Mr. Hopkins had forwarded to the Executive Mansion for trial.

This apparatus was at once tested in my laboratory, with results slightly better than those I had obtained in Boston.

My Boston apparatus did not give a greater hearing distance than 3 cm., whereas with the Hopkins apparatus I could distinguish effects at a distance of 3.75 cm.

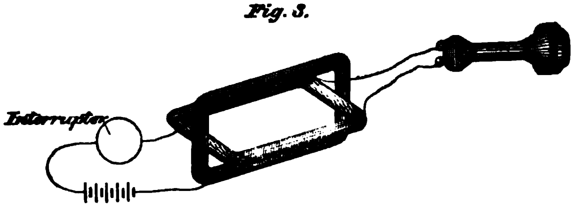
Two of Mr. Hopkins' coils (A B, fig. 2) were then fastened upon a wooden handle to form an exploring instrument, and the whole apparatus was arranged for immediate use in case of any necessity arising for an experiment upon the President. I set myself in communication with Mr. Hopkins, and requested his assistance and coöperation, and in reply received through Private Secretary Brown the following account of further experiments:

"60 IRVING PLACE, BROOKLYN, *July* 16, 1881.

MR. J. STANLEY BROWN:

DEAR SIR: I have made two new instruments on plans differing from that sent, but they yield no better results. The first consisted of two oblong coils arranged at right angles to each other, thus:

Fig. 3.



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[8] See Appendix, note 2.

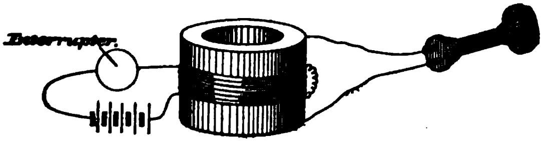
[9] See Appendix, notes 3 and 4.

The outer coil being of coarse wire (No. 18) placed in the primary circuit, the inner coil being of very fine wire (No. 36) and connected with a telephone. The parallel currents traversing the wires neutralized each other, and no audible effects are perceived in the telephone, but on presenting a metallic body to the instrument upon a line bisecting the angle between the coils the clicking in the telephone is heard.

This instrument possesses only one advantage over that sent, and that is that it requires no adjustment.

The other instrument consists of two large coils of very fine wire (No. 36) placed upon opposite sides of a coil of coarse wire (No. 16), the fine coil being connected so that the induced currents neutralize each other, thus:

Fig. 4.



I am sorry to be obliged to say of this as of the other, that it is no more sensitive than the one sent. To produce the best effects from the instrument which you have it will be necessary to use all the battery power possible without burning the coils, and two receiving telephones of the best construction must be used.

As I stated in the first instance, if the ball is more than two inches deep, I think it cannot be located by this means.

If larger coils were used the instrument might be operative at a greater distance, but the area indicated as containing the ball would be so large that the result would be indefinite and without value.

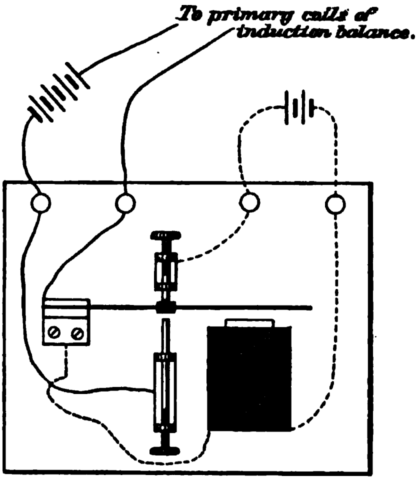
Hoping that Prof. Bell will be able to succeed, I remain,

Yours very truly,

GEO. M. HOPKINS."

Prof. Hughes of London, England, Prof. Trowbridge of Harvard College, Prof. Rowland of Johns Hopkins University, and other authorities were consulted by telegraph as to the best theoretical form of induction balance for the purpose required, while empirical experiments were being carried on under my direction in my laboratory at Washington by Mr. Sumner Tainter; in the electrical workshop of Davis and Watts, in Baltimore, by Mr. J. H. C. Watts, and in the establishment of Mr. Chas. Williams, jr., in Boston, by Mr. Thomas A. Gleason. To test the influence of size of coil an instrument was constructed in which the coils were no larger than the bullet for which we sought (as had been suggested by Prof. Newcomb),[10] and experiments were also made with the enormous coils used by the late Prof. Henry in his researches upon induction, which were kindly lent to me for the purpose by the Smithsonian Institution, but neither the small nor the large coils produced more satisfactory results than those we had already obtained.

Fig. 5.



To test battery power, twenty enormous Bunsen elements, which had formerly been used to light the gas at the Capitol, were placed at my disposal by Mr. Rogers, electrician of the Capitol, but while great electro-motive force was evidently of use we derived no advantage from such a battery as this.

To test the influence of speed of interruption, Mr. Marean, Supt. of the Western Union Telegraph Co. in Washington, kindly lent us an electric motor, by means of which we were able, with the aid of a rotating commutator, to obtain interruptions of the primary circuit of all rates up to 600 interruptions per second,[11] and we found that the more rapid the rate of interruption the more distinct was the sound in the telephone. The hearing distance, however, was not proportionately increased. The automatic interrupter (shown in fig. 5), yielding about 100 interruptions per second, gave as good results as any, and was much more convenient. This interrupter was therefore afterwards used exclusively in our experiments.

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[10] See Appendix, note 5.

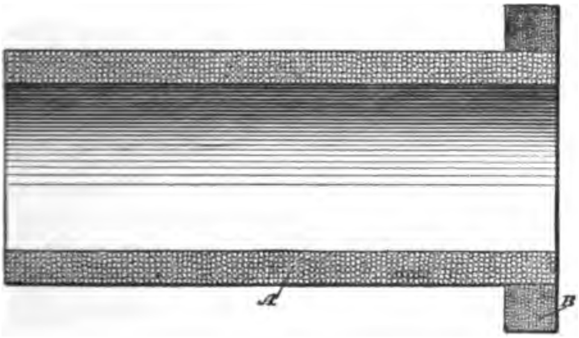
[11] Mr. Sumner Tainter has since made an apparatus operating in a similar manner by means of which he has obtained as many as 4,000 interruptions of the circuit per second.

The theoretical form of coil suggested by Prof. John Trowbridge[12] was substantially the same as that proposed by Prof. Rowland,[13] and is shown in fig. 6.

The arrangement was quite sensitive to metal placed in the interior of the coil, but the hearing distance for a bullet external to the coils was no greater than before.[14]

Professor Hughes[15] proposed to have two flat superposed coils wound on a single reel, so that the two coils should form a single one as regards their relative distance; and Mr. F. T. Bickford, Washington correspondent of the New York Tribune, suggested winding two wires side by side into a single coil, so that the relative distances of the wires from the bullet should be absolutely the same. Mr. Chas. E. Buell[16] and Dr. Chichester A. Bell[17] proposed to determine the depth of the bullet beneath the surface by causing a similar bullet to approach the balancing coils until silence was restored; the secondary bullet it was presumed would then be at the same distance from the balancing coils as the embedded bullet from the exploring coils.

Fig. 6.



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[12] See Appendix, note 6. [13] See Appendix, note 7.

[14] The balance obtained was not quite perfect, and we have since discovered that the insulation of the wires of one of the secondary coils was defective.

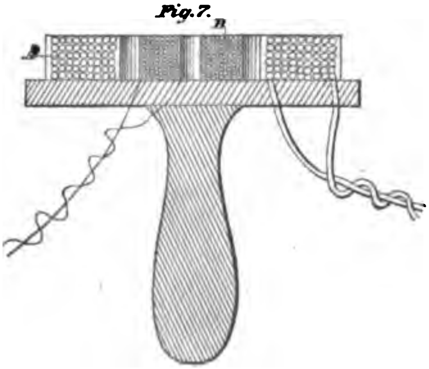
[15] See Appendix, note 8. [16] See Appendix, note 9. [17] See Appendix, note 10.

The results of all the experiments so far made were unsatisfactory. I had tried everything that had been suggested, but 4 cm. remained the extreme limit of audibility for a bullet like that which had struck the President. Even when such a bullet was flattened by being fired against a board, and was presented with its flat side towards the coils of the explorer — the most favorable mode of presentation — no better result was obtained.

Original Experiments.

In the theoretical arrangement recommended by Professors Trowbridge and Rowland (fig. 6) the primary coil A was of smaller diameter than the secondary B. This had given us no better effects than the ordinary form of Hughes' balance (see fig. 2), in which the two coils A B were of equal diameter. We then tried the effect of making the primary coil A of greater diameter than the secondary B (see fig. 7) and in this case we appeared to obtain an increase of hearing distance. Five centimetres (2 inches) was, however, the utmost limit reached, when, on the 19th of July, Mr. J. Stanley Brown and Dr. Woodward visited my laboratory and witnessed some experiments. No difficulty was experienced in detecting a bullet held in the mouth by passing the exploring coil over the cheek; and the presence of a flattened bullet held in the clenched hand was also readily determined. Dr. Bliss, Dr. Reyburn, and Surgeon-General Barnes visited the laboratory next day and expressed themselves as very hopefully impressed by the experiments. These were subsequently repeated in the surgeon's room at the Executive Mansion for the information of Dr. Frank Hamilton and Dr. Agnew, who also seemed favorably impressed.

Fig. 7.



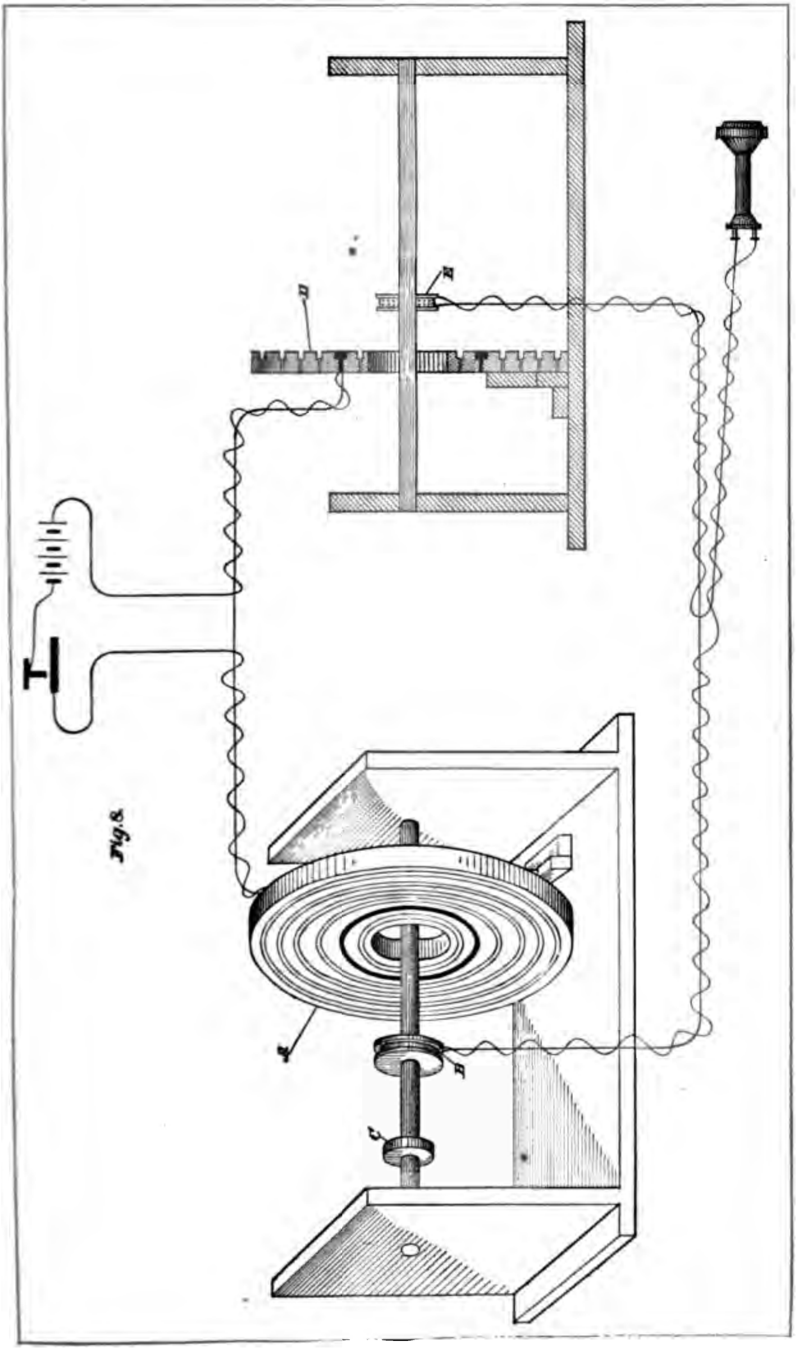


Fig. 8.

Such opinions from the surgeons in attendance upon the President, and the continued interest shown by Professor Newcomb, encouraged me to proceed with the experiments.[18]

It was now determined to test the effect of each convolution of the primary coil, so as to arrive empirically at some idea of the best shape of coil. For this purpose Mr. Tainter constructed the instruments shown in fig. 8. Circular grooves were turned in two boards, one of which is shown in perspective at A and the other in section at D. An insulated copper wire could be pressed into any of these grooves so as to give the wire an exactly circular shape of known diameter, and the two ends were passed through an orifice in the back of the board, making connection with a similar ring of wire in the other instrument as shown. A small secondary coil (B) of fine wire, which could be moved with moderate friction upon the horizontal rod, was connected to another similar coil (E) and to a telephone; and a small brass ring (C), which could also be moved along the horizontal rod, was used instead of a bullet to disturb the balance.

In making an experiment with this apparatus the secondary coil (B) was first placed within the primary ring and in the same plane with it, and the balancing coil E was adjusted to produce silence. The brass ring C was then moved along the horizontal rod until the balance was sensibly disturbed and the relative distance of the coils and the brass ring were noted.

Continuing the experiment, the coil B was moved a determined distance beyond the plane of A, and the balancing coils again adjusted to silence. The brass ring C was once more caused to disturb the balance, and the new hearing distance was noted. The following are the tabulated results of a series of experiments made on the 19th of July, 1881. The battery employed consisted of six bichromate cells connected in series.

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[18] I desire especially to express my gratitude to Dr. Frank Hamilton for words of encouragement spoken at a later date when sympathy and encouragement were greatly needed.

Diameter of Primary Ring.	Distance Between—		
	A B	B C	A C
mm.	mm.	mm.	mm.
30	0	17	17
	5	14	19
	10	13	23
	20	9	29
	30	7	37
	50	0	50
50	0	17	17
	5	19	24
	10	26	36
	20	17	37
	30	14	44
	50	5	55
81	0	21	21
	5	23	28
	10	23	33
	20	18	38
	30	14	44
	50	12	62
113	0	22	22
	5	25	30
	10	27	37
	20	26	46
	30	26	56
	50	17	67
159	0	27	27
	5	20	25
	10	18	28
	20	17	37
	30	14	44
	50	14	64
206	0	12	12
	5	18	23
	10	25	35
	20	19	39
	30	22	52
	50	18	68
253	0	20	20
	5	18	23
	10	20	30
	20	19	39
	30	23	53
	50	20	70

These figures show that *the distance from the primary coil* A (fig. 8), *at which the influence of the brass ring* C *became perceptible, increased with the diameter of the primary ring, and that the secondary coil* B *required to be projected considerably beyond the plane of the primary in order to obtain the maximum effect.*

The conclusion seemed a natural one that the degree of projection A B of the secondary coil should proportionally increase with the diameter of the primary ring, but the tabulated figures did not fully justify the inference.

The experiments had necessarily occupied a considerable time, and I thought that the difference between the results that should have been observed, according to the above hypothesis, and those that were actually obtained, might have been due to the gradual exhaustion of the bichromate battery employed and to its polarization, although every care had been taken to preserve its power by removing the carbon and zinc plates from the solution, excepting when an observation was made. To test whether the battery exerted any material influence upon the hearing distance, a further series of experiments was made with the same battery.

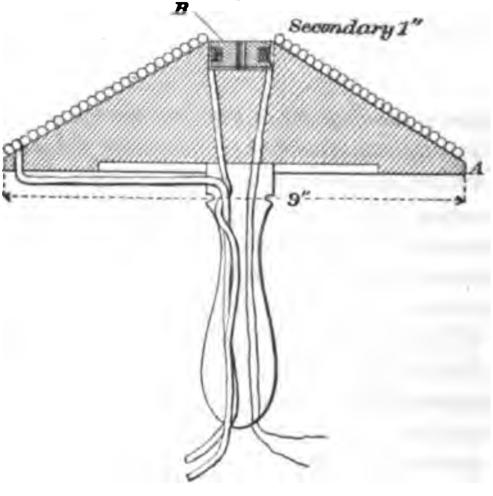
It will be seen by reference to the tabulated statement shown above that the maximum hearing distance B C had been obtained with a primary ring 11.3 cm. in diameter when the distance A B between the primary and secondary coils was one centimetre. This arrangement of the apparatus was therefore adopted throughout the following experiments:

	Hearing distance.
1. Apparatus tried with 1 cell (bichromate battery)	(B C, fig. 8) = 9 mm.
2. Six cells in series	(B C, fig. 8) = 16 mm.
3. Six cells in multiple arc	(B C, fig. 8) = 9 mm.
4. Six cells in two series of 3 each	(B C, fig. 8) = 15 mm.
5. Same experiment repeated	(B C, fig. 8) = 13.5 mm.
6. Same experiment repeated by Mr. Tainter	(B C, fig. 8) = 12.5 mm.

These experiments proved that battery power *did* exert an influence upon hearing distance, and also that the battery in use was gradually deteriorating.

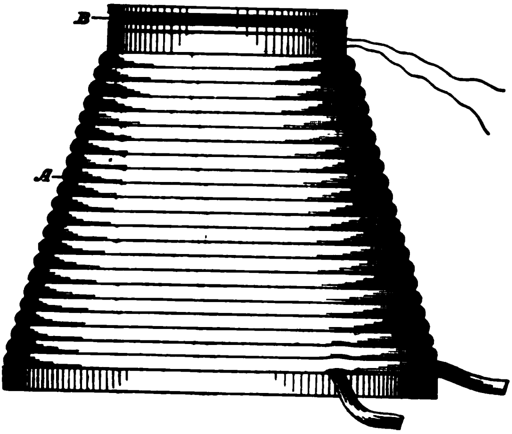
I concluded, therefore, that if the battery power had remained constant, the hearing distance might not only have been proportional to the diameter of the primary ring, but, in order to attain

Fig. 9.



the maximum effect, the projection of the secondary coil beyond

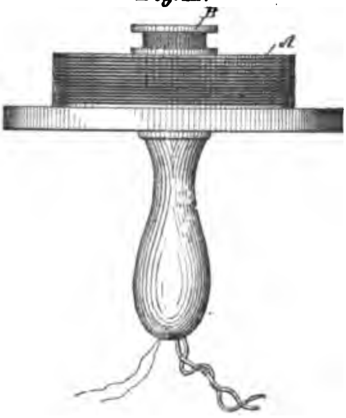
Fig. 10.



the plane of the primary might also have been found to increase in like proportion.

This led me to try the effect of a conical primary coil A with the secondary B at its apex, as shown in fig. 9, but the hearing distance for a bullet was only 3.5 cm.

Fig. 11.



Singularly enough Mr. J. H. C. Watts, in Baltimore, had independently arrived at a very similar form of coil, and with the instrument shown in fig. 10 he had obtained at one time a hearing distance of 7.5 cm. (or 3 inches[19]), but from some cause not ascertained he was unable subsequently to reproduce the effect.

The final form of apparatus adopted as the result of the above experiments is shown in fig. 11. With this arrangement and a battery of six bichromate elements freshly set up, we were always sure of a hearing distance of at least 5 cm., although after the battery had been in use for some time the hearing distance hardly exceeded 4 cm.

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[19] See Appendix, note 11.

The following are the dimensions of the coils A B (fig. 11) and their resistance:

COIL A......	External diameter	7 cm.
	Internal diameter	4.5 cm.
	Depth	2.4 cm.
Wire used, No. 23 (cotton covered). Resistance, 2 ohms.		
COIL B.....	External diameter	2.3 cm.
	Internal diameter	8 mm.
	Depth	8 mm.
Wire used, No. 36 (silk covered). Resistance, 75 ohms.		

The face of the coil B projected beyond the face of coil A 4 mm.

The balancing coils were made as nearly as possible the duplicates of A and B. The resistance of the coil of the telephone employed was 75 ohms.

Influence of Battery Power.

The following experiments were made with this apparatus (fig. 11) on July 20, 1881, to test the influence of battery arrangements upon the hearing distance of a leaden bullet.

I. *Series of experiments with a bichromate battery which had previously been in use for a few minutes.*

		Hearing distance of leaden bullet as observed by—	
		A. G. Bell.	S. Tainter.
		cm.	cm.
1 cell		2.4	2.6
2 cells in series		3.3	3.5
3 cells in series		3.7	4.1
4 cells in series		3.7	4.0
5 cells in series		4.0	4.1
6 cells in series		4.3	[illegible].4
6 cells in multiple arc		2.6	2.9
6 cells in two series of 3 each	— | o o o o o o | —	3.8	3.7
6 cells in three series of two each	— | o o o o o o | —	4.3	4.0

II. *Series of experiments with a Leclanché battery of twenty cells which had been set up for about one month. It had been kept normally upon open circuit, and had only been occasionally used.*

	Hearing distance.
20 cells in series	3.3 cm.
20 cells in 10 series of 2 each . . .	3.6 cm.
20 cells in 5 series of 4 each . . .	4.1 cm.
20 cells in 2 series of 10 each . . .	3.0 cm.

Although the battery appeared to be in good condition, a close inspection showed that the connections were dirty, and that one of the zinc wires was half broken through.

The defective cell was now removed from the circuit, and the connections of all the other cells cleaned and tightened.

III. *The following experiments were then made with the Leclanché cells united in series:*

No. of cells.	Hearing distance.	No. of cells.	Hearing distance.
	cm.		*cm.*
1	2.7	11	3.8[19]
2	2.8	12	4.2
3	3.0	13	4.2
4	3.3	14	4.2
5	3.3	15	4.3
6	3.5	16	4.2
7	3.6	17	4.2
8	3.8	18	4.2
9	4.0	19	4.2
10	3.8[20].		

These results are graphically represented in fig. 12.

It will be observed that the hearing distance was carried nearly two-thirds as far again as at first, simply by increasing the numbers of cells employed without any other change in the arrangement. It will also be noticed that the apparatus requires to be adjusted to complete silence in order to obtain the maximum effect.

As a general result of all our experiments with voltaic batteries, we find that *it is advisable to use a battery possessing great electromotive force and slight internal resistance, and to connect the cells in series.*

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[20] Balance not quite perfect.

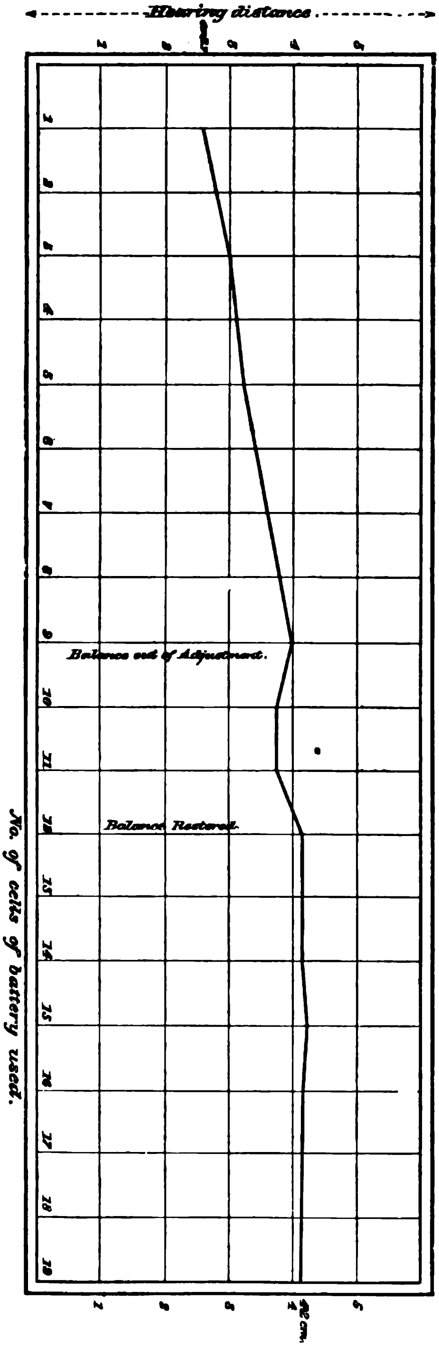
Hearing distance
1
2
3
4
5
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
Balance out of Adjustment.
Balance Restored.
No. of cells of battery used.
4.2 cm.

Fig. 12.

Experiments upon Living Subjects.

On the 22d of July an experiment was made at the request of Dr. Bliss upon the person of Lieut. Simpson, who had carried a bullet in his body for many years.

When the exploring instrument (fig. 11) was passed over the lieutenant's back a sonorous spot was found, but the indications were too feeble to be implicitly relied upon. Imagination very easily conjures up a feeble sound like that observed, but a number of experiments by different observers seemed to indicate that in this case there was an external cause for the sound—probably the presence of a very deeply-seated bullet. The results of this experiment were communicated to Dr. Bliss in a letter dated July 23, 1881.[21]

On the 25th of July Professor Rowland visited me at Washington, and suggested the use of a condenser in the primary circuit. I had previously discussed this idea with Mr. Tainter, but, not having a condenser at hand, we had been unable to make any experiment. After our conversation with Professor Rowland, however, we were so impressed by the importance of the point that we obtained a condenser next morning, and found it to produce not only a different quality of sound when the bullet approached the coils, but also to increase the hearing distance of the instrument shown in fig. 11 at least one centimetre.

On the evening of the same day (July 26) our apparatus was carried to the Executive Mansion, and an experiment tried upon the person of the President.[22]

From some cause then unknown a balance could not be obtained, and the results were therefore uncertain and indefinite. It was discovered afterwards that a mistake had been made in the mode of connecting the condenser. The latter should have been connected at E F (fig. 13), whereas it was placed at E G, thus influencing only one, instead of both, of the primary coils.

With the condenser properly arranged, experiments were tried on July 29 and 30 on three soldiers from the Soldiers Home who had been wounded during the civil war, namely, John Teahan, Asa Head, and John McGill. In the case of John Teahan no results were obtained. In the case of Asa Head, who had a buckshot in the cheek, loud and well-marked sounds were heard in the tele-

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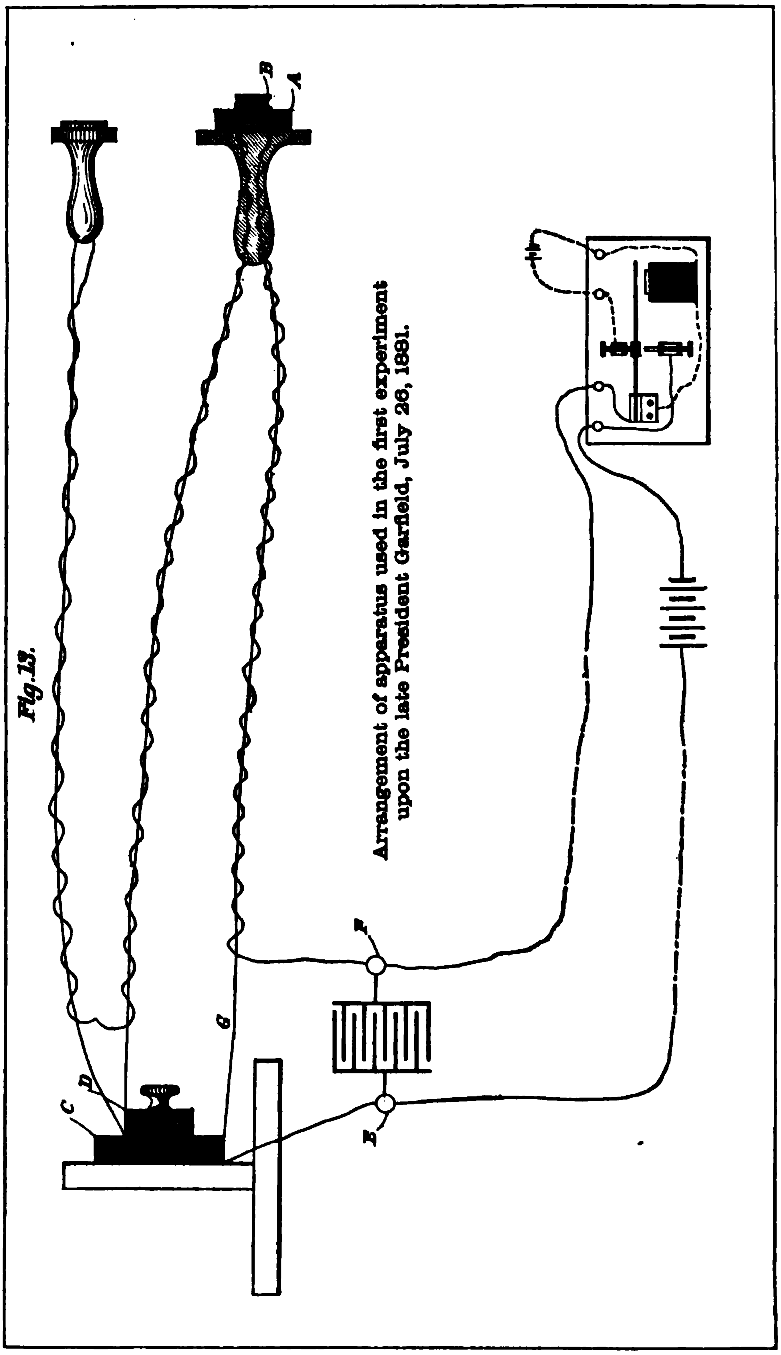
[21] See Appendix, note 12.

[22] See Appendix, note 13.

Fig. 13.



Arrangement of apparatus used in the first experiment upon the late President Garfield, July 26, 1881.

phone; and in the case of John McGill, who was supposed to carry a bullet in his back, no results were obtained.

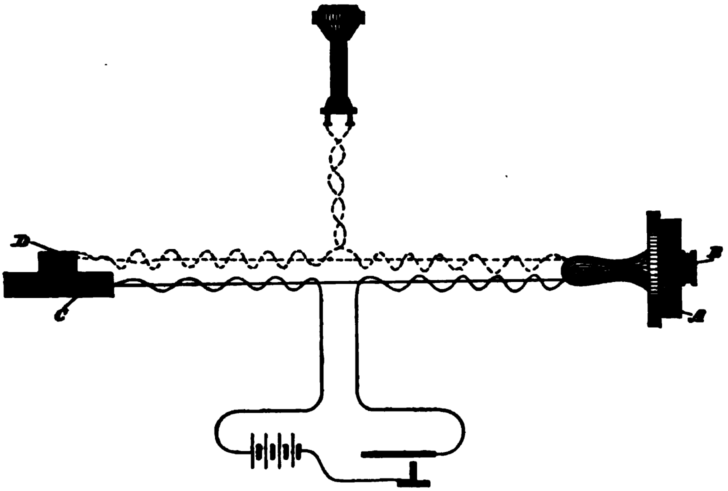
Further efforts were then prosecuted for the improvement of the apparatus.

Further Experiments to Improve Apparatus.

Our attention had hitherto been directed chiefly to modifications of the exploring instrument. We now investigated the effect upon the hearing distance, of the coils used to obtain a balance.

The following experiments, made July, 1881, bear upon the point:

Fig. 14.



Exp. 1. (See fig. 14.) Resistance of primary A of exploring instrument, 2 ohms; resistance of primary C of balancing coils, also 2 ohms; resistance of exploring secondary B, 140 ohms; and of balancing secondary D, 120 ohms.

Result: Hearing distance of bullet from explorer A B, 3.5 cm. Hearing distance from balancing coils C D, also 3.5 cm.

Exp. 2. (See fig. 15.) Same exploring coils as in Exp. 1, but balancing coils consisted of a flat primary, E — resistance, 5.30 ohms; and flat secondary, F — resistance, 83 ohms. The adjustment was made by sliding the secondary coil upon the primary until a position of silence was obtained.

mental could no longer be distinguished, but the high tone persisted, and was clearly audible up to a distance of 13 cm. The effect was very striking, and when the bullet was moved to and fro parallel to the plane of the coils E F at a distance of about 10 cm., the telephone emitted a shrill whistling sound each time the sensitive area (H) was passed.

It was noticed that other metals, such as iron, brass, and copper, did not seem to reinforce this high tone to any great extent, but brought out the fundamental at every distance where an effect was produced.

Exp. 7. The condenser G (fig. 17) was removed from the circuit and the leaden bullet held about 4 or 5 cm. from the coils E F. The fundamental tone was heard, and the characteristic upper-partial could also be distinguished, but it was only faintly audible. Upon now suddenly replacing the condenser, the high upper-partial tone was instantly reinforced as if by a resonator.

Exp. 8. The rheotome employed to interrupt the primary circuit (which had been placed in a distant room) was found to be vibrating badly. The reed I of the instrument (see also fig. 5) was rattling against its contact pieces, thus producing an impure sound, and I could distinguish amongst the upper-partials the tone that had been reinforced by the condenser. Upon screwing up the contact pieces so as to improve the vibration, I could no longer distinguish the particular upper-partial referred to, and upon returning to the room in which the coils E F (fig. 17) were placed, I could no longer detect the effects noted above in Exps. 6 and 7, and the hearing distance did not exceed 9 cm.

The peculiar effects obtained with the arrangement shown in fig. 17 *thus seemed to depend* (1) *upon a particular kind of vibration of the reed of the interrupter, producing a certain high upper-partial or overtone* (2) *upon the use of a condenser acting as a sort of electrical resonator for this tone, and* (3) *upon the use of the metal lead.*

Mr. Marean, of Washington, kindly lent me a number of condensers used by the Western Union Telegraph Co., and we found, upon connecting them with the coils E F, as shown in fig. 17, and, holding a leaden bullet near the coils, that each condenser reinforced a high upper-partial of different pitch. We arranged the condensers so that they could be successively introduced into the circuit with great rapidity. The effect was very curious, and sounded somewhat like a Scotch air played upon the bagpipes. The low hum of the fundamental could be heard continuously, like the drone of the bagpipe, while the higher tone changed its pitch with each change of condenser.

The pitch of the high tone reinforced seemed to depend upon the electro-static capacity of the condenser employed, but the exact relation between the two has not been ascertained. In experiments 5, 6, 7, 8, and the subsequent experiments described above, the battery employed consisted of six pairs of carbon and zinc plates of large area placed in a solution of bichromate of potash containing sulphuric acid.

The effects noted above were not produced satisfactorily when the battery was much run down, nor were they obtained with a Leclanché battery which had been set up for some time, but which appeared to be in good condition.

It is evidently necessary in order to produce this characteristic high tone to use a battery possessing considerable electro-motive force and slight internal resistance.

Our experiments had reached this stage when, on Saturday, the 30th of July, 1881, I was requested to make another trial upon the person of the President at the evening dressing of the wound.

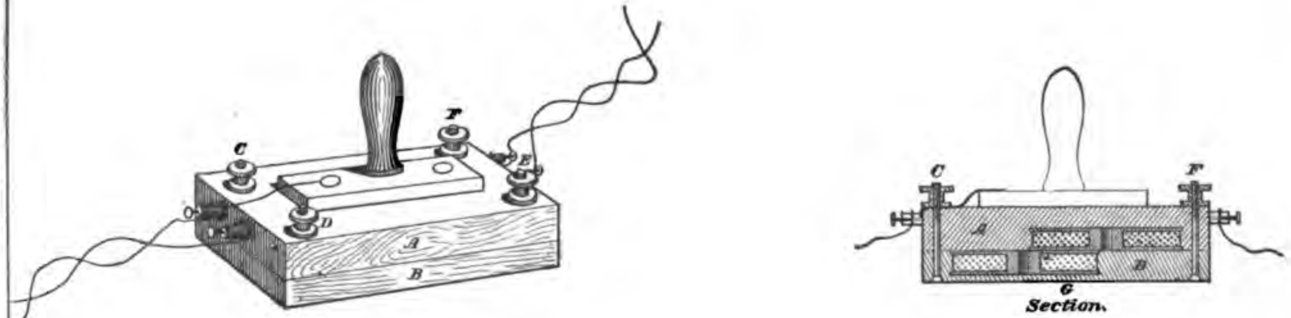
At this time, however, we had no exploring instruments completed excepting one or two like that shown in fig. 11; for it will be understood that the promising results noted above had been obtained from coils that were simply placed upon a table and adjusted by hand.

We immediately proceeded to the Executive Mansion with the apparatus shown in fig. 13, prepared to make a trial, if it was deemed advisable; but upon learning of the results of our later experiments, the surgeons resolved to postpone any further trial until we could arrange the coils (fig. 17) in a portable form.

By forced exertions the coils were arranged that same night in a wooden case, as shown in fig. 18. A shallow circular recess was turned out in each block for the reception of one of the coils, and the two blocks were held together by four pins of ebonite, C, D, E, F, which passed up through slots in the upper block and were secured by ebonite thumb-screws.

When the instrument was completed I found to my great distress that a balance could not be obtained by any adjustment of the apparatus. There was a position of minimum sound, and the tel-

Fig.18

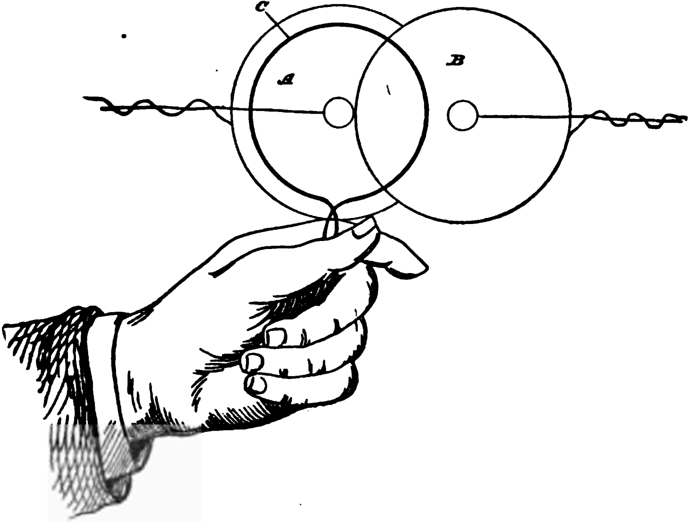


Apparatus used in the second experiment upon the late President Garfield, August 1st, 1881.

ephone responded to a bullet presented to the central part G of the instrument; but the hearing distance did not exceed 3 or 4 cm., whereas we had obtained with the same coils before the construction of the wooden case a perfect balance and a hearing distance of 13 cm.

After numerous unsuccessful experiments had been made to ascertain the cause of the difficulty it occurred to me that if two adjoining convolutions in one of the coils, made contact at any point, a circuit of low resistance would be formed (a single ring of wire, in fact), in which the induced currents might circulate without reaching the telephone connected with the apparatus. I had previously measured the resistance of the coils without discovering any defect, but when I considered the large number of convolutions in each coil it seemed possible that a defect of this kind might exist which could not be discovered by a Wheatstone Bridge, excepting by very delicate and accurate observations. To test whether a short-circuited convolution would produce effects analogous to those observed, a piece of copper wire was bent into an annular form and the ends connected together. On bringing this metallic ring near a pair of coils (A, B, fig. 19), properly adjusted to silence, the balance was loudly disturbed. The copper ring (C) was held as shown in fig. 19, and the balance could not then

Fig. 19.



be restored by any adjustment of the coils. A position of minimum sound was all that could be obtained, and the hearing distance was enormously reduced. This was *prima facie* evidence of the nature of the defect.

The coils (fig. 18) were then removed from their case, but a cursory examination revealed no defect. Upon trial, however (being arranged, as formerly, in fig. 17), a balance could not be obtained, and the hearing distance was only about 4 cm. The defect was thus definitely located in the coils themselves.

Upon close examination it was noticed that the outside convolutions of the primary coil were slightly frayed at one part, but it appeared hardly possible that so great a defect could be due to so apparently slight a cause. However, to test the matter, I removed the outside layer of wires and then tested the coils again.

Result: The defect had vanished — a perfect balance was obtained, and the hearing distance was again 13 cm.[23]

The coils were then replaced in their case and the completed instrument tested. The lower wooden block B (fig. 18) was adjusted by hand as nearly as possible to the position of silence, and then the thumb-screws C, D, E, F were tightened.

The balance now obtained was not quite perfect, but by striking the lower block B a few smart blows with a wooden mallet we were able to reduce the arrangement to complete silence.

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[23] These experiments have revealed the cause of the extreme difficulty always experienced in obtaining a perfect balance with coils of fine wire. I have recently used an Induction Balance to test the condition of the helices that were employed in these researches, and have discovered that in a large percentage of cases the insulation was defective. It is possible that some of the results described in this paper (especially of the earlier experiments) may have been vitiated by errors due to defects in the coils that were not suspected at the time. A defect of insulation that is quite immaterial for ordinary purposes may be absolutely fatal to the success of an Induction Balance. Indeed, so much care is required in this respect that it is extremely difficult to obtain coils that are perfectly suitable for an apparatus intended to search out a bullet embedded in the body. I now make it a rule to test every helix used in Induction Balance experiments by bringing it up to a system of balanced coils like that shown in fig. 19.

1. If the helix is perfect the balance is not disturbed until the terminals of the coil are connected.

2. If there is a break in any of the convolutions the balance is not disturbed, even when the terminals are connected.

3. If a convolution is short-circuited the balance is disturbed, even though the terminals are not connected, and the sound produced is the fundamental of the rheotome employed to interrupt the primary circuit.

4. If the insulation is defective the balance is disturbed, although the terminals are not connected, and a peculiar spluttering effect is noticed like that produced by a series of sparks.

I propose to apply this method practically as a means of testing the condition of the helices used in the construction of Induction Coils and those employed in the manufacture of telephones.

The instrument was then in such a sensitive condition that it could scarcely be moved without affecting the balance. Upon gently swaying it backwards and forwards a pulsation of sound was heard at every swing.

When the motion was carefully made, so that it was always in the same plane, no pulsations were observed. They only occurred when the inclination of the coils was changed.

This defect was found to be due to the bulging of the thin portion G of the wooden case (fig. 18) under the weight of the enclosed coil, and the simple pressure of a finger on this portion of the case disturbed the balance. The movement of the lower coil when the instrument was swayed about must have been inconceivably small, but on account of the extreme sensitiveness of the arrangement it produced a perceptible effect upon the balance.

The pulsating sound did not seem to interfere with the detection of a bullet held in the clenched hand, nor did it seem to affect the hearing distance. I therefore despatched a messenger to the Executive Mansion (Sunday morning, July 31) with a note for Dr. Bliss,[24] to let him know that the instrument was in a condition to be used, should any necessity arise for an immediate experiment. At the same time I informed him that the apparatus in its present form was very crudely constructed, and that I hoped to improve it very greatly in the course of a few days. On Sunday afternoon (July 31) we sent to the Soldiers Home for John McGill, upon whom we had experimented the previous day without results (using the apparatus shown in fig. 11).

Upon trying the new instrument (fig. 18) we had no difficulty in finding a sonorous spot in his back, at the place where the bullet was always supposed to be.

This result was at once communicated to Dr. Bliss,[25] and in reply we were requested to make the experiment upon the person of the President next morning.

On Monday morning (August 1, 1881) we accordingly removed our apparatus to the Executive Mansion.

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[24] See Appendix, note 14.

[25] See Appendix, note 15.

The Late President Garfield.

During the former experiment (July 26) a sudden sonorous effect had been observed upon passing a point near the spot where the surgeons suspected the bullet to be lodged, but I had been unable to verify this by a second observation, although the exploring instrument (A B, fig. 13) was repeatedly passed over the same place. The sound had been so loud and well marked that I believed at the time it must have been caused by a sudden irregularity in the vibration of the reed of the rheotome used to interrupt the primary circuit, for the arrangement (as explained above,) was not perfectly balanced, and any irregularity of this kind would, under these circumstances, have affected the telephone. At the same time the coincidence was remarkable that the exploring instrument should have been at that very time so near the suspected seat of the ball, and this led to the thought that perhaps after all the bullet had been the cause of the sound. I felt confident that the new instrument (fig. 18) would at once decide the question, for the extreme hearing distance of the former apparatus (fig. 13) was only 6 cm., and the apparatus shown in fig. 18 was so superior in this respect that if the sound had really been due to the bullet we should obtain with the new instrument distinct and well-marked effects. When the new explorer (fig. 18) was passed over the suspected spot nothing was heard excepting a slight pulsating sound as the instrument was moved to and fro. This was evidence to me that the former sound had been of accidental origin, whether the bullet was there or not. With a view of eliminating any error of observation caused by the pulsations due simply to the movement of the instrument, I lifted the latter (without changing the inclination of the coils) to a height of about fifty centimetres above the body of the President, and moved it to and fro in as nearly as possible the same way I had done at the lower elevation.

I presumed that if the pulsations heard were due simply to the movement of the instrument, they should occur with equal strength at the two elevations; but if any portion of the sonorous effect was due to the influence of the bullet, the pulsations at the two elevations would be different in intensity. I was struck by the fact that, although the sonorous pulsations were very feeble, they were sensibly louder when the instrument was close to the surface of the body than when it was raised. Continuing the exploration, I found a considerable area over which similar effects were noticed, but upon carrying the instrument towards the back of the President, the difference between the pulsations produced at the two elevations grew less and less, and finally could not be distinguished.

The difference in the loudness of the sound at the two elevations was so slight that it probably would not have been noticed by an ear unaccustomed to listen to feeble effects, and I feared that the general expectation that the bullet would be found in that part of the body might have led me to imagine a difference that did not exist. For the purpose of eliminating as far as possible any personal error, I requested Mr. Sumner Tainter (who was the only other person present whose ear had been sufficiently trained to be reliable in such an emergency) to repeat the experiments and let me know the result. Upon our return to my laboratory we compared notes, and I found that his observations tallied with mine. He declared he could not obtain a distinctly localized effect, but stated that he had observed a reinforcement of the pulsation over an area of at least two inches in the neighborhood of the spot to which his attention had primarily been directed, and that he was convinced that the bullet was within that area.

It appeared reasonably certain that the area of feeble sound was due to some external cause, and was not simply an effect of expectancy. In the absence of any other apparent cause for the phenomenon I was forced to agree in the conclusion that it was due to the presence of the bullet, and I so stated in my report to the surgeons.[26] I was by no means satisfied, however, with the results obtained, for no such effects had been observed before in our experiments with bullets. I tried to reproduce the effects by moving the instrument (fig. 18) at different distances over a bullet, but in every case where an effect was produced the sound was quite sharply localized. I thought that perhaps the body of the patient might have affected the result, and so experimented upon a bullet buried in a piece of meat, but no difference of effect was noted. This led me to fear that the extensive area of feeble sound might have been due to some extensive area of metal that was unsuspected at the time, and I proceeded to the Executive Mansion next morning (August 2) to ascertain from the surgeons whether they were perfectly sure that all metal had been removed from the

[26] See Appendix, note 16.

neighborhood of the bed. It was then recollected that underneath the horse-hair mattress on which the President lay was another mattress composed of steel wires.

Upon obtaining a duplicate, the mattress was found to consist of a sort of net of woven steel wires, with large meshes. The extent of the sonorous area having been so small, as compared with the area of the bed, it seemed reasonable to conclude that the steel mattress had produced no detrimental effect.[27] I was unable to continue experiments with the steel mattress, as just at this time I was obliged to leave Washington on account of illness in my family. Although I was unable for a long time afterwards to carry on personally Induction Balance experiments, the investigations were ably continued under my direction by Mr. Thomas Gleason, in the establishment of Mr. Charles Williams, Jr., in Boston.

Experiments continued in Boston.

Mr. Tainter forwarded from Washington drawings of an improved apparatus he had designed, to remedy the defects of the instrument shown in fig. 18, in which the case, adjusting screws, etc., were all to be composed of ebonite.

Mr. Gleason constructed for me a number of such ebonite instruments differing slightly from one another in detail, and the apparatus shown in fig. 20 combined the different points that had been approved.

The two coils A B were eccentrically arranged in two circular disks of ebonite, C D, and the adjustment was obtained by means of an ebonite key O, like the key used for tuning pianos, which turned a cam M pivoted in the upper disk and working in a slot N in the lower disk.

In order to prevent any movement of the coils, excepting that produced by the adjusting-key O, each coil was placed in a recess turned out in its ebonite disk, the edges of which were bevelled as shown at R. Paraffine was then poured in so as to fill up each recess. But this alone did not prevent a slight pulsation of sound when the instrument was swayed from side to side, and a very slight pressure of the finger on the thin portion of the ebonite plate under the coil B was sufficient to destroy the balance.

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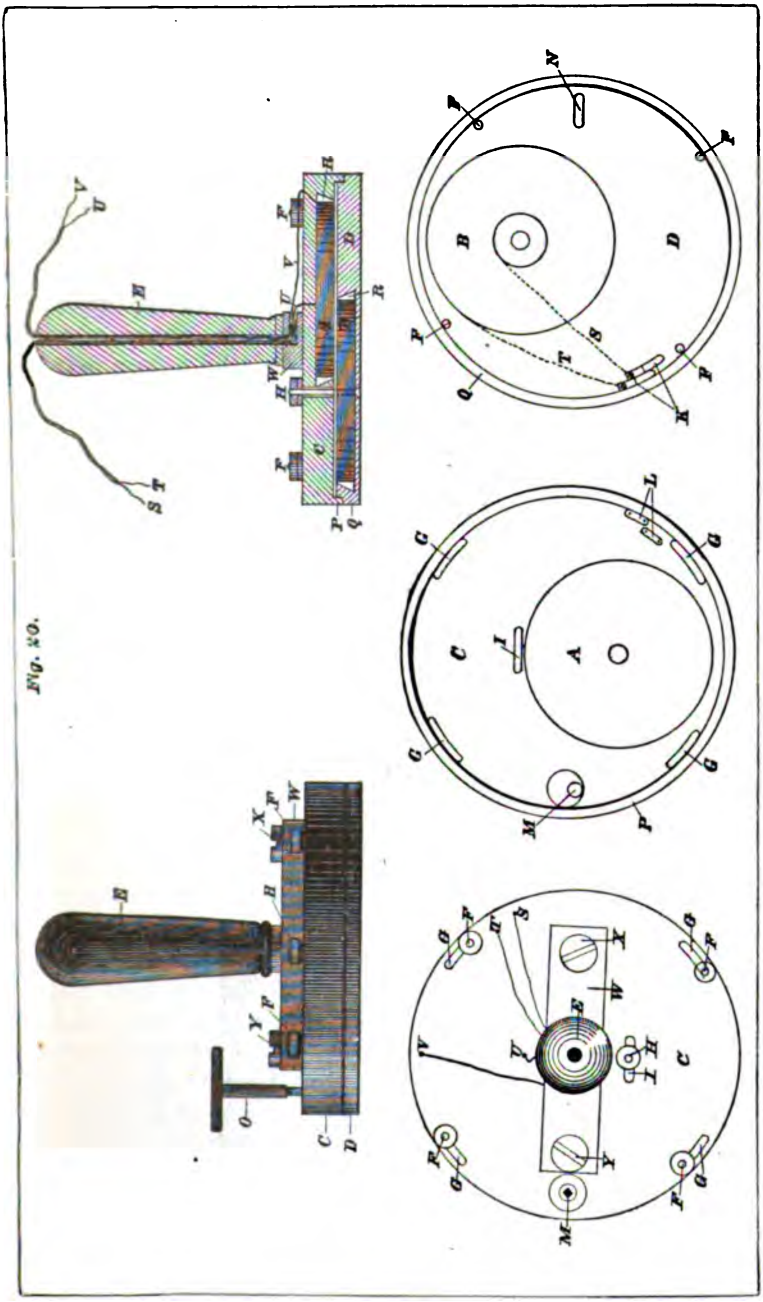
[27] The death of President Garfield and the subsequent *post-mortem* examination, however, proved that the bullet was at too great a distance from the surface to have affected our apparatus.

Fig. 20.


E
O
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X
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I
C
F
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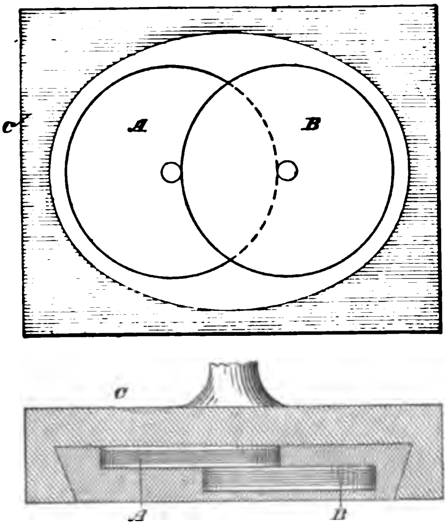
M

This was remedied by strengthening this portion by means of a rod of ebonite, which passed up through the centre of the coil and through a slot I, in the upper ebonite plate, and was clamped firmly after the adjustment of the instrument by an ebonite thumb-screw H. This, however, increased the difficulties of adjustment. When the coils were adjusted to silence, then the tightening of the thumb-screw H disturbed the balance; and if the thumb-screw H was tightened first, then the adjustment could only be made by a series of jerks, on account of friction. In practice we found it best to adjust the instrument *almost to silence*, and then the tightening of the thumb-screw H completed the balance.

Fig. 21.



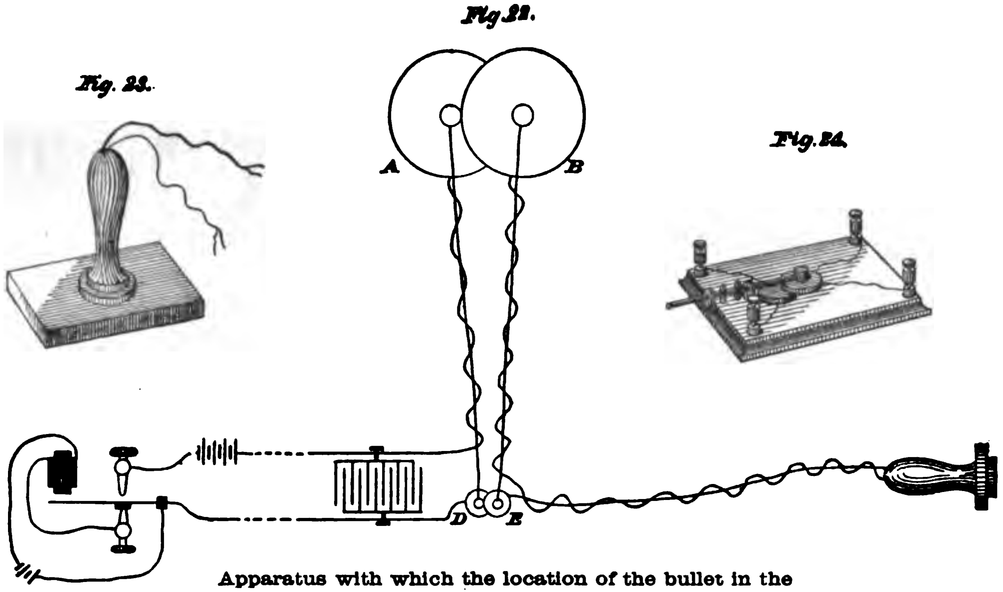
This was the form of apparatus at which we had arrived at the time of the death of President Garfield.

The difficulty of adjusting the coils led me ultimately to the idea of the apparatus shown in figs. 21, 22, 23, 24, which is the most practical form of the instrument yet devised.

The two exploring coils A B (fig. 21) are arranged as shown, in a recess turned out in a single block of wood C.

The coils are temporarily connected with a telephone, battery and rheotome in the manner shown in fig. 1, so that they may be adjusted by hand to form a balance. When they have been arranged in their position of silence the hollow in the block of wood C (fig. 21) is filled with melted paraffine. Upon cooling, the two coils are found immovably fixed in one solid cake of paraffine.

As a matter of practice it is found impossible to fix the coils in this way exactly in their position of silence; but by means of two other very small coils, D E (fig. 22), of insignificant resistance, forming a sort of fine adjustment external to the explorer, a per-



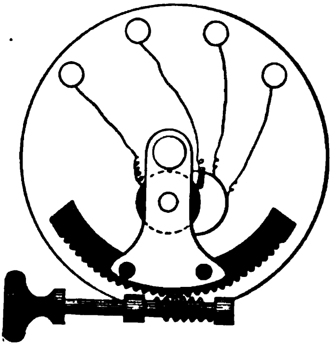
Apparatus with which the location of the bullet in the body of Col. Clayton was discovered, Oct. 7th, 1881.

fect balance is easily obtained. In this instrument the swaying of the coils A B produces no effect upon the balance.

The completed arrangement is shown in plan in fig. 22, and the explorer and balancing coils are shown separately in perspective in figs. 23 and 24.

On account of the small size and slight resistance of the balancing coils we were enabled to make the adjustable parts of the balancer of metal without practical interference with the sensitiveness of the exploring instrument, and this gave us the power of making very delicate adjustments of the balancing coils.

Fig. 25.



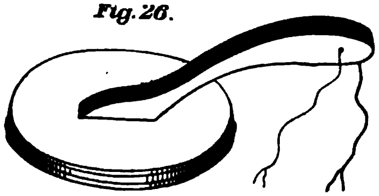
We found it advisable, however, to avoid placing metal over the sensitive area of the coils as had been done in the instrument shown in fig. 24.

In the balancing apparatus shown in fig. 25 (which is the most perfect one yet constructed), the lever to which the upper coil is attached is made of hard rubber.

In fig. 26 is shown the most convenient form of case yet devised for holding the exploring coils.

By invitation of Dr. Frank Hamilton, experiments were made at his office in New York, October 7, 1881, the instruments used being those shown in figs. 22, 23, 24.

Fig. 26.



As this was the first successful application of the Induction Balance to the discovery of the situation of a ball in the body, the position of which was previously unknown, I may be pardoned for entering somewhat into detail.

I shall quote from the *Medical Gazette*[28] of New York, an account of the experiments written by one of the witnesses:

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[28] See *Medical Gazette*, Oct. 15, 1881, pp. 347–349.

"*The First Successful Application.*

On Friday, Oct. 7, by invitation, several medical gentlemen,[29] including the writer, met Prof. Bell at the house of Dr. Frank H. Hamilton, in this city, for the purpose of witnessing the practical application of his improved instrument.

The first person subjected to experiment was General Calvin E. Pratt, judge of the supreme court of the State of New York, and who is now a resident of Brooklyn. General Pratt, at the battle of Gaines' Mills, June, 1862, this being the second day of the famous seven days' retreat across the peninsula, received a ball in his left cheek, which penetrated through the nares and was lodged in the right antrum. Its presence at this time was recognized by his surgical attendant, Dr. Damainville, and its exact position has been known from that day until this, it having given rise at times to much pain and suffering.

General Pratt has been seen by Dr. Hamilton and Dr. Damainville occasionally from that time forward, and they have from time to time urged upon him the necessity of its removal. General Pratt, however, was anxious to know whether Professor Bell's instrument would indicate its presence at the same point as declared by his surgeons.

The results of the experiment were conclusive and entirely satisfactory to General Pratt, the response being heard distinctly, but rather feebly, by every person present in the room. The feebleness of the response was supposed to be due to the fact that, owing to its situation and the peculiar form of the instrument containing the induction coils, it was impossible to bring the centre of its surface very near the site of the ball, the ball being situated very near the depression at the ala of the nose.

The next patient was Col. B. F. Clayton, who received a wound at the battle of Cedar Mountain, Virginia, Aug. 9, 1862.

The missile was supposed to be an Enfield rifle ball, and the wound was supposed to be mortal by the medical director of General Banks' staff and his assistants. The ball passed through the sternal end of the left clavicle, and was supposed to have lodged in the muscles under the superior angle of the corresponding scapula. The injury was followed by complete paralysis of the left arm, continuing for a period of six months ; and his arm has never yet been completely restored to its normal condition. He suffers a great portion of his time from pains in the arm, shoulder, and portions of the back.

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[29] "The following are the names of the medical gentlemen who were present, each one of whom verified personally the results and declared his entire satisfaction with every experiment that was made: J. C. Hutchinson, J. G. Johnson, and J. G. Allen, of Brooklyn; Elias Marsh, of Patterson, N. J.; Nathan Bozeman, J. H. Hunter, G. Durant, F. Delafield, L. Damainville, W. M. Chamberlain, J. H. Girdner. Frank H. Hamilton, and E. J. Bermingham, of New York."

Several small fragments of bone escaped through a fistulous orifice formed near the seat of the original wound.

About eighteen months later an abscess opened on the front of the chest below the fifth rib and to the left of the sternum. Through this sinus his surgeon was able to carry a probe upwards and backwards towards the top of the shoulder several inches, and which sinus was supposed then to communicate with the seat of the ball on the back.

Pleural adhesions were recognized by the medical attendants as having occurred in the upper part of the left thoracic cavity. He has been troubled occasionally ever since the injury with cough, expectoration, and violent palpitations of the heart. A suspicion has even been entertained that the fistulous canal which remained open a period of eighteen months, and then became permanently closed, communicated with the bronchial tubes, but at no time was a suspicion entertained by him or his medical attendants that the ball was not lodged in the back and there closely encysted.

We are disposed to mention as an evidence of Col. Clayton's loyalty and faithfulness as a soldier that within six months of the receipt of the injury, and while the wound was still discharging pus and blood, he returned to active duty with his regiment and remained in the field until the close of the war.

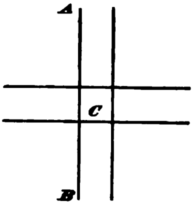
In the presence of the gentlemen assembled Col. Clayton exposed his chest, and Professor Bell proceeded to move the coils across that portion of his back where the ball was supposed to be situated, the colonel indicating the point underneath the superior angle of the scapula as that which had been fixed upon by himself and all the surgeons who had examined him as its exact seat. Although being buried underneath the scapula, they had not been able to verify their diagnosis by the sense of touch. Repeated examinations were made over this region, without any response, both by Professor Bell and several of the gentlemen who were present.

The instrument was then moved in every direction across the back and shoulders with the same result. There was an evident feeling of disappointment on the part of Professor Bell and all the gentlemen present, for no one entertained a doubt up to this moment that the situation of the ball was known and correctly stated by Col. Clayton.

It was not until the lapse of half an hour, and a thorough examination on the part of Professor Bell to determine if there was not some imperfection in the working of the apparatus, that it was suggested to move the instrument along the front of the chest.

This was done by Professor Bell, and immediately he exclaimed: 'I have found it!' And such was evidently the fact, as was verified by the personal examination through the telephone by every gentleman present. The response when the instrument was moved over the seat of the ball was loud and distinct, and left no room for doubt."

Fig. 27.



After all the visitors present had had the opportunity of verifying my discovery of the sonorous spot on the chest of Col. Clayton, experiments were made to determine as accurately as possible the exact position of the ball.

The exploring instrument (fig. 23) was first held over that part of the chest where the maximum sound was obtained. The instrument was then moved slowly towards the left until the sound could no longer be perceived. The position of the centre of the instrument was noted, and a vertical line (A B, fig. 27) was drawn with ink upon the skin through that point. This line indicated the boundary of the sonorous area towards the left. The experiment was then repeated by moving the instrument from the point of maximum sound towards the right, and also upwards and downwards, giving us the four boundary-lines shown in the diagram. (fig. 27). The bullet was thus located within a square, C, of about one inch.

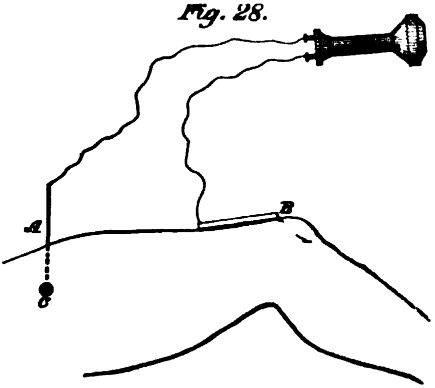
"The exact position of the ball," as described in the *Medical Gazette*, "was found to be within the thorax, probably in immediate contact with the inner surface of the ribs, the point being a little to the left of the sternum, between the third and fourth ribs.

and two or three inches above the cicatrix on the front of the chest. where the sinus, long since closed, had evacuated itself, and in a direct line from this cicatrix towards the left shoulder, which indicated the line of the track of the original sinus."

Experiments with Needles.

During my absence from Washington and from all conveniences for experimenting personally with Induction Balance apparatus, I devised a method of verifying the indications of the Induction Balance and of ascertaining the exact depth at which a bullet lies beneath the surface. This method was communicated through Dr. Woodward to the surgeons in attendance on President Garfield, and it was made the subject of a special paper presented to the French Academy of Sciences, Nov. 7, 1881.

Fig. 28.



This method, although involving extremely slight pain, would ordinarily be used only as a preliminary to an operation for the extraction of a bullet. The arrangement is shown in fig. 28. A fine needle A is connected to one terminal of a telephone, and the other terminal makes contact with a plate B, preferably of the same material as that composing the needle. Place this metallic plate B against the surface of the patient's skin and thrust the needle into that portion of the body where the bullet is believed to be lodged. When the point of the needle makes contact with the surface of the bullet C, a galvanic battery will be formed naturally within the body, the two poles of which are respectively the leaden bullet C and the metallic plate B. Under these circumstances a click will be heard from the telephone each time the bullet is touched by the needle. This has been verified by experiments upon bullets buried in a joint of meat. The click, though feeble, is unmistakable.

I have no doubt that this method of exploration alone, without the induction balance, would prove of great service upon a field of battle, where the employment of complicated apparatus is impossible. Mr. Thomas Gleason has recently communicated to me the particulars of an experiment he witnessed, in the course of which this method was tried upon a living subject. The surgeon who conducted the experiment was unable to obtain any response from the induction balance employed, although from certain indications apparent to the sense of touch he believed that the bullet was located in the part of the body submitted to experiment.

To verify his supposition a needle connected as above (fig. 28) was thrust into contact with the hard substance perceived, but no response was made by the telephone. The surgeon, however, believing that the bullet had been found, etherized his patient and proceeded with an operation, but discovered, when too late, that the bullet was not there.

Further Modifications of Induction Balance.

I sailed for Europe early in October, 1881, and have had no opportunity since of continuing my researches until quite recently. While I was in Europe, however, Mr. Sumner Tainter devised a new kind of induction balance which deserves mention here. The results obtained with this apparatus in its present form (fig. 29) are not to be compared with those produced by the best instruments described above, but there are undoubtedly great possibilities of future development.

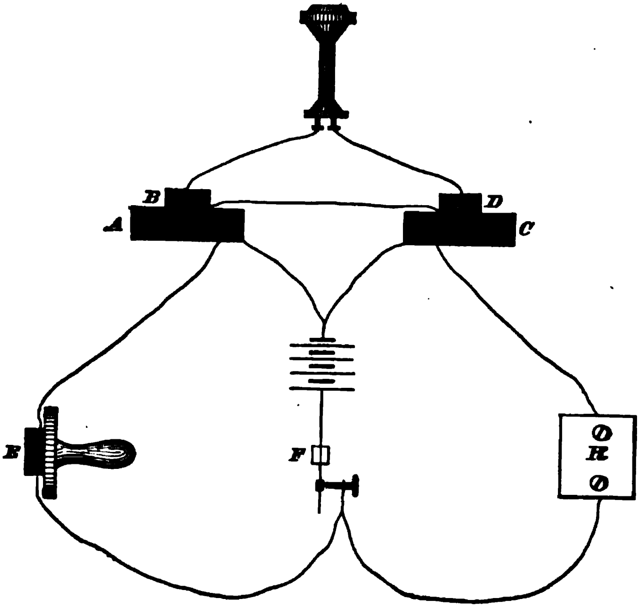
The important feature is that the exploring instrument E consists of *a single coil*, so that there is no possibility of any part of the explorer getting out of adjustment. All the adjustments are made upon the stationary part of the apparatus.

The current of the battery is divided between two equal circuits. One of the primary circuits contains the coil A and the exploring coil E, and the other circuit the coil C and a rheostat R. Coils A and C are exactly similar; and if the resistance introduced at R is equal to the resistance of the exploring coil E, an acoustic balance can be obtained by the adjustment of the secondary coils B D upon the primaries A C; but if the resistance introduced at R is different from that at E, Mr. Tainter states that no balance is possible.

When the apparatus is adjusted to silence the approach of a bullet to the coil E destroys the balance.

Although the great object of the researches that have been brought before you to-day has been to find that arrangement of balance which will detect a bullet at the greatest distance from the coils of the explorer, it must not be forgotten that in every case the instrument is more sensitive to the presence of a bullet placed *inside* the exploring coils than to one exterior to them. When, therefore, we seek the location of a bullet in one of the limbs, it may be advisable to use an annular coil large enough to slip easily over the leg or arm, as the case may be.

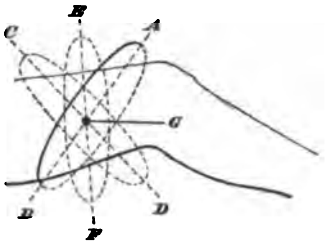
Fig. 29.



In Mr. Tainter's arrangement the exploring coil E (fig. 29) might simply be a large ring consisting of a number of convolutions of thick wire which could be slipped over the limb, or the ring might consist of two coils, forming one side of a Hughes' induction balance.

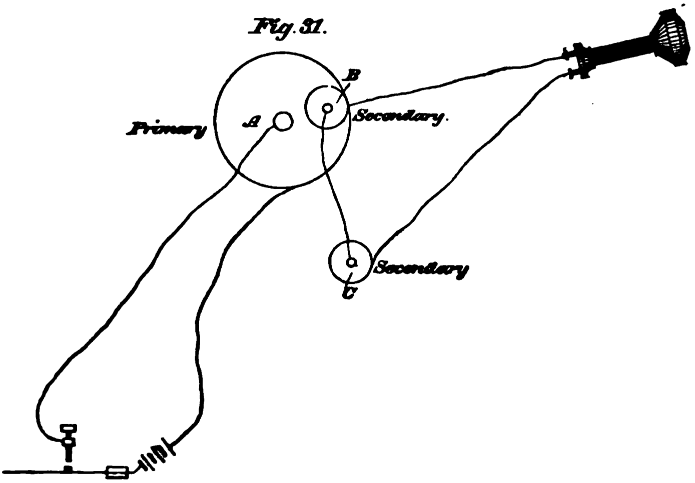
In either case the loudest sound will be produced when the bullet is in the plane of the ring, and its exact location should be deduced from three observations. Suppose, for instance, that with the ring inclined in a particular direction the maximum sound is observed when the ring occupies the position A B (fig. 30). We know then that the bullet is in that plane. Now, incline the ring in some other direction and explore again. Let the position of maximum sound be now C D. We know then that the bullet is somewhere on the straight line formed by the intersection of the planes A B and C D. It is only necessary then to make a third observation with the apparatus so inclined that the plane of the ring cuts this straight line, for instance, the position E F. The point of intersection of the three planes G is then the exact point occupied by the bullet.

Fig. 30.



I shall conclude this paper by the description of an experiment made in Newport, R. I., a few days ago. The results are so unprecedented in my experience that I feel they cannot be received as implicitly reliable until the experiments have been repeated and verified.

Fig. 31.



I had arranged upon a table three coils (as shown in fig. 31). The large flat primary coil A was connected with a battery of four Bunsen elements and an interrupter, as shown, and the two small secondaries of fine wire, B C, were connected with a telephone.

The secondary B was moved about on the primary A until a position of silence was obtained. Upon bringing a leaden bullet near C the balance was disturbed and a distinct sound produced from the telephone. There is nothing very strange about this when we know that the distance between A and C was only fifteen centimetres, so that C was well within the field of induction of A ; but what did seem extraordinary was that the approach of the large steel blade of a penknife to the coil C produced no effect! The iron diaphragm of a hand telephone brought close up to the coil C produced no sensible disturbance of the balance, whereas a small disk of lead produced quite a marked effect. A disk of copper the size of a telephone diaphragm also produced a good effect, but the sound was not sensibly louder than that due to the small leaden disk. A diaphragm of zinc occasioned a feeble, but distinct, disturbance of the balance. It is unfortunately the case that in all the forms of induction balance described above, lead gives the poorest effect of all metals. If people would only make their bullets of silver or iron there would be no difficulty in finding them in any part of the body! In the apparatus shown in fig. 31, however, it seems (unless subsequent experiments should reveal some fallacy) that we have an arrangement which is sensitive to lead and not to iron, or, at all events, which is more markedly influenced by lead than iron.

It is hardly necessary to state that when the coil C was removed to a considerable distance from the primary A, no effect was produced by the approach of metal to the coil C.

I have in this paper brought before you an outline of a labor of love pursued through many anxious days and sleepless nights. However imperfect or disappointing may be the results so far achieved, they are sufficiently encouraging to enable us to look forward with confidence to the attainment of still greater perfection.

I hope to continue these researches in the future ; and certainly no man can have a higher incentive to renewed exertion than the hope of relieving suffering and saving life.

APPENDIX.

NOTE 1. — Another mode of painless exploration suggested itself to my mind at this time, based upon the fact that a leaden bullet is much more opaque to light than the substances composing the human body.

I was aware of the fact that the ingenious M. Trouvée, of Paris, had, by means of his polyscope, produced a light inside a living fish, and was thus enabled in the dark to see the anatomical structure of the fish as it swam about in a vessel of water.

M. Trouvée had himself shown me a modified form of this apparatus for the illumination of the interior of the human stomach, and I understood him to say that when the instrument was used the body of the patient could be seen in the dark, faintly illuminated, like a Chinese lantern, and that the extent and location of tumors in the stomach could be detected on account of their great opacity.

It occurred to me that leaden bullets were certainly more opaque than tumors, and that a painless method of exploration might be based upon the observations of M. Trouvée.

It would evidently be impracticable, in the great majority of cases, to introduce into the stomach any illuminating apparatus; but if the light of a properly protected incandescent platinum spiral, introduced into the stomach, could produce effects visible from the outside — that is, if this feeble light could penetrate through the substance of one-half of the body — then why should not the intense light of an electric arc, or of a lime light, penetrate the whole body from one side to the other so as to produce similar effects?

The most feasible plan that occurred to me was to place an electric lamp at one end of a long opaque tube, and to apply the other end closely against the skin of the patient. The interior of the tube between two plate-glass diaphragms could be filled with a saturated solution of alum, or some other highly-absorbent transparent substance, so as to obstruct the passage of heat rays.

Of course, the whole apparatus was to be so arranged that no light from the lamp could escape into the room to interfere with the experiment.

Under these circumstances, the body of the patient, or at least a portion of it, should, in a dark room, appear self-luminous; and it seemed possible that the shadow of an embedded bullet might be projected upon the skin. The track of the bullet might also, perhaps, be discernible as an illuminated streak either more or less bright than the surrounding surface.

These considerations led me to try a number of experiments, which proved that the method was feasible where the bullet was very near the surface of the body. On account of the great and irregular refraction of light in passing through the tissues of the body, I doubt whether the shadow of a deeply-seated small bullet could be distinguishable upon the skin, unless, indeed, a very brilliant light emanating from a single point could be safely produced inside the body.

The following experiments bearing upon the subject were made in Boston, July 6, 1881, with the assistance of Mr. Wm. Schuyler Johnson:

Experiment 1.—Mr. Johnson placed in his mouth a glass cylinder containing a spiral of platinum wire, which was rendered incandescent by the passage of an electrical current. He also held in his cheek a small leaden bullet.

In a dark room the effect was very striking. The cheek appeared semi-transparent, and the location of the bullet was detected at a glance by a shadowy spot upon the skin.

Experiment 2.—A metallic glove button (not more than 4 or 5 mm. in diameter) was attached to the glass cylinder by a few turns of black thread passed round the cylinder.

In this case the effect, although not so marked as in the former experiment, was quite discernible, and a faint shadowy streak could be seen crossing the cheek, caused by the shadow of the black thread tied round the cylinder.

NOTE 2.—*Letter from Mr. George M. Hopkins, published in the New York Tribune, July* 11, 1881.

LETTERS FROM THE PEOPLE—THE BULLET FIRED BY GUITEAU.

A suggestion that the Induction Balance be used to discover its position in the President's body.

To the Editor of the Tribune:

SIR: The attempt upon President Garfield's life and the present condition of the sufferer have called forth the nation's deepest sympathies, and elicited very many suggestions as to the methods of promoting his comfort and assuring his recovery.

As one crisis after another has passed hope has risen, and the people wait with earnest desire for the last danger to pass away.

The attending and consulting surgeons state that it is impossible to predict the final result of the shot without locating the ball.

With all deference to the several methods proposed, I desire to suggest a method of ascertaining the position of the missile which will be painless and harmless, and which, in my experiments under conditions analogous to those under which it might be practised in the present instance, has proved successful. I refer to the use of the Induction Balance—a most delicate electrical instrument for detecting the presence of metals—a modified form of which could be easily applied in this case with a reasonable expectation of success. This instrument consists of two short glass cylinders, around each of which are wound two parallel coils of fine insulated copper wire.

One coil of each pair is included in a battery circuit, in which there is a clock microphone. The other pair is placed in a closed circuit with a receiving telephone. The two glass cylinders, with their encircling coils, may be widely separated.

The induction set up in the secondary or telephone circuit is balanced by the reversal of one of the secondary coils, and so adjusted that the induction in one of the secondary coils exactly balances or neutralizes the induction in the other, so that when the ear is applied to the receiving telephone no sound is heard.

Now, by placing ever so small a piece of metal in one of the glass cylinders, the electrical balance is disturbed and the clock on the microphone is heard to tick loudly, thus indicating the presence of metal, and the same is true if the coil be placed in the vicinity of a piece of metal.

It occurred to me to try the effect of a lead bullet upon the instrument, placing it at different distances, and separating it from the coil by insulating material. The result exceeded my anticipations, as with a set of coils that were by no means sensitive I was able to locate the bullet with the coils raised a vertical distance of nearly two inches. With more sensitive apparatus it is more than probable that the bullet might be located, even though distant several inches, by passing a pair of coils over the President's back and abdomen; and by comparative tests the depth of the bullet might be ascertained.

GEORGE M. HOPKINS.

BROOKLYN, *July* 10, 1881.

NOTE 3.—*Letter from Mr. G. M. Hopkins to Private Secretary Brown, enclosing printed description of Hughes' Induction Balance.*

OFFICE OF THE SCIENTIFIC AMERICAN, NO. 37 PARK ROW,
NEW YORK, *July* 11, 1881.

MR. J. STANLEY BROWN,
Executive Mansion, Washington, D. C.:

MY DEAR SIR: I send herewith a full description of the Induction Balance as promised in my note of yesterday. I will send the apparatus for trial if desired.

I am certain that the bullet can be located with it if it is not too deeply seated.

Please advise Dr. Bliss.

Very respectfully,

GEO. M. HOPKINS,
of the Scientific American.

NOTE 4.—*Letter from Mr. Geo. M. Hopkins to Private Secretary Brown accompanying the Hughes' Induction Balance apparatus he forwarded to the Executive Mansion for trial.*

60 IRVING PLACE, *July* 13, 1881.

MR. J. STANLEY BROWN:

DEAR SIR: I give below a few suggestions in regard to the use of the Induction Balance sent herewith.

Very respectfully,

GEO. M. HOPKINS.

Suggestions: Connect A to A, B to B, etc., as marked on the wires. Strength of current used, 15 or 20 volts.

A strong current extends the influence. Use the clock interrupter, and place a drop of mercury in the vulcanite cup to make connection.

The coils are now adjusted to "silence," but will probably have to be readjusted on their arrival in Washington; as a $\frac{1}{100}$ inch movement of one of the coils will throw it out of adjustment, it will be seen that the adjustment is a matter of great nicety. Two telephone receivers should be employed, and the most acute sense of hearing is required to distinguish the ticking when the ball is two inches distant from the coils.

The shorter instrument is intended for application to the patient should it be considered sufficiently sensitive to warrant the experiment.

As a preliminary experiment pass the induction balance over the pocket containing coins, keys, etc.; also over buttons, buckles, etc., attached to the garments.

Lead is the poorest of all metals to locate with this instrument. If the ball were of iron it could be readily found at a distance of 3 or 4 inches.

NOTE 5.— *Telegram from Professor Newcomb.*

EXECUTIVE MANSION,
WASHINGTON, D. C., *July* 18, 1881.

To Prof. A. G. BELL,
Care of Chas. Williams, 109 *Court St., Boston, Mass.:*

Perhaps small core for coil no larger than bullet, with very fine wire, might give best effect. Shall telegraph to Rowland to know best theoretical form. Telegraph me or Brown, care Executive Mansion, what train you take.

S. NEWCOMB.

NOTE 6.— *Telegram from Prof. John Trowbridge.*

BAR HARBOR, MAINE, *July* 16, 1881.

To Secretary Brown,
White House, Washington, D. C., for Professor Bell:

Make resistance of secondary coils equal to telephone. Put large number of turns of wire on primary and secondary coils. Primary coils long compared with width. Put secondary coils around middle of primary coils.

JOHN TROWBRIDGE.

NOTE 7.— *Telegram and letter from Professor Rowland to Professor Newcomb.*

HUNTER, N. Y., *July* 14, 1881.

To Prof. SIMON NEWCOMB,
Executive Mansion, Washington, D. C.:

Telegram just received. Make cores four inches long, with six layers of number twenty wire; diameter an inch and one-half. Make outer coil half an inch wide and half an inch thick, of finest wire, and slip over inner coil at its end.

H. A. ROWLAND.

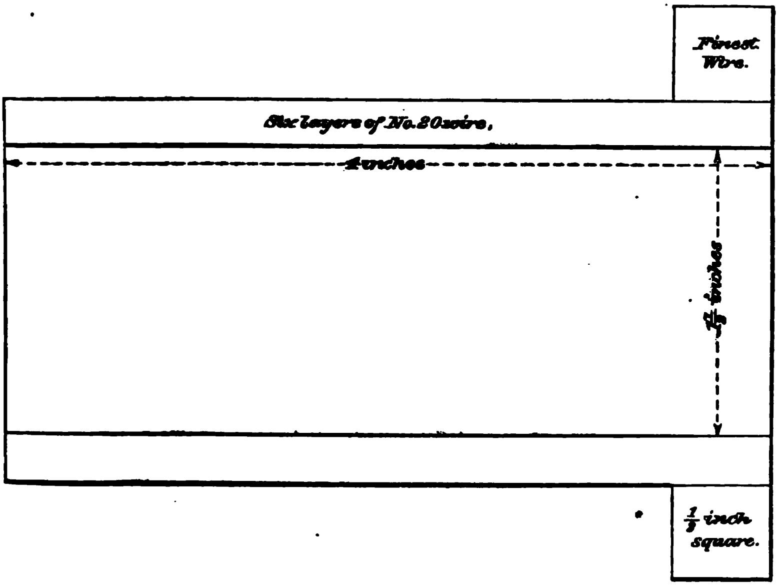
Letter following above telegram.

HUNTER, *July* 14, '81.

Professor NEWCOMB:

DEAR SIR: When your telegram arrived I had gone to the Hotel Kaaterskill, about ten miles from here, to stay over night. The telegram was sent back to the office to be sent to me, and my friends thought I had received it; but the stupid man here simply kept it and would not send it, because he had no authority to send it farther.

I have telegraphed back a reply. The dimensions should probably be about as follows:



Of course, the two must be precisely alike, and the induction coëfficients of one should be made to vary by a screw, which would move one of the secondary coils nearer to or farther from the end of the primary.

Yours truly,

HENRY A. ROWLAND.

P. S.—For these dimensions a telephone with many turns of fine wire in its coil would be best.

NOTE 8.— *Communications with Prof. D. E. Hughes, of London, England.*

(*a.*) *Cablegram to Mr. Preece, Superintendent Postal Telegraph, London, England.*

WASHINGTON, *July* 15, 1881.

TO PREECE,
London:

Can Hughes suggest form of Induction Balance to locate leaden bullet in President? If so, cable at my expense.

GRAHAM BELL.

(*b.*) *Telegraphic reply to above.*

WHITNEY,
Washington, D. C.:

Preece says to Graham Bell, Washington: "Interesting and hopeful letter from Hughes with diagrams sent you to-day."

W. GREEN,
Pres. West. Union Telegraph Co.

(*c.*) *Note from Mr. Preece enclosing letter from Professor Hughes.*

GENERAL POST OFFICE, LONDON, ENGLAND, 19 *July*, 1881.

MY DEAR BELL: The enclosed very interesting letter from Hughes, will enable you to make some experiments, which I trust will result in success.

Yours, sincerely,

W. H. PREECE.

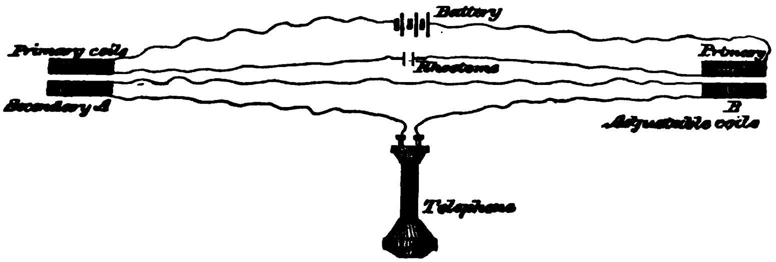
Prof. ALEXANDER GRAHAM BELL,
Washington, D. C.

(*d.*) *Professor Hughes to Mr. Preece.*

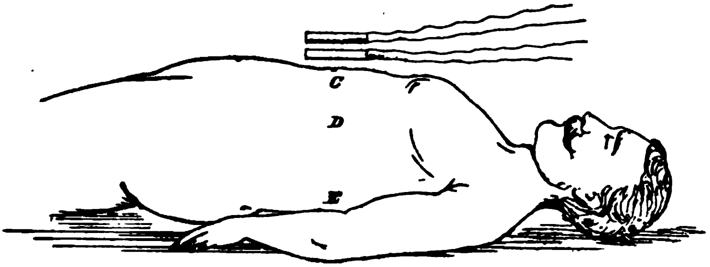
108 GT. PORTLAND STREET, W. LONDON, *July* 18, 1881.

DEAR PREECE: In reply to yours of to-day enclosing telegram from Prof. Graham Bell, I believe a specially-constructed Induction Balance could be made to locate the leaden bullet in President Garfield, provided the ball was nearer to one side of his body than the other.

Suppose we have two flat superposed coils on a single reel, so that these two coils form a single one as regards their relative distance; these coils, if connected with the usual adjusting coils of my balance, could be reduced to silence; then, on moving these coils near a metallic body, sounds would be heard. Thus the only difference would be instead of as usual taking the metallic body to the coils upon a fixed table, we take the coils to a fixed body in which we suppose metal, such as a bullet, to be hid.



Now, suppose we take coil A and move it over a body with a bullet, thus:



If bullet near C, we should hear it when coils just above it; at E, when the coils were below; but if bullet in the centre of the body, then we should have equal or probably no indications.

All this could be most easily done and with some results if the bullet had been of copper or silver, but lead has such a high resistance and gives, consequently, very feeble tones.

We can easily hear a copper penny at some inches distant from the coils, but lead requires that it should not be more than one or two inches distant: consequently, more sensitive coils would be required or a larger battery.

I think the experiment a hopeful one, and above all that the coils are easily made, and simple experiments could be first made to find a bullet in a mass of cotton, etc.

The microphone is invaluable as a probe for bullets. I made the first for Sir Henry Thomson to find out stone in bladder. But it is even more applicable to bullets. It consists of a simple hammer and anvil microphone adjusted upon the handle of the ordinary probe. The instant this probe touches any hard substance a loud sharp click is heard in the telephone. The smallest shot can thus be heard, and there is a very distinctive tone between when the probe strikes a bone or bullet. This instrument is well known to surgeons, so I need not say more about it.

If you write to Professor Bell, please enclose this, as I should be pleased to hear of his success.

Sincerely yours,

D. E. HUGHES.

W. H. PREECE, F. R. S.

NOTE 9.—*Letter from Mr. Charles E. Buell to Col. Rockwell.*

Col. ROCKWELL:

DEAR SIR: Please suggest for me that in an electrical test to locate the bullet it will only be done with certainty by obtaining a balance, which can be done as follows: When the loudest tick is obtained by placing the inducing coil in the vicinity of the embedded bullet, then move another bullet of equal size towards the other coil of the bridge until a balance is had and no tick is audible, when the embedded bullet will be known to be a like distance from its respective coil.

CHARLES E. BUELL,
Electrical Engineer, New Haven, Conn.

NOTE 10.—*Note from Dr. Chichester A. Bell.*

N. Y. CENTRAL R. R., BETWEEN SYRACUSE AND BUFFALO,
July 16, 1881.

DEAR A. G. B.: If your coils are symmetrically arranged, should it not be possible to balance the effect of a bullet on one side by a bullet placed in a corresponding position on the other?

Probably you have thought of this before, but, if not, it may be worth a three-cent stamp.

Yours, truly,

CHICHESTER A. BELL.

NOTE 11.—*Note and telegram from Mr. J. H. C. Watts.*

BALTO., *July* 21, 1881.

Prof. A. GRAHAM BELL,
1221 *Conn. Ave., Washington, D. C.:*

Am working hard at our problem. Succeeded in reaching full three inches last night, but owing, I think, to my battery weakening somewhat, can hardly reach so far now. Won't you please advise me how you are getting along, and oblige,

Yours, very truly,

J. H. C. WATTS.

Telegram.

BALTIMORE, *July* 23, 1881.

). C.:

to obtain satisfactory results since first trial and
east as good as mine.

J. H. C. WATTS.

NOTE 12.—*Letter to Dr. Bliss.*

LTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 23, 1881.

'on:

u were kind enough to suggest that preliminary ex-
nade with the Induction Balance upon the person of
whose address you gave me. I accordingly commu-
tenant, and he came to my laboratory last night in
nton.

ts we had hoped to use were not complete, so we
ves with the apparatus you tested the other day.
ils over the back of the lieutenant, it was found that
und made its appearance — too feeble, however, to be
as evidence of the presence of a ball. I find that
e that heard are easily conjured up by imagination
he following facts seem to indicate that in this case
an external cause — probably to the presence of a
l bullet:

ugh it could not always be distinguished, uniformly
le, *at the same spot.*

ependently by Mr. Tainter, Dr. Stanton, and myself
(If I recollect rightly, the lieutenant himself also
t my father could hear nothing.)

was then made. Mr. Tainter closed his eyes and
he coils were moved over different parts of the
he moment the sonorous spot was covered Mr.
ould hear the sound. We are to repeat the experi-
instruments as soon as they are completed, and I

Yours, truly,

ALEXANDER GRAHAM BELL.

NOTE 13.—*Letter to Dr. Bliss.*

WASHINGTON, *July* 27, 1881.

ermit me to make a few remarks upon the value of
Induction Balance in determining the location of
led in the human body.

Positive deductions concerning the location of the ball can only be safely made from positive indications of its presence. Negative indications may mean anything.

For instance: If we hear a sound from the telephone connected with the balance every time the exploring coil is passed over a certain part of the body, this may be taken as proof positive that a metallic mass is to be found in the neighborhood of the sonorous spot. If, on the other hand, we fail to obtain audible effects, we are not entitled to conclude that the bullet is absent from the part explored, or that it is embedded at a greater distance below the surface than the penetrating distance of the instrument used, for the bullet may be within any part of the area explored and close to the surface without affecting the balance if it is flattened, and if it is so located that the plane of its face is nearly perpendicular to the plane of the face of the exploring coil.

A few words may not be out of place here concerning the experiment made last night upon the person of the President.

Before entering the President's room, I applied the telephone to my ear, and heard a peculiar spluttering sound which it was found impossible to extinguish by any adjustment of the coils. I satisfied myself, however, that the arrangement was in a sensitive condition, by holding near it a flattened bullet. The hearing distance appeared to be about four cm. When the exploring coil was passed over the back of the President near the spine, no definite pulsation of the sound was heard; but when the coil was first passed over a portion of the front part of the abdomen near the right side, I heard at one point a sharp and sudden reinforcement of the sound. Upon moving the coil backwards and forwards a number of times over the same spot, I was unable to verify the observation. I should state hear that when a perfect balance is not obtained, it is not safe to place reliance upon a single observation of a pulsation in the sound, as a similar effect might be caused by an accidental irregularity in the vibration of the instrument used to interrupt the electrical current.

The sound heard was distinct and well marked, but it would not be safe to conclude that it was due to the presence of the bullet, unless the effect could be reproduced a number of times and always at the same spot.

I think I mentioned to you that it was discovered yesterday morning that the application of a tin foil "condenser" to the Induction Balance markedly influenced the hearing distance, increasing it in our experiments by about one centimetre.

A condenser was therefore connected to the arrangement used last night, but it has since been found that the condenser was only connected to one side of the balance, instead of to both. This mistake is enough to account for the difficulty experienced in adjusting the coils so as to obtain an acoustic balance, and for the observed reduction in the hearing distance.

If it is of importance to locate the bullet at once, I would recommend an immediate repetition of the experiment with the condenser properly arranged.

Whatever other results might be obtained, I feel sure we could, at all events, settle immediately whether the sound I heard was due to the presence of the bullet or was an accidental phenomenon.

If it is not important to locate the bullet at the present time, it might be well to postpone a repetition of the experiment, so as to give Mr. Tainter and myself more time to improve our apparatus.

Yours, truly,
ALEXANDER GRAHAM BELL.

NOTE 14.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

MY DEAR DR. BLISS: I write to let you know that my new form of Induction Balance gives brilliant promise of success. The indications with a flattened bullet are well marked and distinct at a distance of three inches, and audible effects can even be distinguished at five inches, but beyond three inches, silence and the greatest attention are requisite. Effects are produced at about two inches, when the bullet is held with its edge towards the instrument — a position that gave no results with our former apparatus.

Altogether I feel very much encouraged. The apparatus in its present form is a very clumsy affair, the surface that would be applied to the person of the President measuring seven inches by four. I hope to reduce the size of the apparatus very greatly in a day or two. In the meantime, should any necessity arise for an experiment upon the President, we have much better chances of success than at any previous time.

Yours, very truly,
ALEXANDER GRAHAM BELL.

NOTE 15.—*Letter to Dr. Bliss.*

VOLTA LABORATORY, 1221 CONNECTICUT AVENUE,
WASHINGTON, D. C., *July* 31, 1881.

Dr. BLISS,
Executive Mansion:

DEAR SIR: We have made experiments this evening upon the person of Private John McGill, an old soldier, who was wounded at the battle of Gaines' Mill, in 1862, and who still carries the ball that shot him.

I found no difficulty in finding a sonorous spot in his back, where undoubtedly the bullet lies embedded.

Mr. Tainter also located the bullet in the same place. Upon pressing with the fingers upon the spot a lump could be felt between two of the ribs. We experimented upon this same man yesterday, using a similar form of Induction Balance to that we tried upon the person of the President the other day, but could obtain no indications.

The new form of Induction Balance is so sensitive that a new difficulty is introduced by the effects produced by large metallic masses—for instance, gas lustres, iron fire-places, etc.

I think also that the earth's magnetism affects the result. We shall investigate these causes of disturbance to-night.

Yours, sincerely,
ALEXANDER GRAHAM BELL.

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16. *Report to the surgeons published in the daily papers Aug.* 2, 1881.

VOLTA LABORATORY, 1221 CONN. AVE.,
WASHINGTON, D. C., *Aug.* 1, 1881.

To the Surgeons in attendance upon President Garfield:

GENTLEMEN: I beg to submit for your information a brief statement of the results obtained with the new form of Induction Balance in the experiments made this morning for the purpose of locating the bullet in the person of the President. The instrument was tested for sensitiveness several times during the course of the experiments, and it was found to respond well to the presentation of a flattened bullet at a distance of about four inches from the coils.

When the exploring coils were passed over that part of the abdomen where a sonorous spot was observed in the experiments made on July 26, a feeble tone was perceived, but the effect was audible a considerable distance around this spot. The sounds were too feeble to be entirely satisfactory, as I had reason to expect from the extreme sensitiveness of the instrument a much more marked effect. In order to ascertain whether similar sounds might not be obtained in other localities I explored the whole right side and back below the point of entrance of the bullet, but no part gave indications of the presence of metal, except an area of about two inches in diameter, containing within it the spot previously found to be sonorous. The experiments were repeated by Mr. Tainter, who obtained exactly corresponding results. We are therefore justified in concluding that the ball is located within the above-named area.

In our preliminary experiments we found that a bullet like the one in question when in its normal shape produced no audible effect beyond a distance of two and a half inches, while the same bullet, flattened and presented with its face parallel to the plane of the coils, gave indications up to a distance of five inches. The same flattened bullet, held with its face perpendicular to the plane of the coils, produced no sound beyond a distance of one inch. The facts show that, in ignorance of the actual shape and mode of presentation of the bullet to the exploring instrument, the depth at which the bullet lies beneath the surface cannot be determined from our experiments.

I am, gentlemen, yours truly,
ALEXANDER GRAHAM BELL.

On Secondary Batteries. By George F. Barker, of Philadelphia, Pa.

[ABSTRACT.]

A SECONDARY battery is one which produces a current in consequence of a previous electrolytic condensation or polarization upon the surfaces of its plates. Electrolytic polarization and the production of a secondary current by means of it, are almost as old as the voltaic battery itself. In 1801, Gautherot, having used platinum or silver wires as electrodes in a solution of salt, noticed a momentary inverse current when the battery was replaced by a galvanometer. Ritter observed the same phenomenon in 1803 with gold wires. He constructed a secondary battery with pieces of gold separated by cloth disks moistened with salt water. After contact for some seconds with a Volta's pile of more pairs than itself, it produced a current to which Ritter gave the name of secondary current. Platinum, gold and silver, gave a more marked result than copper, brass, iron or bismuth; and no effect at all was obtained from lead, tin or zinc. Fifty disks of copper separated by disks of cloth moistened with salt or sal ammoniac solution, when charged with a Volta's pile of 100 pairs, gave a current which decomposed water and produced all the effects of the primary current. Volta himself, as well as Marianini, explained the results by supposing that the decomposition of the salt produced acidic and basic deposits upon the metal disks; a view which Becquerel apparently confirmed by immersing a platinum plate in an acid and another in an alkali and noting that the current was the same in direction as the secondary current. But in 1826 De la Rive obtained the secondary effect from platinum plates immersed in dilute sulphuric acid; and since no basic or acidic deposits were now possible, he regarded the phenomenon as a physical one due to a special or polarized condition of the plates themselves. The current he called a polarization current. In 1833–4, Faraday's researches on electro-chemical decomposition definitely established the proportionality of the current to the amount of electrolyte decomposed. He observed the "peculiar state" of the plates in his voltameter; and even noticed that when lead acetate was electrolyzed, metallic lead was deposited on the negative plate and lead peroxide on the positive; but he offered no explanation of these phenomena. In 1842, Grove, following out an observation of Matteucci, constructed a gas battery in which the current was produced from two plates of platinum immersed in oxygen and hydrogen gases respectively. In 1852, C. W. Siemens used two carbon plates in lead acetate solution as a secondary battery; obtaining, by the action of the current, lead on one of these plates and lead peroxide on the other. One cell of this battery decomposed water; so that the electromotive force of the current must have been nearly two volts.

In 1859, Planté began an elaborate research upon this secondary action, studying carefully the influence of the metal and of the solution employed. The maximum effect was given by lead plates immersed in dilute sulphuric acid; and in 1860, he produced secondary batteries of remarkable power by rolling two sheets of lead together, a strip of insulating material being between them, and placing them in a ten per cent sulphuric acid. When connected with a battery, the oxygen evolved upon the positive plate attacked it and converted its surface into peroxide; the hydrogen for the most part escaping. When now the current was reversed, the hydrogen reduced this peroxide to finely divided metallic lead and the oxygen attacked the other plate, producing peroxide. By such alternations, the metal may finally be corroded to any depth and the amount of finely divided lead and peroxide indefinitely increased. In Planté's experiments this forming process required three months. In 1881, Faure brought out a secondary battery in which, as in Planté's, lead plates in sulphuric acid were employed. But in order to avoid the tedious process of "forming," in which the deposits upon the plates were produced by electric action, Faure covered the plates at the outset with a layer of minium or red lead. Upon passing the current, this red lead was reduced to metallic lead on the one plate and oxidized to peroxide on the other; thus producing the battery charged and ready for use in a single operation requiring but a few hours.

The theory of the storage of energy by secondary batteries is not difficult. Energy is capacity of doing work and work is force acting through distance. Whenever two attracting bodies are separated work is done upon the system and energy is absorbed by it; and this whether the attraction overcome is mechanical, molecular or atomic. When an electrolyte is decomposed by an electric current, the current yields energy in proportion to the amount of the ions separated. The quantity of lead peroxide formed in a secondary battery represents the amount of energy stored from the current. So soon, however, as the separating force ceases to act, the ions reunite and the stored energy becomes again free. Since the amount of energy thus set free, like that originally stored up, is proportional to the quantity of matter concerned, it is clear that the amount of electricity obtained from a given secondary battery is proportional to the amount of electrolytic products deposited upon its plates. In theory, the electrical energy set free should equal that originally stored up; but in practice this result is never attained.

In its primary expression the chemistry of the secondary battery is simple. In the form made by Grove, two platinum plates are placed in acidulated water. Under the action of the current, oxygen is set free on one of these and hydrogen on the other,[1] and the plates are said to be polarized. The electromotive force between these plates is the sum of the contact-potentials involved. Its value is 1.45 volts and it measures the chemical attraction between oxygen and hydrogen. If now the two plates be connected by a wire, the oxygen and the hydrogen recombine and the energy of combination appears in the form of a secondary current. In general, however, the electrolytic action is accompanied by secondary chemical changes which render the phenomenon much more complicated. In the lead battery, for example, secondary products appear at both electrodes. The oxygen set free at the anode combines with the lead, the final product there being peroxide. The hydrogen evolved at the cathode reduces the oxide which it finds there and produces metallic lead. As before, the electromotive force in the battery is the sum of the contact-potentials; and this, in the case of lead and lead peroxide, is a little above two volts.[2] When now the plates are connected by a conductor, the entire process is reversed. Water is decomposed, and, since the immersed portion of the plates changes sign on discharge, the hydrogen now goes to the peroxide surface and oxygen to the finely divided metallic lead. The former is deoxidized to monoxide and the latter oxidized to the same condition. Simultaneously, a current flows through the conductor, the electromotive force of which is two volts, the strength of which is the ratio of this electromotive force to the resistances in circuit, and the duration of which depends upon the amount of secondary products deposited upon the plates during the charging. Beside this, however, another action takes place in the secondary battery which is of great importance. The sulphuric acid in which the plates are immersed attacks both the peroxide on the one plate and the metallic lead on the other and converts both into lead sulphate. Moreover, as fast as the monoxide is produced by the discharge of the battery, it also is converted into sulphate. So that upon complete discharge, the plates are precisely alike, both being covered with lead sulphate.

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[1] Undoubtedly the oxygen is produced by a secondary reaction.

[2] Planté observes that, after complete charging, the electromotive force may rise above two volts at the instant of removing the battery. But as it falls to this value at once on closing the circuit and remains so, he attributes the first result to the temporary production of a peroxidized substance, as hydrogen peroxide or persulphuric acid, at the anode.

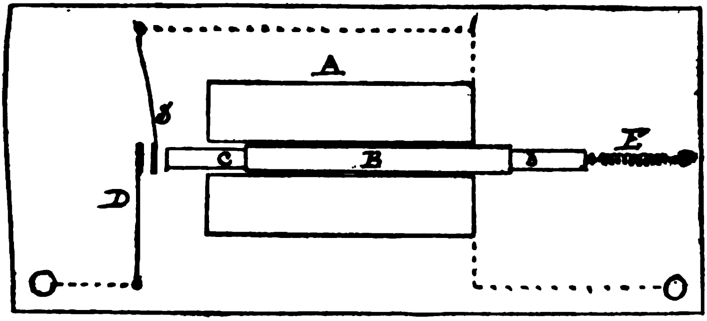
The secondary batteries employed for experiment were those of Planté and Faure. Of the former type, two cells of the larger size made by Breguet were used. Of the latter, one cell of medium size made by Reynier, and thirty-four cells of the size exhibited at the Electrical Exhibition at Paris, were experimented with. The battery of thirty-four cells was made in January under my direction.[3] Each cell consisted of six plates, each 20 by 60 centimetres, the lead being a millimetre thick. The exposed surface of each plate, therefore, was nearly 0.25 of a square metre, and of each cell about 1.50 square metres. For the whole battery the surface was about 50 square metres (exactly 48.96). Each plate was covered on both sides with a coating of red lead about a millimetre thick made into a paste with water. Parchment paper was then folded over the whole, and over this a covering of felt sewed together at the edges. The plates were then folded transversely and each pair intercalated. Heavy lead terminals were attached to each plate, and three pairs of plates were placed in each box, the terminals of one plate of each pair being united together and furnished with a connector. The boxes were lead-lined and measured 31 cm. in length, 25 in depth and 11 in width. One of the two sets of plates was in contact with the lead lining, the other was insulated. The latter was always made the positive end. The total weight of a single cell was 17 kilograms.[4]

[3] This battery, as well as the cut-out, Fig. 1, was made by Jas. W. Queen & Co., of Philadelphia.

[4] Mr. W. Lachlan, the electrical engineer of the "Light and Force Company," of New York, has kindly furnished me the following figures concerning the Faure batteries of improved construction recently imported in the steamer Labrador. Four types of Faure batteries are made in Paris, called A, B, C and D. Type A consists of seven positive plates, each 37 × 23.5 cm. and 3 mm. thick, perforated with holes 6 mm. in diameter 10 mm. apart, alternated with seven negative plates of the same size, but

The charging current was generated by a Gramme machine whose field was maintained by an auxiliary dynamo. The maximum obtainable electromotive force of this current was 85 volts. In general, however, a much less electromotive force was used, sufficient only to give a current of from 10 to 20 amperes through the thirty-four cells arranged in series. Currents as high as 45 amperes, however, were occasionally employed. At the outset, considerable difficulty was experienced from the discharge of the battery when from any accidental cause the current fell below the potential of the thirty-four cells. The wear of the brushes of the auxiliary dynamo sometimes opened its circuit and thus destroyed the magnetic field of the Gramme. There being now no counter electromotive force to oppose it, the secondary battery current flowed back through the machine, discharging the battery completely. A simple device was arranged to prevent loss of the charge in this way should the charging current become weakened. This device is represented in fig. 1, where A is a coil of No. 10 copper

FIG. 1.



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only 0.5 mm. thick, and similarly perforated. The seven positive plates weigh 16.66 kilograms; the seven negative ones 2.77 kilograms. The red lead spread on their surfaces weighs 22.18 kilograms, the two exterior plates being covered only on their interior surfaces, so that there are twenty-six active surfaces in all. Each plate has an electrode 7 cm. wide. The box is 45 × 20 × 40 cm. in size, is pitched throughout, and weighs with its cover, 10 kilograms. The battery complete, ready for charging, weighs 63 kilograms. In type C there are five positive plates each 24 × 19 cm., 8 mm. thick, and four negative plates 0.5 mm. thick, both perforated with holes 5 mm. in diameter, and a centimetre apart. The positive plates weigh 7.14 kilos, the negative 0.868 kilos. Sixteen surfaces are covered with red lead, weighing 8.28 kilos. The electrodes are 3.5 cm. wide. The box weighs 3 kilos., and the battery complete 20.37 kilos. Each cell of type A contains, it is claimed, when fully charged, 375 ampere-hours of current delivered at an electromotive force of 2 volts. Hence it will furnish, for example, a current of 25 amperes for fifteen hours. Type C contains 150 ampere hours. Types B and D are round cells in jars of earthenware. The former weighs 35 kilos., and contains 225 ampere-hours; the latter 8 kilos. and contains 75.

wire. Within its core is a rod of iron B, prolonged at both ends by brass rods of smaller diameter, *b* and *c*, these being supported upon rollers. Opposite the end of one of these rods is a flat spring S, faced with platinum and making contact when depressed with a similar brass strip D, also faced with platinum. The course of the current is shown by the dotted lines. When the circuit is closed between S and D, the current traversing the coil draws the iron bar strongly into its axis, and the end of the brass rod pressing against S holds the circuit closed. Should the current weaken, the spring S reacts against the rod pushing it back and opening the circuit. By means of an auxiliary spiral spring E, the instrument may be adjusted to any required current-strength. It would seem that by no possibility can the accumulated charge in the battery be lost, when this automatic cut-out is in circuit. As now described, the coil is of large wire, and the entire current passes through it. But a second form of it has been used in which the coil is made of finer wire and is placed in a shunt circuit as shown in fig. 3. In both cases of course, the entire current passes through the contact springs.[5]

After charging, the current from the secondary battery was used to maintain seventeen Edison incandescent lamps of the B pattern. More frequently, however, the battery was used in connection with the Gramme machine. The source of power being an Otto gas engine which, at best, has an explosion only every fourth stroke, the motion appears very irregular when tested by so delicate a method. As every explosion increases the speed, the higher electromotive force developed increases for an instant the brightness of the carbon filament, producing a marked pulsation in the light. If now a secondary battery of nearly the same electromotive force be placed in multiple circuit with the machine and lamps, it acts as an equalizer, and destroys entirely the pulsations. Figs. 2 and 3 show the arrangement actually employed, in which B is the secondary battery, and L the lamps; the automatic cut-out being at C. When the circuit is opened at *a*, *b* and *c* being closed, the lamps are fed by the machine alone; when opened at *b*, *a* and *c* being closed, by the battery alone; when at *c*, *a* and *b* being closed, the machine charges the battery. When all three are closed, the lamps are fed by the machine and battery together, the latter acting as an equalizer. A galvanometer inserted at *a*, *c* being opened and *b* closed, indicates the charging current; when *c* is closed and *b* open, the discharge current; and when *b* and *c* are both closed, the equalizing current. If the electromotive force of the machine-current be but slightly in excess of the battery current, the deflection to the right in the first case will about equal that to the left in the second; so that in the third case the deflection is practically zero, not the least spark appearing on opening at *a;* and this, though the pulsations are completely destroyed, the light being perfectly steady. The advantage of a much more agreeable light is thus secured; but in addition the life of the lamps must be increased by the use of this steadier current, as it has been shown to be by using the secondary battery alone.

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[5] Sir Wm. Thomson has devised a polarized cut-out for this same purpose. He exhibited it at the York meeting of the British Association in September last.

FIG. 2.

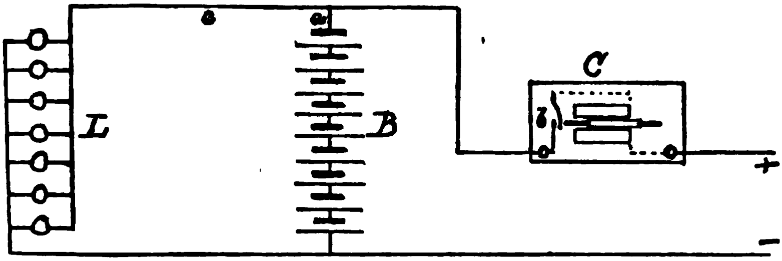
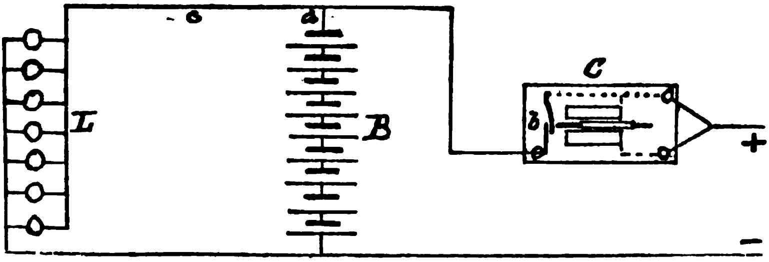


FIG. 3.



On the 27th of February the battery was charged continuously for fourteen hours, or until hydrogen was freely evolved from each of the thirty-two cells employed. It then gave an electromotive force of 59.3 Daniell cells; or calling the Daniell 1.08 volts, of

64.04 volts. On the tenth of June, the same thirty-two cells gave an electromotive force of 56.7 Daniell's, or 61.2 volts. On the 24th of June, their electromotive force was 54.3 Daniells', or 58.6 volts. It may be assumed then that the average electromotive force of a single cell was not far from two volts. This value has never been exceeded in any of the measurements made, either on single cells or on groups of cells. When first made, the resistance of the single cells was low, about 0.02 ohm; but subsequently the internal resistance increased considerably, rising even to twice this value.

The chief defect of the secondary lead battery is the local action which takes place in it. By local action is meant any deterioration in the efficiency of a battery which occurs when it is left on open circuit. This defect was noticed by Planté, who says: "If the lead peroxide deposited on the positive plate had no tendency to spontaneous reduction in the acidulated water, by forming a local circuit with the metal that it covers, the preservation of the charge taken by the secondary couple should be indefinite. But this peroxide is reduced, and with the more facility as the layer of it is thinner. In a new secondary couple the charge cannot be preserved." In a well formed battery he attributes the preservation of the charge to a reduction of the superficial layer to monoxide, which acts as a protecting coating. Another local action, however, takes place in this battery which appears to be of more importance. This is the action of the sulphuric acid upon the products of electrolysis. The finely divided lead on the negative plate, as well as the peroxide upon the positive, is alike converted into sulphate by this action. When the circuit is closed this action is normal; but it takes place equally well when the circuit is open. It is then abnormal and of course injurious. The formation of lead sulphate in the secondary battery has been studied by Gladstone and Tribe.[6] They found that with a Planté cell the whole of the peroxide formed on charging was converted into white sulphate within seventeen hours. The same fact was observed with the Faure battery. So far however, from regarding this formation of sulphate as wholly injurious, they regard it as "absolutely requisite in order that the charge should be retained for a sufficient time to be practically available." They believe that the insoluble lead sulphate clogs up the interstices of the peroxide and after a while forms an almost impermeable (*sic*) coating between it and the lead plate; so that the mass of peroxide which is in contact with the lead plate expends its energy slowly, while the same mass of peroxide, brought into connection through its own lead plate with another lead plate at a distance, expends its energy even through the sulphuric acid, in a tenth or a hundredth part of the time.

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[6] Nature, XXV, 221, 461; XXVI, 251, 1882.

My experiments with the Faure battery above described confirm entirely those of Gladstone and Tribe as to the formation of lead sulphate. In several cases, the acid in charged cells disappeared entirely on standing so that the water was tasteless; and in some instances this was repeated three times with the same set of plates. This action increased very greatly the internal resistance, first by replacing the acid by water and second by forming the higher resistant sulphate. On examining the plates, lead sulphate formed the entire coating upon both of them. On attempting now to recharge such a cell, this notable increase in the resistance showed itself in the reduced current transmitted by the same electromotive force. Gradually, however, the resistance fell and the current finally regained its normal strength. In one instance, the charging current rose from 2 amperes to 12, in four hours, 28 cells being in circuit, arranged in series. It was observed, however, that when the current was first applied, torrents of gas were evolved from both plates; and that as the charging process went on the oxygen was first completely absorbed and then the hydrogen. When the charge was nearly complete the evolution of hydrogen reappeared. There would seem, therefore, to be a very considerable loss of energy, due to the resistance consequent upon the formation of sulphate. In the first place the current energy is wasted as heat in the cells; and in the second, a large portion of it is dissipated in the evolved gases.

Another defect of considerable magnitude is developed when a number of secondary cells is placed in series, and is due to a want of uniformity in their capacity for storage. This arises probably first, from the difficulty of constructing them exactly alike originally, and second, from the impossibility of uniform action among them on charge and discharge. The difference in the cells of the same battery even, when they all have been treated precisely alike, is surprising. On the 14th of June, thirty-two cells were freshly charged, and the current strength from each cell measured on a tangent galvanometer. Eleven cells gave 80° deflection each, seventeen gave 79° each, one gave 78°, one 77°, one 75°, and one 20°. When seventeen Edison B lamps were placed in circuit, the discharge deflection of the thirty-two cells in series was 67°. On the following day the battery was charged again, and, after an hour's use on the lamp circuit, was again tested. Those giving a deflection of 87° were two; of 86°, two; of 84°, two; of 83°, one; of 81°, one; of 78°, five; of 77°, two; of 76°, one; of 13°, three; of 11°, one; of 10°, three; of 8°, one; of 6°, one; of 5°, three; of 4°, one; of 2°, one; of 1°, one; and of 0°, one. Obviously, so long as any peroxide is present, the electromotive force is constant; hence this wide variation of current strength must be attributed apparently to the variation of the resistance in the different cells. But this is not the worst aspect of the matter. If the discharge be continued for a longer time, some of the cells become exhausted sooner than others, and are then charged in the inverse direction by the current from the rest. On the 12th of June, the thirty-two cells, after pretty complete discharge, were measured as above. Nineteen cells gave positive deflections ranging from 87° to 5°; thirteen gave negative deflections varying from 13° to 1°. Faraday said of primary batteries in 1834, that "weak and exhausted charges should never be used at the same time with strong and fresh ones in the different cells;" and this remark is even more applicable to secondary batteries, where this condition is more liable to occur. Not only does a dead cell introduce resistance into the circuit, but what is of more importance, its reverse polarization introduces a counter-electromotive force, so that every such cell neutralizes another cell, the dead resistance of which is also added to the circuit. In consequence, it is not safe in practice to draw from a secondary battery all the energy it should be capable of yielding, and in most cases not more than half can be safely taken out of it. In place of continuing uniform as in a single cell, the electromotive force of a series of cells begins to fall when about half the charge which it ought to be capable of yielding has been drawn from it. The practical result is that to supply for lighting a definite number of coulombs of electricity, a secondary battery of twice the size is necessary under these circumstances.

With reference to the two forms of lead secondary battery devised by Planté and by Faure respectively, my experiments show that while the electromotive force is the same in both, and while the internal resistance and storage capacity may readily be made the same, yet that the local action is far less when the peroxide is formed by the action of the current upon the lead plate itself than when it is produced from minium spread upon it. The rapidity of the formation of the peroxide is proportional to the density of the current; but when rapidly formed it is fine and spongy. In one of my Planté cells, the peroxide is beautifully crystalline and very hard. Not a trace of sulphate has been formed in it apparently, though it has been in use for six months, and has been frequently charged and discharged during that time. The necessarily finer condition of the peroxide in the Faure battery would seem to predispose it to a more considerable local action. True, the Faure battery requires very much less time than the Planté to form it. But this is more than compensated by the increased permanence of the latter. With an equal storage capacity, as in M. de Kabath's form of Planté cell, it would appear that in efficiency it will surpass the form devised by Faure.

The commercial aspect of the question of the storage of electrical energy has recently assumed a very considerable importance. But it is evident at the outset, that an agent, which has been produced and then stored, must cost more when redelivered than when first produced, by exactly the cost of the storage; that is, supposing there is no loss in storing. But there is a loss, and this is almost if not quite one-half of the energy involved; as it would seem from the Conservatoire experiments in Paris, among others. The first cost of the accumulators, the expense of charging them, their low efficiency, and especially their bulk and weight, must ever prevent, it would seem, their competing successfully with the direct use of the dynamo-electric machine, at least with any form of secondary battery yet devised. For special uses, however, the storage battery has a high value. Even if the cost of an ampere of current produced by it is twice as great as if produced directly by the dynamo-machine, this cost is only one-half of that required to give the same current by any available form of primary battery. Where a strong current of low electromotive force is needed, under conditions where a dynamo cannot be employed, there the secondary battery has its most important field. Its value rapidly decreases as the number of cells is multiplied, as when a high electromotive force is needed for the production of light. Its chief advantage is its transportability; since by placing it near the work to be done, the loss of energy on long conductors is obviated.

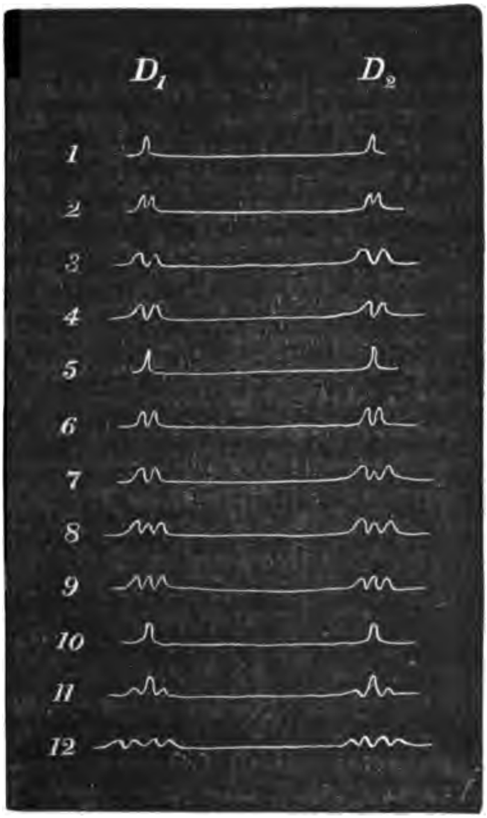
ON MODIFICATIONS OF THE SPECTRUM OF SODIUM VAPOR IN A BUNSEN FLAME. By CHARLES S. HASTINGS, of Baltimore, Md.

[ABSTRACT.]

AT the Boston meeting of the Association, Prof. Young described an experiment, with a sodium-tinted Bunsen flame having a lime-light behind it, which in some respects appeared to offer contradictions to the fundamental theory of spectrum analysis. To an explanation of the phenomenon in accordance with accepted theory which I suggested in a letter, he objected that it implied the existence of phases which he had not observed. To satisfy my own mind I tried a series of experiments shortly after, which presented some striking results, predicted, it is true, by the explanation which I had entertained, but worth noting.

In front of the slit of the large spectroscope belonging to the Johns Hopkins University, was placed a Bunsen burner with its top about two inches below the slit. This gave a flame seven inches in length, hollow for more than half its length and with well defined inner and outer walls. The dispersing member of the spectroscope was a Rutherford grating of over 30,000 lines, and the observations which follow were made in the spectrum of the second order with a magnifying power of 26. This power was quite sufficient to show the duplicity of b_3 and b_4, and even of E_1. Into the flame, a quarter of an inch below the level of the slit, was introduced a bead of fused chloride of sodium; a less volatile compound would not yield the results here described.

When the bead was gradually moved through the flame, starting from the side next the slit, the spectrum underwent a series of changes of which the typical forms are given in the cut from 1 to 10 inclusive. Here the spectral lines are represented by the ordinary convention, abscissas representing deviations and ordinates the corresponding intensities. Thus 1, 5, 10 corresponding to positions where the bead was hardly touched by the flame—5 being in the centre of the hollow portion—represent simply two sharp bright lines; 2, 3, 4, 6 indicate lines more or less widened with a narrower dark central line; 8 and 9 exhibit the lines still wider, so that they may be more properly called bands, striped by two black lines each; 7 is interesting as emphasizing a difference in the two sodium lines which are ordinarily so like. By lowering the flame until the top of the hollow centre was but just above the slit, a spectrum represented by 11, which may be regarded as a simple modification of 9, was obtained; and if, while in this position, another bead was introduced cautiously into the *side* of the flame and lower down, the phase described by 12 might be observed. This form of the sodium spectrum at low temperature, consisting of eight bright lines defining six dark ones, I have never had any difficulty in reproducing when the pressure on the gas mains was sufficient to give a good flame.



The explanation of these phenomena, though in some cases a little involved, is easy to one who has mastered the fundamental principles of spectroscopy.

Perhaps it can be given in the fewest words by formulating certain laws governing gaseous radiation, viz.:

(*a*) A gas can only produce a dark line by absorption when at a lower temperature than the source of radiation.

(*b*) Even then it can produce a dark line only when it exists in a sufficient abundance.

(*c*) The brightness of a spectral line can be increased in two ways, either by increasing the number of molecules which send the proper vibrations to the objective of the collimator, or by increasing the temperature of the gas of which the molecules are a part.

(*d*) By increasing the density of a gas, or by increasing its temperature at a constant density (in other words, by increasing the frequency of molecular impact), its spectral lines become broader.

These laws may be applied as follows: When the bead touches the flame at any one point of its four surfaces in line of axis of collimator, only a small amount of sodium is volatilized, and this rare hot vapor gives well defined lines. If the bead is gradually carried into the flame from the hither side the vaporization first goes on more rapidly and the denser gas yields widened lines, until, when the bead is fully enveloped by the flame, the copious evolution of sodium vapor causes it to overflow the limits of the flame and form a layer of cooler vapor between the hot flame and slit. This gives rise to the forms 2, 3, 4, according as the density varies in different portions. The form 6 corresponds to the case when the sodium bead enters the farther surface of the hollow cone within the flame. This, as is well known, is the hottest portion of the flame, though in this case its temperature is lowered over the solid chloride of sodium, the evaporation of the salt contributing to this end more powerfully perhaps than conduction; thus the physical conditions are not essentially different from those which obtained on the nearer side of the hollow cone and 6 should resemble 3 and 4. As soon as the solid is moved beyond this position, however, the conditions are greatly changed, for then there is a layer of gas at an extremely high temperature between the slit and bead, and very near the latter. If this layer is impregnated with a small amount of sodium vapor, we shall have, according to the laws which have been formulated, bright sharp lines superimposed upon the spectrum of the colder vapors beyond. These last, however, may give such a spectrum as 3, whence the forms 7, 8, 9. If the relative importance of the hot layer be increased by lowering the flame until the conical summit of the inner mantle is nearly opposite the slit, form 11 is easily reproduced. Finally, if, with this spectrum in the field of the spectroscope, we introduce a layer of rare and cool vapor between its source and the slit by means of a second bead, the last form can be secured with little difficulty.

SIEMENS' ELECTRICAL DEEP-SEA THERMOMETER. By J. R. BARTLETT, Commander U. S. Navy.

[ABSTRACT.]

THE principle of this new apparatus for obtaining temperatures of sea-water below the surface is based upon the variations in resistance of metals to the passage of an electric current arising from changes of temperature.[1]

The apparatus was constructed by Mr. Siemens of London, for the U. S. Coast Survey and placed on board the steamer "Blake." It consists of 2600 fathoms of double insulated cable, which is wound on a large reel on the upper deck. To the end of this cable is attached a resistance coil, which can be lowered to any depth desired.

The other ends of the insulated cable are brought to a room on the main deck and are attached to a Wheatstone bridge specially arranged for this purpose. Another resistance-coil of the same material and resistance as that lowered into the sea is likewise attached to the bridge. The latter coil has an attached thermometer and is kept in a copper vessel filled with water. A battery of twelve Leclanche's cells and a Thompson's marine galvanometer are attached to the bridge. The resistance-coil at the end of the cable being lowered into the sea just below the surface and some of the same water placed in the copper vessel containing the comparison-coil, the reflected pencil of light, when a current is sent into the bridge, will be at zero on the scale, as under these conditions both resistance-coils are in the same temperature. If the cable is then paid out and the resistance-coil at its end reaches any depth, say 100 fathoms, where the temperature is lower, the pencil of light will be deflected to the left, when the circuit is closed. Now, if cold water is added to that in the copper vessel containing the comparison-coil, with the circuit closed, until the pencil of light comes back to zero on the scale, it is assumed that the water around each coil is the same, and the reading of the attached thermometer of the comparison-coil will give the temperature at 100 fathoms, the depth to which the cable has been lowered. After taking a series of temperatures to 800 fathoms in this way, the cable was reeled in, stopping at intervals for temperatures; in this case, with circuit closed, the deflection of light was to the right, and it was necessary to raise the temperature in the copper vessel until the light was brought to zero on the scale.

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[1] A diagram, showing arrangement of apparatus, is given in Jour. of American Geographical Society, Vol. XIV, 1882; also in No. 20 of the Proceedings U. S. Naval Institute.

Water frozen by a Carré ice machine was used to reduce the temperature of the deck resistance coil.

Trials were made in latitude 36° 50′ N., longitude 74° 25′ W., during a perfect calm, with smooth sea and the ship's engine at rest, under which conditions the results of our several lowerings were very satisfactory. Having taken many thousand deep-sea temperatures with the Miller-Casella thermometers, my confidence in them was greatly strengthened when, after repeated lowerings, their readings agreed with the Siemens apparatus. Readings can be taken with the latter to one-quarter of a degree; but with the Miller-Casella it is difficult to decide on half a degree.

In a previous trial in the strength of the Gulf Stream, off Charleston, during moderate weather, with the vessel rolling 15°, it was found very difficult to read the galvanometer, and this could only be done when the engine was at rest. Under these trying circumstances, however, the temperature agreed with the Miller-Casella. I give an example of a single lowering off the Chesapeake.

	DEPTH IN FATHOMS.												
	Surf.	5	10	15	20	30	50	75	100	150	200	300	400
Siemens App ..	76½	76½	76½	69	58	54	54½	52½	50½	46½	43½	40½	40
Miller-Casella..	76½	76½	76	68	58	54	53½	52½	50½	46½	43½	40½	40

The above was taken on August 11; on the 12th the Siemens thermometer indicated a warm stratum of water between thirty and seventy-five fathoms; the Miller-Casella gave only the maximum and minimum, and did not indicate this. The temperature at twenty fathoms was 57° with both thermometers, at thirty, 52°; but at fifty fathoms the Siemens thermometer read 54½°, while the minimum of the Miller-Casella read the same as at thirty fathoms; at seventy-five fathoms the Siemens read 53°, the Miller-Casella still 52°; at 100 fathoms they agreed at 50½°. These observations were continued during the entire day with always the same results.

By lowering the Miller-Casella thermometer at the rate of 200 fathoms per minute to 50 fathoms, and hauling it by the 30 fathom depth at the same rate of speed, the temperature would read as high as 53½°.

AN INSTRUMENT FOR READILY PRODUCING LOW TEMPERATURES. By HENRY CARMICHAEL, of Brunswick, Me.

[ABSTRACT.]

THE instrument exhibited consists substantially of three test tubes placed within one another.

The outer tube is four centimetres wide and thirty-one centimetres long. The middle tube is of the same length and two and a half centimetres wide. Upon it have been blown a series of enlargements for increasing the surface, which, however, do not prevent a free passage of air between the tubes. The bottom of the latter tube contains numerous perforations which have been made by plunging a red-hot needle through the glass already softened before a blast lamp. This tube is held in place by a rubber ring which makes a tight joint at the top, and by a suitable aperture admits an air current to the annular space between the tubes. The bottoms of the tubes are one-half a centimetre apart.

The third tube is somewhat longer than the others and one centimetre wide. It is intended to hold the substance to be submitted to a low temperature.

For producing low temperatures, liquids are used which in a current of air blown through them are rapidly vaporized.

The operation of the instrument is as follows: 50 c c. of the volatile liquid is introduced in the middle tube. The aperture in the rubber ring is connected by flexible tubing with bellows, and the air-current thus introduced at the top of the narrow annular space between the outer and middle tubes flows to the bottom and there passing through the fine perforations beats the liquid into foam.

If ether or the petroleum distillate "gasolene" is used, the liquid is quickly reduced to—23°C. Six grammes of water placed in the inner tube is speedily frozen.

In conducting this experiment to save the tube, it is important to withdraw it before the water is congealed to the centre. The ice cylinder is easily shaken out of the tube after warming it in the hand.

By substituting more volatile liquids in the evaporator, lower temperatures are reached. Seventy-five grammes of mercury are almost instantly solidified in a bath of sulphurous anhydride.

When the vapors discharged are undesirable in the work-room, they are conveyed away through flexible tubing connected with an aperture in a cork ring which forms there a joint between the middle and inner tubes.

The effectiveness of this apparatus depends largely upon the fact that the escaping cold saturated current is made to cool the warm entering air before it comes in contact with the liquid to be evaporated.

Whatever moisture is contained in the air is deposited as frost upon the middle tube, and when the transparency of the latter is to be preserved the air is previously dried.

The instrument described is not merely for illustrating the fact that evaporation is accompanied by an absorption of heat, but is intended as a convenient means of securing for a short or long period the low temperatures necessary in many physical and chemical researches.

By a slight change in its construction, the perforations are placed above the bottom, and the region of intense cold remains free from the foam that might otherwise interfere with the direct observation of phenomena at the bottom of the middle tube.

UPON A PROPOSED METHOD OF PRODUCING ARTIFICIAL RESPIRATION BY MEANS OF A VACUUM JACKET. By ALEXANDER GRAHAM BELL, of Washington, D. C.

[ABSTRACT.]

I PROPOSE to surround the waist of the unconscious patient by a rigid jacket or drum somewhat larger in diameter than his body. The apparatus can be rendered practically air-tight by a rubber band round the thorax, and another round the loins. Upon exhausting the air inside the drum a partial vacuum is produced around the abdomen. Under such circumstances the pressure of the atmosphere forces air through the mouth and nose into the thorax causing the depression of the diaphragm and consequent expansion of the abdomen.

The alternate rarefaction and condensation of the air confined around the abdomen thus cause alternate inspiration and expiration.

RADIANT HEAT AN EXCEPTION TO THE SECOND LAW OF THERMODYNAMICS. By H. T. EDDY, of Cincinnati, O.

[ABSTRACT.]

THIS paper attempts to show that the second law of thermodynamics must receive some modification in the case of radiant heat, by reason of the fact that heat is not propagated through space instantaneously.

TITLES OF OTHER PAPERS READ IN SECTION B.

DANGERS FROM LIGHTNING INCREASED BY TELEGRAPH WIRES. By Stephen S. Haight, of West Farms, N. J.

ATOMIC PHYLLOTAXY. By Pliny Earl Chase, of Philadelphia, Pa.

THE TENSION OF THE LUMINIFEROUS ETHER. By De Volson Wood.

THE FORCE OF DIFFUSION OF THE GASES FORMING THE ATMOSPHERE. By George Iles, of Montreal, Canada.

*THE COLOR OF THE SUN. By S. P. Langley, of Alleghany, Pa.

*ELECTRIC INDUCTION BY STRESS. By S. W. Robinson, of Columbus, Ohio.

*A NEW SOIL-THERMOMETER. By D. P. Penhallow, of Mountainville, N. Y.

*ON TEMPERATURE OF THE SUN. By A. E. Dolbear, of College Hill, Mass.

ON THE TELEPHONE AS AN EXPLORER IN AN ELECTRIC FIELD. By A. E. Dolbear, of College Hill, Mass.

*ON THE CHARACTER OF HEAT. By A. E. Dolbear, of College Hill, Mass.

*The papers thus marked were only read by title owing to the absence of the authors when the papers were called.

ON ELECTRICAL CONDUCTION *versus* INDUCTION. By A. E. Dolbear, of College Hill, Mass.

ON TELEGRAPHING WITHOUT WIRES. By A. E. Dolbear, of College Hill, Mass.

ON THE CONSTITUTION OF MAGNETS. By A. E. Dolbear, of College Hill, Mass.

ON VORTEX RING PHENOMENA. By A. E. Dolbear, of College Hill, Mass.

EXHIBITION OF A SIMPLE AND INEXPENSIVE COMPARATOR FOR MEASURING DISTANCES BETWEEN THE LIMITS OF 1 MM. AND 1 DM. By William A. Rogers, of Cambridge, Mass.

EXPERIMENTAL DETERMINATION OF THE LIMITS OF ACCURACY IN MEASUREMENTS BY THE SENSE OF FEELING. By W. A. Rogers, of Cambridge, Mass.

DETERMINATION OF THE RELATION:— METRE DES ARCHIVES = IMPERIAL YARD + 3.37015 INCHES. By W. A. Rogers, of Cambridge, Mass.

NOTE ON THE APPEARANCE OF A HALO ON THE EVENING OF AUGUST 4, 1882. By James D. Warner, of Brooklyn, N. Y.

ON SOME PHENOMENA OF DIFFRACTION DUE TO THE SHAPE OF THE SOURCE OF LIGHT. By Brown Ayers, of New Orleans, La.

THE INFLUENCE OF THE PHASES OF HARMONICS ON THE TIMBRE OF SOUND. By Rudolph Kœnig, of Paris, France.

ON THE REDUCTION OF THE ELECTRICAL RESISTANCE OF THE CARBON BUTTON BY THE PASSAGE OF AN ELECTRIC CURRENT. By T. C. Mendenhall, of Columbus, Ohio.

CONCAVE GRATINGS FOR USE IN SPECTROSCOPIC WORK. By H. A. Rowland, of Baltimore, Md.

ON THE "ROTATIONAL COËFFICIENT" IN GOLD, IRON, ETC. By Edwin H. Hall, of Cambridge, Mass.

ON THE DIATONIC SCALES OBTAINED IN THE CHROMATIC SCALES OF EQUAL TEMPERAMENT OF TWELVE, NINETEEN AND THIRTY-ONE TONES IN THE OCTAVE, WITH EXHIBITION OF NOVEL SCALE-INDICATOR AND CORRESPONDING KEYBOARD. By P. H. Van der Weyde, of New York, N. Y.

SECTION C.

CHEMISTRY.

ADDRESS

BY

H. CARRINGTON BOLTON,

VICE PRESIDENT, SECTION C.

FELLOW MEMBERS OF THE CHEMICAL SECTION;

Ladies and Gentlemen:—

THE recognition by the Association of the equal rights of chemical science and the elevation of the late "Permanent Subsection" to the dignity of a "Section of Chemistry," now assembled for the first time, mark an important epoch in the annals of our organization. Permit me to offer congratulations on our promotion and to express my high appreciation of the honor of presiding over your deliberations.

The Permanent Subsection of Chemistry was organized at the Hartford meeting of the Association in 1874. It had its origin in the action of a number of chemists assembled at Northumberland in August of the same year to commemorate Priestley's discovery of Oxygen. At that memorable gathering a discussion of the advantages of forming an independent organization in the interests of chemistry led to the appointment of a committee* "to co-operate with the American Association for the Advancement of Science at their next meeting, to the end of establishing a chemical section on a firmer basis."

This committee met at Hartford a few weeks later and united with the chemical members of the Association in founding a Permanent Subsection in accordance with the provisions of the new constitution adopted at the same meeting. Meetings of the subsection were held on two days and arrangements were made for its permanent organization. In the following year the Association met at Detroit and the Subsection re-assembled under the chairmanship of Prof. S. W. Johnson. Before adjourning the section elected Prof. Geo. F. Barker chairman for the ensuing year and passed a resolution requesting the chairman-elect to prepare an address. This was the origin of the custom which it is my pleasant duty to follow. Professor Barker accepted the task and his masterly essay on the Atom and the Molecule (1876) is remembered by many in this audience. Since then Prof. F. W. Clarke (1878) has urged in your hearing the endowment of laboratories of research; Prof. Ira Remsen (1879) has magnified the claims of his chosen field of investigation — organic chemistry; and Prof. J. M. Ordway (1880) has gracefully surveyed the experiences of the past, the needs of the present and the prospects of the future of chemistry.

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*The committee consisted of Profs. Silliman, Smith, Horsford, Hunt and Bolton.

The amendments to the Constitution, adopted by the Association at the Cincinnati meeting, provide, as you are aware, for nine sections, each with its own presiding officer; since this may involve an equal number of annual addresses, it is hardly to be expected that each will maintain the elaborate character of those of former years.

I ask you therefore to bear this fact in mind while I invite your attention to a rather superficial survey of

CHEMICAL LITERATURE.

The literature of chemistry, extending as it does through a period of more than fourteen centuries, varies greatly in character, in province and in design; it partakes of the peculiar phases exhibited by the science at different epochs and depicts the experiences and thoughts of those who cultivated it in all ages. It may be studied from several points of view: the biographer searches the voluminous records to acquire knowledge of the intellectual activity of individuals; the historian unfolds the progress made by the science in a special field or in its entirety, with philosophical inquiries respecting effects and causes; the bibliographer, scarcely penetrating beyond the title pages of the dusty tomes, laboriously catalogues them to facilitate the researches of others.

We do not propose to give you a biographical, an historical or a bibliographical treatise, but rather to review chemical writings as sources of information and as portions of the world's literary productions. We shall concern ourselves less with the questions what were the personal history and life-work of a given author, and more with the queries what are the characteristics of the various classes of works at different epochs, what discoveries do they chronicle and what was their influence on the contemporaneous science.

The very earliest information concerning chemical arts comes to us from that ancient nation supposed by some to have given its own name to the science itself; not only do the sculptured tombs and temples of Egypt portray with unimpeachable authenticity and wonderful accuracy the technical skill of that venerable people, but these same monuments are even now relinquishing their hold on long-buried treasures in the form of papyri, whose perplexing script no longer conceals their meaning from the erudition of Egyptologists.

Of these miraculously preserved papyri the most valuable to chemistry is that discovered by Prof. George Ebers at Thebes in 1872, and named after its learned discoverer. We have described this elsewhere[1] and shall not here enter into details. It is the most ancient medical work extant, being assigned to the sixteenth century B. C., and contains a vast amount of information on the medical practice and the pharmaceutical preparations at that remote period. The unknown author wrote less obscurely than many of a much later date, and when the whole papyrus shall have been deciphered it will prove an invaluable contribution to chemical history.

The most ancient manuscript treating exclusively of chemical operations is a Greek papyrus of Egyptian origin preserved in the Library of the University of Leyden. Its authorship is unknown, its date is placed by Reuvens in the third or fourth century A. D. This MS. consists of a collection of prescriptions and receipts for conducting various operations in metallic chemistry, such as the testing of gold and silver; the purification of lead, of tin and of silver; the hardening of tin and of silver; the albification of copper, etc. It deals little with alchemy though some of the receipts evidently refer to transmutations, as those entitled: "the preparation (artificial?) of silver;" "the preparation of gold;" "the purification of tin by silver," etc.

Reference is made to sandarach (realgar), cadmia (zinc ore), chrysocolla, cinnabar, natron (soda), mercury and other chemical substances, but no receipts are given for their preparation. The author quotes from the Materia Medica of Dioscorides who probably preceded him by about two centuries. It is to be regretted that the full text of this ancient manuscript has never been published; the little known of it foreshadows information of great interest.[2]

The great libraries of Paris, Rome, Venice, Milan, Escurial, Cracow, Gotha, Munich and Cologne preserve a large number of Greek alchemical manuscripts of unknown authorship and uncertain date. Hoefer, the French historian of chemistry, refers them to the third and fourth centuries,[3] but other authorities with greater probability place them not earlier than the tenth and eleventh.[4]

The most celebrated of these essays are attributed to Zosimus, of whose history nothing is certainly known, and bear these titles: "On Furnaces and Chemical Instruments," "On the Virtue and Composition of Waters," "On the Holy Water," "On the Sacred Art of Making Gold and Silver." In a treatise attributed to Synesius, we find a description of a hydroscopium or hydrometer which was rediscovered as long after as the sixteenth century.

In a treatise attributed to Olympiodorus, he cites as authorities Democritus, Anaximander, Zosimus, Pelagius, and Marie a certain Jewess whom the later alchemists confounded with Miriam, Moses' sister.

In these manuscripts chemistry is called the "sacred art" and the exceedingly obscure and figurative language in which they are written makes it well nigh impossible to separate fact from fancy; Hoefer has indeed attempted to discover modern chemical conceptions in the allusions to Egyptian myths and the chaotic collections of spagyric arcana.

Of systematic nomenclature there is absolutely no trace; indeed each author seems to have aimed to write treatises intelligible only to himself, and we greatly doubt his success in even this respect. "Cadmia," we are informed, "is magnesia," and "magnesia is the female antimony of Macedonia;" "nitre is white sulphur which produces brass;" equally clear is the statement that the "apospermatism of the dragon is the mercury of cinabarn." That lexicons were early in demand is not surprising; in fact some of the most ancient MSS. are "vocabularies of the sacred art," but even with their assistance it is difficult to form satisfactory concepts of contemporary chemical science.

Suidas,[5] a Greek lexicographer of the eleventh century, states that Diocletian having conquered the rebellious Egyptians (296 A. D.) destroyed their books on the preparation of silver and gold, lest becoming rich by the practice of that art they might again resist the Romans. Regrets at the wanton acts of this imperial biblioclast are tempered by the reflection that modern scholars are spared the study of such literary absurdities.

The Chinese, that curious people who always claim a hearing when the origin or antiquity of arts and sciences is under consideration, were acquainted at a very remote period with many branches of chemical technology. We do not know of any special chemical literature produced by them, but the researches of Rev. Joseph Edkins[6] and of Dr. W. A. P. Martin[7] make it highly probable that scholars will yet discover contributions of no small importance to the early history of chemistry. Prof. George Gladstone[8] has endeavored to show that the Chinese originated the doctrines and pursuit of alchemy and communicated it to the Arabians by whom it was disseminated throughout Europe.

The high state of civilization and extraordinary intellectual development of the Arabians has left a deep impression on chemical science. Cultivated chiefly by physicians, attention was directed to its pharmaceutical applications, and in spite of the prohibitions of the Koran to the fascinations of alchemy. Of their extant writings, preserved in European libraries, only a portion have been edited; those best known partake of the poetical imagery and hyperbole characteristic of the Oriental mind. This is shown to some extent in the singnlar titles prefixed to their treatises, *e. g.*, "The Rise of the Moon under the Auspices of Golden Particles," by the alchemist Dschildegi; "A poem in the Praise of God, of Mahomet and of Alchemy," by Dul-nun-el-Misri.[9]

The well known treatises of Geber,[10] "Of the Investigation of Perfection," "Of the Sum of Perfection," "Of the Invention of Verity," and "Of Furnaces," notwithstanding a bewildering style of composition, which seems to confirm Dr. Johnson's derivation of gibberish, from Geber, display very great familiarity with a large number of chemical substances and operations.

Geber's works are generally assigned to the eighth century and consist chiefly of compilations from the "Books of the Ancients;" he mentions no author by name. They contain chapters devoted to the seven known metals, to the methods of distillation, calcination, cupellation and other operations, to the preparation of saline substances and to chemical philosophy. Geber adopted Aristotle's views of the constitution of matter from four principles, the hot and cold, the wet and the dry, and adds thereto: "Mercury and sulphur are the components of metals," a doctrine which with slight modifications prevailed for more than eight centuries. Geber describes the preparation of nitric acid, of aqua regia, and of mercuric oxide; he mentions the increase in weight of metals when calcined with sulphur, and gives the results of a rude quantitative analysis of crude sulphur. He constantly maintains the doctrine of transmutation of metals and gives a refutation of the ingenious arguments opposed thereto. His remarks on the qualifications of a chemist are most intelligent and are not inopportune in modern times; he urges the necessity of diligence, patience, learning, a temperate disposition, slowness to anger, and a full purse, " for this science agrees not well with a man poor and indigent, " together with faith in the God who " withholds or gives to whom he will" the secrets of nature, and who will infallibly punish the foolish meddler with magical mysteries.

To detail fully our obligations to Arabian chemists is no part of our plan. They have left an indelible impression on the very language of the science, in the words alcohol, alembic, alkali, borax, and many others. All honor to the intelligent authors who a thousand years ago defined chemistry as the " Science of Combustion, the Science of Weight, the Science of the Balance!" [11]

In the middle ages intellectual activity was confined largely to the clergy, who controlled the schools of learning, the libraries, and nearly all sources of knowledge. University chairs were occupied exclusively by clerical professors[12], literature and science were cast in ecclesiastical moulds. Scientific treatises were the production of monks and emanated from cloisters. Many distinguished philosophers mastered widely separated branches of learning: among these were Alain de Lille (b. 1114), celebrated as a physician, theologian, poet and historian, who filled the episcopal chair at Auxerre; Roger Bacon (b. 1214) an English cordelier; Raymond Lully (b. 1235), a Franciscan friar, and Albertus Magnus (b. 1193), Bishop of Ratisbon. The latter, amid the monotonous routine of a Dominican monastery, found leisure to distinguish himself in astronomy, medicine, alchemy and, according to his enemies, in necromancy. At this remote period, accusations of dealing with magic were not unfrequently made against those whose learning and skill in experimental sciences excited envy and superstitious zeal.[13]

To treat the writings of these eminent ecclesiastics as a part of chemical literature requires perhaps a stretch of the imagination, yet three hundred years ago they were regarded as masterpieces of the science and formed the text-books of students of alchemy. The writings of these ecclesiastical philosophers are as comprehensive as the branches of learning they cultivated, and incredibly voluminous; Albertus Magnus' collected works fill twenty-one folio volumes.[14] But a small fraction of these treatises are occupied with science and chemistry; and of this fraction there is in many cases a reasonable doubt as to their authenticity. In fact, nothing was more common than the ascription of work by an obscure second-rate writer to some celebrated philosopher of preceding ages, in order to give the work the stamp of authority,—a deception which previous to the invention of printing was more readily accomplished.

It became difficult therefore to distinguish the apocryphal writings from the genuine. The former, it is true, frequently betray themselves by anachronisms and other blunders, but many ingenious writers avoided such traps by adopting an enigmatical style worthy of the Delphian oracles.

Basil Valentine was the reputed author of works held in the very highest esteem by the alchemists of the Middle Ages, yet the very existence of this individual is seriously questioned. Mystery surrounds Valentine's entire history, and his writings were given to the world in a most dramatic manner; according to tradition they were hidden in the wall of a church at Erfurt and long after his death a thunderbolt shattered the wall and revealed the precious documents.

Whether Valentine was a real personage or not the works ascribed to him exhibit great familiarity with many chemical substances and operations, though the obscure and incoherent style renders their intelligent perusal very difficult.

Valentine's celebrated "Chariot of Antimony," [15] extolling the medical virtues of this metal, is perhaps the least obscure of his works; the "Twelve Keys of Philosophy" [16] with its singular plates, one of the most unintelligible; yet beneath the extravagant jargon characteristic of the period, glimpses are obtained of light and intelligence. The latter work presents clearly the theory that all metals are compounded of three principles: fixedness, metallicity and volatility, represented respectively by salt, mercury and sulphur, an hypothesis which long completely controlled chemistry until it gave place to the seductive theory of Phlogiston. It is uncertain whether the works ascribed to Valentine were first written in Latin or in German; his writings were collected in the seventeenth century and have been through many editions.[17] Several of his treatises have been translated into English and into French.[18]

In the fifteenth century the newly invented printing press was employed in the production of few works which can be regarded as chemical, and these were chiefly confined to isolated treatises of the ancient philosophers; in the sixteenth century the alchemists began to publish the results of their industry and speculations, and in the succeeding century a prodigious number of alchemical works were issued in Germany, France and England, creating literature of an extraordinary type.

Some of these treatises, which are numbered by thousands, record valuable experiments made by enthusiasts seeking the philosopher's stone, but the majority contain "a crude mass of incoherent propositions and wild assertions, a mixture of poesy and insanity, in which all logical ideas are lost amidst the stilted phraseology, but through which breathed a blind yet fervent faith." [19] Great obscurity of style [20]; an enigmatical method of naming chemical substances which found its highest development in the use of arbitrary symbols and the pictorial representations of alchemical processes [21]; the intimate association with astrology; the honest or affected intermingling of pious comments and prayers [22]; the extravagant claims to antiquity as respects authorship and processes [23]; the attempts to interpret the mythology of Egypt and Greece on an alchemical basis; the endeavor to associate the mysteries of Hermes with the sacred truths of the Christian religion[24],—all combine to produce literary monstrosities as fascinating to the student of chemical history as they are profitless to the practical worker in modern science.

Among the fabulous writings, highly esteemed by the credulous alchemists, may be mentioned the celebrated inscription of Hermes Trismegistus upon an Emerald Tablet[25], the Golden Leaves of Abraham, Jew Prince, Priest, Levite, Astrologer and Philosopher, which in the hands of Nicolas Flamel[26] yielded such a rich harvest, the Practical Chemistry of Miriam the sister of Moses[27], and a multitude of grotesque writings ascribed to personages of known reputation. Raymond Lully is credited with five hundred works; Hermes Trismegistus, the mythical Father of Sciences, with several thousand.

Many of the alchemical works published during the period of which we speak are degraded by admixture of the contemporaneous pseudo-sciences, judicial astrology and magic. To this class belong the celebrated works of Dr. Fludd and the writings of the Rosicrusians; excluding these as wholly beneath our consideration the number of occult works on alchemy is still very large. We imagine it will be hard to discover in the whole range of literature writings having scientific pretensions more senseless than the aphorisms of the disciples of Pythagoras, collected in the "Turba Philosophorum"[28] so often quoted by the alchemists of the sixteenth and seventeenth centuries. Its improbable character is perhaps equalled by the "Gloria Mundi" in which the anonymous author favors his readers with the chemical views of Aristotle, Plato, Socrates and Democritus, interspersed with equally authentic statements by Hermes and Morien, Lamech and Methuselah, Abel and Seth, and even of Adam himself.[29]

In the early part of the seventeenth century, Michæl Maier, physician to the Emperor of Germany, Rudolph II, a royal patron of astrologers and alchemists, published several treatises now much sought after by alchemical bibliophiles.[30] Maier's "Symbola aurea mensæ" and "Atalanta Fugiens" contain emblematic plates, supposed to illustrate the hermetic interpretation of the fables and allegories of Egypt and Greece.

The connection between these ancient mythologies and the secrets of the philosopher's stone was a favorite subject with many authors and has been exhaustively treated by the Abbé Pernety in his two curious works "Fables Egyptiennes et Grecques dévoilées" and "Dictionnaire Mytho-hermétique."[31] Not content with prose, several authors clothed their alchemical inspirations in poetry;

of these may be mentioned the Arabian treatises already named, the Twelve Gates of Alchemy by Sir George Ripley, written about 1450[32], the Crede Mihi of Thomas Norton, written about 1477[33] and the Chrysopœia of Aurelius Augurelli.[34] The latter written in Latin hexameters with more pretensions to elegance than usual with the prosaic alchemists was dedicated in 1514 to Pope Leo X. Leo rewarded Augurelli by presenting him with an empty wallet, remarking that one who knew so well how to make gold had need only of a purse.

The alchemists, in common with their contemporaries in other branches of literature, took pleasure in prefixing to their essays eccentric titles; Helvetius writes of "The Brief of the Golden Calf (The World's Idol) discovering the rarest Miracle in Nature;"[35] Glauber names one of his treatises, "The Golden Ass well managed and Midas restored to reason."[36]

Perhaps the height of absurdity is reached in the famous *Liber Mutus*[37], which consists of a series of fifteen symbolical engravings purporting to disclose the whole Hermetic Philosophy. The utterly unintelligible character of much alchemical literature is occasionally acknowledged by those who otherwise accepted the prevailing popular belief in transmutation; Lenglet du Fresnoy, speaking of Raymond Lully's Clavicula, says "Lully assures us that this treatise is indispensable to the comprehension of his writings; but, after reading it, one is little wiser than before."[38]

There are several large collections of alchemical treatises which the curious in these matters should consult; the most extensive is Zetzner's Theatrum Chemicum[39] published in 1613 in six octavo volumes; this contains no less than 209 distinct treatises, notwithstanding which Lenglet du Fresnoy remarks that many excellent ones are wanting. Manget's Bibliotheca Chemica Curiosa,[40] published in 1702 in two folio volumes, contains 133 treatises; and the Musæum Hermeticum[41] (1678) contains 21 treatises, some of which are illustrated. The most extensive English collection is Ashmole's Theatrum Chimicum Britannicum, published in 1652, and a noted French collection is Salmon's Bibliothèque des Philosophes Chimiques (1672), of which the edition by Richebourg (1741) is by far the best.[42]

Stimulated by avaricious hopes, the zealous alchemists labored most industriously; and, by subjecting mixtures of all known substances to heat and to the action of acids, discovered a multitude of new bodies having more or less medical and economic value.

As we have seen, their writings consist for the most part of monographs describing disconnected experiments, with no attempt at exhaustive treatment of any single topic, no classification of phenomena and no well studied arrangement of material such as characterizes later handbooks. Nor can a scientific collocation be expected during the formative period of chemistry and previous to the introduction of theories around which to group the isolated phenomena. One of the earliest attempts to treat chemical facts in a systematic manner was made by Sir George Ripley, Canon of Bridlington, who lived in the fifteenth century. In his "Compound of Alchemy" written in 1471 the whole science of hermetic chemistry is unfolded in a poem divided into twelve sections called "Gates" through which the reader is conducted to the mysteries of transmutation.[43]

"But into chapters thys Treatis I shall devyde,
In numbre twelve, with dew recapytulatyon;
Superfluous rehearsalls I lay asyde,
Intendyng only to give trew informatyon
Both of the theoryke and practicall operatyon:
That by my wrytyng who so wyll guyded be,
Of hys intente perfyctly speed shall he.

The fyrst chapter shall be of natural Calcination;
The second of Dyssolution, secret and phylosophycall
The third of our elementall Separation;
The fourth of Conjunction matrimonial;
The fyfth of Putrefaction then followe shall;
Of Congelation Albyficative shall be the sixt,
Then of Cybation, the seaventh shall follow next.

The secret of our Sublymation the eyght shall show;
The nynth shall be of Fermentatyon;
The tenth of our Exaltation I trow.
The elevent of our mervelose Multiplycatyon,
The twelfth of Projection; then Recapitulatyon,
And so this treatise shall take an end,
By the help of God, as I entend.

. . . .

Thus here the Tract of Alchemie doth end;
Which tract was by George Ripley, Chanon, penn'd
It was composed, writt and signed his owne,
In anno twice seaven hundred seaventy one.
Reader, assist him, make it thy desire,
That after life he may have gentle fire! Amen.

Ripley's versification and theme remind one of Chaucer's Canon's Yeoman's Tale composed nearly one hundred years earlier.

We can only briefly allude to Paracelsus' bombastic productions[44], which are concerned chiefly with medical chemistry, and to the remarkable works of George Agricola[45], distinguished by profuse illustration, portraying the apparatus and operations of mining and metallurgy in the sixteenth century.

Conspicuous for accuracy of description and systematic arrangement of topics is the "Alchymia" of Andrew Libavius, published at Frankfurt in 1595.[46] Libavius, a physician and teacher in the gymnasium at Coburg, rejected the absurd doctrines of the adherents of Paracelsus, combated superstition and quackery and, excelling in observation of chemical phenomena, gained a worthy position among his compeers. In his Alchymia he treats of the *Encheria* or manual operations of chemistry and of the *Chymia* or descriptions of substances, in separate books. The former he divides into two sections, one dealing with the instruments and the other with the management of fires and construction of furnaces. He describes at length a sumptuous laboratory provided not only with every requisite for chemical experimentation, but also with means of entertaining visiting guests, including such luxuries as baths, enclosed corridors for exercise in inclement weather, and a well stocked wine-cellar.

Libavius was the discoverer of stannic chloride which still bears the name "fuming liquor of Libavius"; he describes the method of preparing artificial gems by coloring glass with divers metallic oxides, and he seems to have been the first to apply the balance to the examination of mineral waters. Notwithstanding his progressive position he devotes eighty pages of his Commentary to the Philosopher's Stone in which he was a firm believer. The second edition of his works, published in 1606, forms a folio of 800 pages. It has sometimes been called the First Text-book of Chemistry.

The "Tyrocinium Chymicum" (1608) of Jean Beguin[47], Almoner to King Louis XIII of France, a less pretentious work, is characterized by freedom from ambiguity and prolixity as well as from hermetic superstitions; it deals chiefly with the medical applications of chemistry.

In 1660, Nicolas le Febvre, demonstrator of chemistry at the Jardin des Plantes, published a "Traicté de la Chymie"[48] greatly superior to all preceding works of its kind. He collected the most reliable theories and experiments from all published sources and arranged them in a logical, systematic manner. The first six chapters treat of the theory of chemistry; the author admits five elementary principles: phlegm (or water), spirit (or mercury), sulphur, salt and earth, the first three being ingredients of volatile substances, and the latter those of refractory bodies. He defines metals as hard bodies generated in the bowels of the earth, capable of extension under the hammer and of being melted by fire. He divides metals into the perfect and the imperfect, and also into male and female, the latter subdivision being based upon their behavior with acids; gold, lead and antimony are male metals because soluble only in aqua regia, and the other five, silver, copper, iron, tin and mercury are female, because soluble in unmixed acids.

In the second part the author discusses the manipulations of chemistry and recognizes many nice distinctions now overlooked; for example, under the head of mechanical division he describes in detail the following operations: "limation, rasion, pulverization, alcoholization, incision, granulation, lamination, putrefaction, fermentation, maceration, fumigation both dry and humid, cohobation, precipitation, amalgamation, distillation, rectification, sublimation, calcination both actual and potential, vitrification, projection, lapidification, extinction, fusion, liquation, cementation, stratification, reverberation, fulmination or detonation, extraction, expression, incineration, exhalation, digestion, evaporation, desiccation, circulation, congelation, crystallization, fixation, volatilization, spiritualization, corporification, mortification and revivification."

In the experimental part Le Febvre explains the arrangement in the following language:

"We shall begin with the meteoric bodies, rain, dew, honey, wax and manna; we shall then describe the preparations made from animals and their secretions; next, the numerous products of the vegetable world; and lastly, the mineral kingdom with its stones, salts, marcasites and metals;" a division still recognized.

The order in which he treats mineral chemistry is as follows: Earths; stones, precious and otherwise; metals; semi-metals, embracing mercury, antimony and bismuth; salts, including common salt, saltpetre, alum, sal-ammoniac and the vitriols; and lastly, the sulphuretted minerals, arsenic, etc. Under each division he gives precise instructions for numerous experiments upon these substances and describes their medicinal value. The whole work is written in a clear style, free from affectation of mystery; it rapidly passed through five editions, and was translated into English and German.

Three years after the appearance of Le Febvre's Traité, his successor at the Jardin des Plantes, Christopher Glaser[49] of Basle, published a work having the same title also marked by special excellence.

The most successful handbook of the seventeenth century was undoubtedly the "Cours de Chymie" by Nicolas Lemery,[50] an eminent lecturer in Paris. The first edition of this work was published in 1675, and it reached the tenth edition before the close of the century; a fourteenth edition enlarged by Baron appearing as late as 1756. It was also translated into English (1677), German (1698), Italian (1763), and Spanish. The remarkable success of this work is due to a facility for describing dry facts with remarkable simplicity and accuracy. His style is more concise than Le Febvre's, and his arrangement of material shows a progressive spirit. The limits of this address forbid an analysis of Lemery's Handbook, which, moreover, is better known than some others to which we have granted fuller treatment.

Passing over a period of fifty years, the next complete compendium of chemical knowledge which we notice is the "Elementa Chemiæ" of Hermann Boerhaave, the celebrated professor of botany, chemistry and medicine in the University of Leyden. Boerhaave's great erudition, purity of style and brilliant eloquence attracted students in great numbers; such was the popularity of his lectures that "certain booksellers who aimed at lucre by the most scandalous means" published them in 1724, without his authority or consent; this "surreptitious edition"[51] contained, as Boerhaave himself complains, such "false, ridiculous and absurd things in every page," that he was compelled to publish his lectures in an accurate and complete form. Both editions were translated into English by Dr. Peter Shaw, and the authorized work passed through many editions in Latin, French and German.[52] Instead of attempting to give an analysis of two quarto volumes, we will quote Boerhaave's own account of his plan.

"My design," he says, "is to initiate students in the knowledge of chemistry; and to do this in the most effectual manner, I shall give a clear methodical explication of all that is necessary for understanding the best authors and for performing the chief operations in this experimental art." After acknowledging the difficulty of a systematic treatment of "a science which has been cultivated rather by experiments at random than upon any regular principles, and by persons usually destitute of all . . . knowledge in the liberal arts;" he claims that "these obstacles may be surmounted by making a collection of the several effects, which the art has actually produced, justly deducing general rules therefrom and duly digesting the whole." He then explains the division of his work into three parts.

"The first will rehearse the origin, progress, cultivation and fortune of chemistry, . . ; the second part will deliver certain theorems or principles of chemistry; the third will exhibit the actual operations of chemistry, whereby bodies are changed agreeably to the rules of the art and to the end proposed therein." Boerhaave's sketch of the history of chemistry begins with the earliest times but seems to have been left incomplete; it is rendered useful by classified lists of chemical writings and numerous references.

In his treatment of the theory he departs somewhat from his plan and introduces material which really belongs to the third or practical part. Boerhaave introduces sections on the use of chemistry in natural philosophy, in medicine and in the mechanical arts; gives an exhaustive account of the wonderful nature of fire and experiments in heat, together with ingenious speculations concerning the heat of celestial bodies; he describes the various forms of chemical apparatus and the preparation of curious and useful substances from the vegetable, animal and mineral kingdoms. Boerhaave exhibited the spirit of a true philosopher and produced a work of extraordinary merit. He was quite free from the follies of alchemy, though he cautiously remarks that "we should always remember the limits of nature are by no means to be defined by us, things are taken for impossible which are only unknown by the ignorant" and "many things in chemistry are apparently more incredible than that lead should lose its natural form and be converted into gold." [53]

Contemporary with the systematic compilations of Libavius, Le Febvre, Boerhaave and others, were published hosts of independent works setting forth the results of prodigious labor in the chemical laboratory; many of these are filled with descriptions of experiments made at haphazard, with no definite object in view, and having no necessary connection with each other.

Glauber's[54] voluminous writings, published in 1658, may be taken as an example of this class; in his works, amid controversies with Galenical physicians and curious apologies resulting therefrom, amid descriptions of alchemical mysteries and receipts for a universal panacea, amid extravagant praises of the "sal mirabile" (Glauber's salt), and curious narratives of personal history, we find many novel chemical facts having a medical or an industrial value. The whole is clothed in a very crude style with an affectation of secrecy and under capricious captions.

On the other hand, some of these treatises show signs of genius, especially in attempts to establish theories in explanation of the familiar yet astonishing phenomena; thus Van Helmont, whose contributions to medical chemistry we pass by, invented the word *gas* to aid in discriminating aeriform bodies[55]; and Rey, in his "Essays on the increase in weight of tin and lead when calcined[56]," demonstrated the weight of the air thirty years before the masterly researches of Boyle.

To attempt in this rapid review to do justice to the philosophical writings of the Hon. Robert Boyle is a hopeless undertaking. From his first publication, in 1660, "New experiments, physico-mechanical, touching the spring of the air and its effects," to his posthumous treatise "Medicinal Experiments," published in 1692, his works teem with ingenious experiments described with great candor and fidelity, and from which conclusions are drawn with cautious reserve and philosophical soundness, as admirable as his rare modesty.

In his "Sceptical Chymist" (London, 1661), Boyle raises doubts as to the elementary nature of earth, air, fire and water, as well as of the alchemical principles, sulphur, salt and mercury, and claims that the first elements of bodies are atoms of different shapes and sizes, the union of which forms what are vulgarly called elements; he argues that their number should not be confined to three nor four, but that probably a large number of primary constituents would eventually be separated or isolated as such. He also first clearly recognizes the distinction between mixture and chemical combination.

The rejection of Boyle's prophetic hypothesis by his contemporaries much retarded the progress of the science. "More, however, than for his views on the nature of the elements, science is indebted to Boyle for his clear statement of the value of scientific investigations for its own sake, altogether independent of any application for the purposes either of the alchemist or of the physician . . Boyle was in fact the first true scientific chemist, and with him we may date the commencement of a new era for our science when the highest aim of chemical research was acknowledged to be the simple advancement of natural knowledge." (Roscoe.)

Boyle's voluminous writings were collected by Birch in five folio volumes and published in 1744. His diffuse style of composition, with frequent long digressions, renders a perusal of his writings wearisome and led to an edition by Dr. Shaw, in which his works are "abridged, methodized and disposed under general heads," forming three quarto volumes bearing the date 1725.[57] Many of Boyle's papers were published in the Philosophical Transactions.

The literature pertaining to the theory of phlogiston belongs almost wholly to the eighteenth century. It is true that Becher's Physica Subterranea was published in 1659[58], but the theory of a combustible principle existing in all metals remained dormant until Becher's admirer, George Ernest Stahl, published an edition of the Physica Subterranea in 1702; to this he prefixed a long preface elaborating those doctrines which exerted such immense influence on both the theoretical and practical science, for more than a century. The writings of Becher and Stahl are notorious for their barbarous mixture of poor Latin and German, for which indeed both authors apologize.[59]

In Mayow's Treatise on the nitro-aërial spirit[60], we find records of admirable experiments in pneumatic chemistry supported by accurate reasoning and almost prophetic insight of later theories of combustion, but his early death prevented the continued support of his views, and Mayow's treatise was soon forgotten by the prejudiced followers of Stahl.

In the year 1683 a noteworthy event occurred in the chemical world, the opening of public laboratories of instruction. Chairs of chemistry had long existed in institutions of learning, the first being filled nearly eighty years before by Johann Hartmann, and the honor belonging to the University of Marburg. Practical instruction had been secured also in the private laboratories of the wealthy, thus Homberg and Friedrich Hofmann worked in Boyle's establishment[61]; but the first public laboratory for instruction was opened at Altdorf, Bavaria, under the direction of Prof. J. M. Hofmann. A survey of the status of chemical knowledge at that date offers a tempting digression but the length of this address forbids.

The second public laboratory was opened in the same year (1683) at Stockholm, under the patronage of Karl XI of Sweden, and under the guidance of Urban Hiärne. Both institutions issued publications bearing the general title Acta[62] which may be regarded as forerunners of the "Contributions from the Laboratory of ——— University" now so common in periodical literature.

Lexicons and dictionaries have been a feature of chemical literature from the earliest times; we have already alluded to the vocabularies of the sacred art found among the Greek MSS. of the Paris National Library and referred to the tenth and eleventh centuries. These, however, seem to have been compiled for the purpose of misleading the novice in alchemical mysteries, and as Hoefer justly remarks themselves need commentaries to become intelligible.

The universal employment of symbolic characters, to represent both chemical substances and operations, rendered alchemical literature difficult of perusal and early led to the publication of keys and vocabularies explaining them. Such keys occur in the manuscripts just mentioned, in Eschenreuter's "Universal Panacea for Men and Metals"[63] which purports to have been written in or before the fourteenth century, and in the works of Oswald Crollius[64], Kircher[65], Le Febvre, Lemery and many authors of the seventeenth century: these however are not properly dictionaries.

Dr. Martin Ruland, court physician to Rudolph II, published a "Lexicon alchemiæ" in 1612, and William Johnson[66] a "Lexicon chymicum" in 1657, but the first really satisfactory treatment of chemistry in monographs alphabetically arranged was by Macquer[67] whose "Dictionnaire de Chymie," published in 1766, had great success, passing through several editions and being translated into the German, English, Italian, Spanish, Danish and Russian languages.[68] Macquer found many followers, among whom may be mentioned Nicholson (1795), Cadet (1803), and well known authors whose works are indispensable to all students of the science.

Prof. Wm. Ripley Nichols, of Boston, late chairman of this Section, in a recent private communication, has expressed the need of yet another dictionary, one of chemical synonymes. He calls attention to the large number of names by which certain chemical substances have been known at different epochs and in different professions, and to the utility of a dictionary explaining their synonymy. Professor Nichols suggests "restricting the list to terms found in the literature of the preceding hundred years, but including such alchemical names as have peculiar historic interest and such as have persisted more or less in commerce and pharmacy." He also suggests that the work should include English, French and German names, except when they are simply literal renderings of the same term, and names of those minerals which are of simple composition like halite, witherite, fluorite, etc., under their appropriate heading. In an example of the proposed method of treatment he gives a long list of names for ferric oxide under the several heads: alchemical, chemical, mineralogical, pharmaceutical, commercial and popular.

The idea is certainly excellent and we have Professor Nichols' permission to mention it publicly in the hope that some one may be incited to compile such a work. We are not aware of the existence of any modern chemical synonymicon, not having seen Berta's Dictionary of chemical terms[69], published at Padua in 1842; perhaps the nearest approach to the ideal work of Professor Nichols is Sommerhoff's Lexicon pharmaceutico-chymicum[70], published in 1701, a folio of 400 pages.

About the middle of the seventeenth century the dissemination of the views of Lord Bacon, as expressed in his *Novum Organon*, gave a great impulse to scientific investigations, and the "splendid fiction of the new Atlantis" was practically realized in the foundation of the "Royal Society for improving Natural Knowledge." The learned men, who in 1645 met in London, and there disturbed by unhappy dissensions of civil war withdrew to Oxford to report and discuss philosophical experiments, laid the foundations of an edifice destined to rise higher, endure longer and to shelter a nobler offspring than the most sanguine could have foreseen. The organization was granted a charter by King Charles II in 1662 and three years later began the publication of the "Philosophical Transactions, giving some accompt of the present undertakings, studies and labours of the ingenious in many considerable parts of the world." In the following year the French Academy of Sciences began their Mémoires. Thus arose the element of periodicity in scientific literature, one which rapidly increased in importance.

The year 1665 also witnessed the birth of the parent of that large class of periodicals not issued by societies but published by private parties either individually or coöperatively. The first number of the "Journal des Scavans" appeared January 5, 1665, and for one hundred and thirty years was the most prominent literary and scientific journal of Europe. At that early date the periodicals partook of a literary nature rather than of the scientific, but gradually these two elements became distinct and the lines became more and more closely drawn until every branch of pure and applied science supported a serial especially devoted to its progress. Among the earliest periodicals devoted to chemistry and its associate physics may be mentioned the "Journal de Physique et de chimie," edited by the Abbé Rozier, de la Méthérie and others, begun in 1770 and continued through ninety-five volumes to 1822; the "Chemisches Journal" of Lorenz Crell, 1778–86, which was followed by the "Chemische Annalen" by the same editor, extending until 1803. The "Annales de Chimie et de Physique," begun in 1789, enjoys the honor of being the oldest surviving serial of this class.[71]

Of the foundation of Societies exclusively devoted to chemistry in France, England, Germany and America, and of their periodical publications, we can make mere mention. The existence of a "Columbian Chemical Society" in Philadelphia, as early as 1811, deserves passing notice. Two volumes of its Memoirs were published containing articles by Drs. Mitchell, Cutbush, Manners, Bache and others.

Of chemical bibliography and works relating exclusively to the history of the science, we have elsewhere[72] given a critical résumé.

As we proceed with this review of chemical literature, the publications increase enormously in number, variety and importance, and the end of our undertaking seems to move forward faster than we progress. It is manifestly impossible in the brief time allotted to this address to analyze the chemical literature of the preceding hundred years. How shall we attempt to portray the theories and experiments recorded in the writings of Bergmann[73], Scheele[74] and Gahn, the eminent Swedes; of Black[75], Cavendish[76] and Priestley[77], the English triumvirate; of the French philosophers Fourcroy[78], de Morveau[79], and Lavoisier[80]; of the industrious German chemists Kunckel[81], Marggraf[82], Wenzel[83], Klaproth[84] and Richter[85]; or of the Italian philosophers Galvani[86], Volta[87], and Brugnatelli?[88] And passing to a later period, how shall we do justice to the labors of Berthollet[89], Gay Lussac[90], Thénard[91] and Laurent[92]; of the immortal Dalton[93], of the brilliant Davy[94] of the indefatigable analyst Berzelius[95]; of Mitscherlich, Liebig, Wöhler, and Hofmann; of Dumas, Berthelot and Adolph Wurtz; of Graham, Frankland, Abel and Crookes?

American contributions to chemical literature have been exhaustively discussed in the hearing of many present and need not now detain us; I refer to Prof. Silliman's Address at the celebration of the centenary of Priestley's discovery of Oxygen, held at Northumberland in 1874.[96]

The modern period of chemical literature is characterized by two opposing forces, a tendency to dispersal and an effort to collect the widely scattered publications. The multiplication of learned societies especially in Europe, each of which supports its own organ, and the increasing number of nations interested in scientific pursuits, in each of which arises an independent periodical, tend to the wide dispersal of memoirs, essays and notices.

On the other hand, industrious editors laboriously collect and set in order these scattered observations, constructing compact, massive handbooks and many volumed cyclopedias. The monumental work of Gmelin, which is even now passing through another edition, has worthy rivals in the magnificent compilations edited by Henry Watts and by Adolph Wurtz, and in the work of Beilstein in a field of special difficulty. For systematic treatises we can point to Miller, Graham-Otto, Pelouze et Frémy, Roscoe and Schorlemmer and the ever youthful Fownes.

Invaluable, too, are the annuals known as Jahresberichte, monuments of conscientious editorship.

Another feature of modern growth is the multiplication of special treatises dealing with single topics in every branch of chemistry, inorganic, organic, theoretical, analytical, technological, pharmaceutical and physiological, until mere acquaintance with their titles becomes a serious undertaking for busy workers in the laboratory.

The amount of time and labor required to search for a given point throughout the maze of modern chemical journals, transactions, treatises and hand books, is well appreciated by my audience; the superb dictionaries and annuals referred to accomplish much in cataloguing, condensing and systematizing the infinity of observations; nevertheless, all working chemists feel the need of special indexes, simply arranged, as complete as possible and accessible to every one. Sharing in this feeling, I have contributed my mite to the literature of chemical indexes and have inspired several friends, fellow-members of this Association, with the courage to follow. Uranium[97], Manganese[98], Titanium[99], Vanadium[100], Ozone[101], and Peroxide of Hydrogen[102] have been indexed on a uniform plan, and my pupil W. W. Webb, B. S., has prepared an Index to the Literature of Electrolysis[103], which will soon be published in the same channel as the others;— the Annals of the N. Y. Academy of Sciences.

Pardon, we beg you, these personal allusions: we have indulged in them with the object of bringing before this Section a proposition for organized effort in the preparation of Indexes to all the Elementary Substances on a uniform plan.

The advantages which would accrue from the publication of special indexes to the literature of the elements and of such other subjects as may prove desirable are too obvious to need argument; I have thought that the undertaking might be placed in the hands of a Committee of the Chemical Section of this Association. The existence of an Index Society in England under the guidance of Henry B. Wheatley, F. S. A., Hon. Sec.,[104] affords a precedent, if indeed any be required.

The first duty of the Index-Committee of the Section of Chemistry would be to secure volunteers willing to undertake the compilation of special indexes; surely in this National Association a few chemists can be found ready to coöperate in such an important work. The Committee would further adopt a uniform plan for the indexes, determine the method or channel of their publication, and exercise general editorial supervision over the undertaking.

Hoping that this suggestion will receive your approbation I leave it in your hands.

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104. Mr. Wheatley's address is 5 Minford Gardens, West Kensington Park, London W., England.

PAPERS READ.

ON A NEW MICROCHEMICAL METHOD OF DETERMINING THE FELDSPARS IN ROCKS. By J. SZABÓ, of Budapest, Hungary.

MY method of determining the feldspars in rocks is based chiefly on the coloration of the flame by potash and sodium, and on the degree of fusibility of the different kinds of feldspars.

In order to obtain the same results, the circumstances under which the experiments are made must be the same. The coloration of the flame and the fusibility are observed at the same time, in the same experiment.

I take a Bunsen burner of the dimensions described by Bunsen, the lamps being those furnished by Desaga in Heidelberg. I use two parts of the flame, one without the chimney at a height of five millimetres above the lamp for a lower temperature, and one for the highest temperature, which is produced by putting the chimney on the lamp and using a height of 5–10 millimetres above the chimney. It is the "Schmelzraum" of Bunsen, the region of fusion, and he estimates its temperature at 2300° C.

The platinum wire for introducing the feldspar grain must be very thin, a piece of one decimetre length should not weigh more than thirty-two milligrammes. I take pieces of it of three centimetres in length, connect them by fusion with a glass tube at one end, and at the other end, a ring is made of about one millimetre diameter, on which the grain of feldspar is fixed by means of distilled water.

The size of the feldspar must be approximately the same; it is the size of a fine mustard seed. If perfectly fused, it should be about this size (•). The feldspar grain is introduced into the different parts of the flame by fixing the glass tube to the holders used in the observations for spectrum analysis.

New determination of the fusibility of the minerals.

The result of determining the fusibility of the minerals being in many respects different from the results as obtained by Von Kobell, I find it necessary to give a brief description of my method of obtaining the degrees of fusibility.

For determining the fusibility with the same platinum wire and of the size mentioned, I use three parts of the Bunsen flame: *a*, the base of the flame; *b*, the height of five millimetres as mentioned, and *c*, the region of fusion, "Schmelzraum." Now I give the degrees of fusion according to the results obtained in those three experiments, and discriminate the following *eight* degrees for the fusibility of the minerals in general.

0. Infusible in all the three cases. Quartz, spinel.

1 (I = 0; II = 1) In five millimetres height during one minute no change is visible; but in the highest temperature after being exposed for one minute, the points and edges begin to be a little rounded, but on the sides no change is remarked. Bronzite, hematite, some anorthite, and some augite (Montė Rossi, Etna).

2 (I = 1; II = 2). The base of the flame serves in most cases only to fix the grain to the platinum wire. I speak of the first experiment, when the grain is introduced into the flame at five mm. height, and I call the second experiment obtained in the highest temperature. The mineral, to which I give the second degree of fusibility, shows after having been exposed for one minute to the flame in five mm. height a change in the points and edges, but the faces keep their original condition. In the second experiment even the faces began to fuse, but without being rounded. Some diallogite, bytownite.

3. (I = 1–2; II = 3). In the first experiment (five mm.; one minute) the points, edges and faces show the fusion but the general form of the grain is not changed. In the second experiment (region of fusion; one minute) the whole grain begins to be rounded, without becoming globular. Adularia (St. Gothard), labradorite, magnetite.

4. (I = 2–3; II = 4). In the first experiment (fifty-five mm.; one minute) it fuses without forming a globule; but in the second experiment it gives a perfect globule. Some hypersthenite, amphibole, most kinds of augite.

5. (I = 4). It fuses to a globule in the first experiment (five mm.; one minute); in this case the second experiment is not re-

quired. Petalite, garnet, some albite (of the trachytes of Hungary).

6. It fuses to a globule at the height of 5 mm. in less than one minute, or in the base of the flame but after the red heat. Borax, cryolite.

7. It fuses to a globule after some seconds near the base of the flame, before becoming red hot. Stibnite, sulphur, ozokerite, etc.

The results may be sometimes intermediate, for instance: 6–7 for rock salt, which begins to be fused at the commencement of a red heat; and the degree of fusion would be indicated as 5–6, 4–5, etc.

The exclusive use of Bunsen's burner in determining the fusibility is an advantage of my method as compared with the method of Plattner and Von Kobell, because the result is not affected by subjective ability of handling the blow-pipe. The temperature is nearly always the same, the gas flame used by me is never higher than fourteen centimetres. The time is also fixed, it is one minute.

The use of a lower and higher temperature and the observation of the change with a lens are in some cases very interesting. It is sometimes found that the substance is a mixture, for example, of feldspar and quartz; the feldspar is fusible, and the points and edges of the infusible quartz become visible in the mass in the first experiment, while at the high temperature the two substances may give one fused mass.

The chemical elements or the invariable binary compounds (quartz, magnetite, cassiterite, etc.) have a constant degree of fusibility, but this is not the case with compounds in which the isomorphic elements replace each other, or which are variable in the relative quantity of their elements, and that is the case with by far the greater number of minerals and especially with the feldspars. The presence of some elements make the mineral more or less fusible. Sodium is the element in the silicates which makes them easily fusible; the magnesium and aluminum render them less fusible, or altogether infusible. This important fact has induced me to establish the standard for the degree of fusibility according to the circumstances under which the fusion takes place, and not to take a mineral representing the degree, because such minerals are not to be found for all degrees among the elements, or the double constant compounds and the more complicated minerals, as amphibole, orthoclase, garnet, etc., may be variable, even when obtained from the same locality.

The fusibility of the feldspars, amphiboles, augites, as the chief rock-forming minerals, varies between 0–5.

The feldspars.

According to the dominant protoxide element there are potash, soda and lime-feldspars, which are passing into each other not only according to the silicic acid but also according to the protoxide element.

Tschermak discerns in the three kinds of feldspars on the "principle of equidistance" of the characteristic protoxide the following ten series.

A. Potash feldspar (orthoclase).

1. Adularia	series	K_2O	16–13	per cent
2. Amazonite	"	"	13–10	" "
3. Perthite	"	"	10–7	" "
4. Loxoclase	"	"	7–4	" "

B. Sodium feldspar (sodium plagioclase).

5. Albite	series	Na_2O	12–10	per cent
6. Oligoclase	"	"	10–8	" "

C. Lime feldspar (calcium plagioclase).

7. Andesine	series	CaO	6–10	per cent
8. Labradorite	"	"	10–13	" "
9. Bytownite	"	"	13–17	" "
10. Anorthite	"	"	17–20	" "

If the dominating protoxide elements show this change, the other elements vary in their relative quantity, of which the limits are given in the following table, taken from a considerable number of reliable analyses.

	Na_2O	K_2O	CaO	Al_2O_3	SiO_2
1. Adularia	1–2	16–13	0–15	17–19	67–64
2. Amazonite	2–4	13–10	0–15	17–19	67–64
3. Perthite	4–6	10–7	1–15	17–19	67–65
4. Loxoclase	6–8	7–4	1–15	19–20	67–65

	Na_2O	K_2O	CaO	Al_2O_3	SiO_2
5. Albite	12–10	0–1	0–1	18–20	69–66
6. Oligoclase	10–8	1–2	2–6	21–24	65–61
7. Andesine	8–5	2–1	6–10	24–29	60–54
8. Labradorite	5–3	2–1	10–13	25–30	56–50
9. Bytownite	3–1	1–0	13–17	29–34	52–47
10. Anorthite	2–0	1–0	17–20	32–37	46–43

If we take into consideration the results of the chemical, optical and crystallographical study of feldspars, we must distinguish: (*a*) in the class of potash feldspars, chemically, the adularia, amazonite, perthite and loxoclase; optically, orthoclase and microcline; (*b*) in the class of sodium feldspars, from a chemical point of view, albite and oligoclase; from an optical point of view, soda, plagioclase and the very rare soda orthoclase; (*c*) in the class of the lime feldspars the dimorphism has not yet been observed, therefore we discern only the four chemical species: andesine, labradorite, bytownite and anorthite, which on optical and crystallographical grounds are to be considered as four distinct species.

According to Tschermak's classification ten series have been established; there are intermediate series too, and in the same manner it is possible to distinguish less than ten series. From a geological point of view it is enough to establish the following five species: orthoclase, microcline, oligoclase, labradorite and anorthite.

The potash feldspar, as a rock-forming mineral, in most cases corresponds with the amazonite and perthite series, while adularia is restricted to the crystals found in veins and cavities of some orthoclastic rocks. It is therefore, for the geologist, sufficient to make use of the general name of orthoclase for the potash feldspar, except in the case when the microscope shows the presence of the dimorphic state, then the microcline must be named too. The microcline is very common in the old orthoclastic rocks, but in the newer volcanic rocks, containing the feldspars in a glassy state, I have never observed it.

In the series of the soda feldspars albite is restricted to the minerals found in the cavities of certain rocks, while the soda feldspar as a rock forming mineral is usually the oligoclase. In geological respect the oligoclase and the andesine have nearly the same rôle, therefore generally they might be taken for one species; the distinction may be reserved for a more detailed classification.

The labradorite is the main representative of the soda lime feldspars, but variable too. It stands sometimes nearer to the andesine; at other times nearer to the bytownite series.

The true lime feldspar is anorthite with bytownite, which are nearly always found together.

In the same rock it is very rare to find only one kind of feldspar; there are at least two, if not more, to be found together, but one of them is usually dominant, and then it is entitled to enter into the nomenclature of the rock.

The coloration of the flame.

The yellow coloration of the flame by the feldspars is a constant character; I have never found a feldspar without sodium, though analyses are met with in which the sodium has not been mentioned.

The coloration caused by sodium is very intense; it overcomes every other color, and this is also the case with the red flame produced by the presence of potash.

To observe the coloration produced by potash, I prefer to make use of the solution of indigo in concentrated sulphuric acid prepared as described by Fresenius, but diluted only so far, that the white-heated platinum wire begins to be visible through it. Cobalt glass is good only when it contains a high percentage of oxide of cobalt.

The calcium combined with silicic acid never shows any coloration.

I discover five degrees in the yellow flame of sodium:

1. The yellow flame is very small. It is obtained by introducing into the flame the anorthite of Monte Somma. It corresponds to one or less per cent of Na_2O in the feldspar.

2. The yellow flame is a little larger. It is shown by the potash feldspar called valencianite from Mexico. It indicates 1–2 per cent of Na_2O (in the analysis made by Plattner only the potash is mentioned).

3. The yellow coloration is stronger. Observable in the bytownite, found in the gabbro of Volpersdorf (Silesia), 2–4 per cent, Na_2O.

4. The yellow coloration is intense. Shown by the potash feldspar called microcline from Fredericksvarn. 4–8 per cent, Na_2O.

5. The most intense yellow flame, oligoclase, St. Gothard, 8-16 per cent, Na_2O.

The red coloration produced by potash is very weak when the feldspar is exposed to the high temperatures by itself, and becomes visible only in such feldspars which contain more than four per cent of it. Now all soda and lime feldspars contain less than four per cent K_2O; therefore, if a red coloration is visible in the first two experiments, the feldspar is a potash feldspar of a greater or less percentage, and accordingly I discern three degrees.

1. Scarcely visible, doubtful. Loxoclase, Hammond, 4–10 per cent, K_2O.

2. Distinctly visible. Sanidin, Perlenhardt, Rhine, 10–13 per cent, K_2O.

3. Very distinctly visible. Adularia, St. Gothard; leucite; 13–20 per cent, K_2O.

In order to be quite certain as to the potash, a third experiment is made, by adding gypsum to the feldspar, by which the alkalies are converted into sulphates, and thus rendered volatile in such a manner that even a very small part of one per cent becomes very distinctly observable. As regards the sodium, the five degrees remain the same as in the first two experiments, but for the potash four degrees can be discerned, of which the first two relate to the plagioclase, the last two to the orthoclase.

1. Very weak and visible only for some seconds. Anorthite of Monte Somma, 0–1 per cent, K_2O.

2. Weak but very distinct and lasts for two minutes. Andesine from the trachyte of Déva (Transylvania), 1–4 per cent, K_2O.

3. Strong red flame, clearly visible for two minutes. Sanidin of Perlenhardt (Rhine), 4–13 per cent, K_2O.

4. The red flame is very strong and permanent for more than two minutes. Adularia of St. Gothard, or leucite of Albano, 13–22 per cent, K_2O.

Fusibility and the quality of the fused matter.

It has been mentioned that the degree of fusibility, as regards the feldspars, varies from zero to five degrees; but besides this character, there is a remarkable difference in the quality of the fused matter. In all cases, when a feldspar is fusible, it assumes this state slowly without intumescence; if this phenomenon is observable, for instance as is the case in many zeolites, the substance is not a feldspar.

The fused matter may be a pellucid or transparent glass, or an opaque enamel. The glass may contain bubbles or not; the enamel is sometimes gray, sometimes white and may contain bubbles. The bubbles are situated for the most part inside, and the surface of the globule is smooth, but many of the potash feldspars exhibit the peculiar property of having also outside bubbles; this is the case especially with the orthoclases of granites and other old rocks, while the glassy potash feldspars never show this character. Whenever the microscope gives us notice of the presence of microcline, the outside bubbles are never wanting.

Practical course of the determination.

The Bunsen burner, and two holders on each side of it to the right and left, are placed on a thick piece of glass, in order to secure a smooth motion. Two feldspars may be determined at once, or one feldspar, and on the other holder may be put a typical feldspar for comparison. If the grain will not adhere to the moistened ring of the platinum wire, the ring may be too large or too small for the size of the mineral fragment. Sometimes the platinum takes up no water; in that case I place the grain on the ring by means of a finely pointed pencil, or any other non-elastic matter.

The mineral fragment must be looked at with a Coddington lens in order to observe the slightest change in the consecutive experiments.

There are three experiments to be made, the first two with the mineral alone, but introduced at different temperatures for one minute; the third is made in the high temperature, but with addition of gypsum, and exposing to this high temperature for two minutes. The results observed are inscribed in blank forms, which must have columns for every observable phenomenon, so that after the three experiments we get a group of characters, which enables us to give the name of the feldspar, supposing that it was in a normal condition, or at least not far from it.

The next table shows the order of succession of the three experiments and their numerical results, not only for the ten series of feldspars, but also for some feldspathic minerals found in volcanic rocks, especially of newer date. At the end, two common minerals, the amphibole and augite of one special locality in Hungary, are exhibited in their somewhat unusual behavior; both those minerals being usually fusible 4–5.

TABLE

SHOWING THE ORDER OF SUCCESSION OF THE FLAME-EXPERIMENTS AND THEIR NUMERICAL RESULTS FOR THE FELDSPARS, AND SOME ASSOCIATED MINERALS.

FELDSPARS.	I EXPERIMENT. 5 MM. 1 MINUTE.				II EXPER. REGION OF FUSION. 1 MIN.				III Exp. with gypsum.		Flame reaction of the HCl solution.
	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-3	Degree of fusion, 1-5	Quality of the fusion.	Na 1-5	K 1-4	
1 Adularia	1-2	3	1-2	Glassy, sometimes bubbles outside.	2-3	3	3	Often bubbles outside.	2-3	4	Ca 0, Na 1-2, K 0.
2 Amazonite	2-3	3-2	2-3	"	2-3	3-2	3-4	"	3	3-4	Ca 0, Na 1-2, K 0.
3 Perthite	3-4	2	3	"	3-4	2	4	"	3-4	3	Ca 0, Na 1-2, K 0.
4 Loxoclase	4	1	3-4	"	4	1	4-5	"	4	2-3	Ca 0, Na 1-2, K 0.
5 Albite	5	0	3-4	Glassy, contain bubbles.	5	0	4-5	Bubbles inside.	5	0-1	Ca 0, Na 1-2, K 0.
6 Oligoclase	5-4	0	3-4	Glassy, or enamel-like.	5-4	0	4-5	"	5-4	1-2	Ca 0, Na 2, K 0.
7 Andesine	4-3	0	2-4	Glassy, transparent.	4-3	0	3-5	"	4-3	1-2	Ca 0-1, Na 2-3, K 0-2.
8 Labradorite	3-2	0	1-2	" "	3-2	0	2-3	Enamel-like.	3	1-2	Ca 1-2, Na 2-3, K 0-1.
9 Bytownite	3-2	0	1	Glassy, whitish.	3-2	0	1-2	Glassy, with some bub's.	3-2	0-1	Ca 2-3, Na 1-3, K 0-1.
10 Anorthite	2-1	0	0	Nearly no change.	2-1	0	0-1	Glassy, without bubbles.	3-2	0-1	Ca 2-4, Na 1-3, K 0-1.
Leucite	2-3	3	1	Glassy, translucent.	2-3	3	2	Glassy, no bubbles.	2-3	4	Ca 0, Na 2-3, K 4.
Nephelite	5	1-3	2-3	Glassy, enamel-like.	5	2-3	3-4	Glassy, with rare bubbles outside.	5	3-4	Ca 0, Na 5, K 3-4.
Nosite	5	1	1	A little glassy, grayish.	5	1-2	2	Partly translucent.	5	2-3	Ca 0, Na 5, K 3.
Haüynite	5	1-2	1	Nearly no change.	5	1-3	2-3	Partly glassy.	5	2-3	Ca 0, Na 5, K 1-3.
Sodalite	5	0-3	1-3	Glassy, whitish.	5	0-3	3-4	Glassy, bubbles.	4-5	0-3	Ca 0-1, Na 5, K 1-3.
Amphibole Trachyte, Drégely var	2	0	1-2	Black, no lustre.	2	0	4	Black, shining.	3	1	Ca 0, Na 1-2, K 0.
Augite Trachyte, Drégely var	1	0	0	No change.	1-2	0	1-2	Black.	2	0-1	Ca 0, Na 1, K 0.

In the *first* experiment, without the chimney, the mineral is introduced at the lower temperature (5 mm.) for one minute. The coloration caused by the sodium is estimated, and then immediately the observation with the blue indigo-fluid is taken in order that it may be completed at the end of one minute. After this time the holders are removed and the grains observed with a lens in order to give a number to the degree of fusibility and to its quality. All results are noted in their respective columns on the form.

Second experiment. The chimney is put on, the platinum wire is placed at such a height, that it may reach the region where the highest temperature is obtainable. All the observations are the same as in the first experiment, except that for the degree of fusibility as well as for its quality which is now conclusive.

Third Experiment. The same grain is exposed to the same temperature again, but after having been moistened with distilled water, and brought in contact with powdered gypsum, in order to cover it with it on all sides, and in that state it is introduced into the flame for two minutes. Now only the degrees for sodium and potash are noted.

General remarks for the three kinds of feldspars. For the *potash feldspars* the presence of this protoxide element in a higher percentage is the most essential character, and this is determined by the first two experiments, the potash feldspars showing alone the red flame and none of the plagioclastic, soda, or lime feldspars. The first experiment will therefore determine whether the feldspar is an orthoclase or a plagioclase.

The potash feldspars are not acted upon by concentrated hydrochloric acid ; there is only a very small quantity of sodium extracted, so that the solution introduced into the flame produces a feeble yellow tint.

For the *soda feldspars* the great quantity of sodium is characteristic, and visible at the very moment when the mineral is introduced into the flame. At the same time the red flame must be absent. In the third experiment even the small quantity of potash contained in the soda feldspars becomes visible. The oligoclases contain always more or less potash, but among the albites there also occur specimens without potash.

The oligoclase is very commonly found together with the orthoclase in the same rock, and I have observed that whenever

poor in potash (loxoclase) the oligoclase contains
it; but with the amazonite or perthite a variety
always associated, in which the percentage of
ondingly higher.

spars are not acted upon by concentrated hydro-
solution gives only a slight yellow coloration to

ars are characterized by the gradual intensity of the
absence of the red flame, and by the lower degree
y. The degree of fusibility was not an essential
potash and soda feldspars, but it becomes very
case of the lime feldspars, the four series being,
r increasing basicity, less and less possible.

e of fusibility for the andesine is 5–3
" " " " labradorite " 3–2
" " " " bytownite " 2–1
" " " " anorthite " 1–0

the fused material shows also some slight pecu-
orthite is not fusible, or fusible only at the points,
glassy, and contains no bubbles; the bytownite is
or but with a somewhat more advanced degree of
ed labradorite is in most cases a whitish or grayish
ke to some chalcedony; the andesine shows this
the first experiment, but in the second it gives
a limpid globule with bubbles inside, like the

nembered, that while for the highest and lowest
ity the size of the grain has no essential influence,
gment of quartz only the tenth part of the size
method, it will show the infusibility in the same
the contrary, if we take of borax ten times as
t nearly as easily as the standard size; but it is
ith the middle and neighboring degrees. If the
rite is somewhat smaller, it shows the fusibility
that case the experiment must be repeated with a
fragment: if it fuses, it is andesine, if not wholly
adorite.

The concentrated hydrochloric acid exerts very little action when left in contact with the andesine, but the labradorite is partly decomposed, the bytownite still more, and much more the anorthite, the protoxide elements being accordingly converted into chlorides, which are very effective in coloring the flame.

The flame experiments with the hydrochloric solution.

The application of this well known principle is carried on in the following manner: I put in a short glass tube (height about 2 cm., diameter 5–6 mm.) some fine fragments of the mineral, and add only as much of the concentrated hydrochloric acid, as is required to cover it completely. The glass tube covered with wax is left for twenty-four hours; on the next day, at the same hour, I take out a drop by plunging in a thick platinum wire (like horse hair), which ends in double spiral rings. While the thin platinum wire, which we have employed for the flame reactions with the solid matter, would produce an instantaneous phenomenon, of which, I dare say, no use could be made, the platinum wire of the thickness mentioned will separate the chlorides according to their volatility. I introduce the drop into the base of the flame on the front side, and the following phenomena are to be observed. The first is the yellowish red coloration caused by the calcium chloride, but mixed with the yellow of sodium. To be certain that lime is present even in small quantities, I use a direct-vision spectroscope which shows the red and orange line very distinctly even for a trace of calcium. At the next moment the great bulk of the yellow coloration makes its appearance, and when arrived at its culmination, the red flame of the potash chloride may be observed through the indigo solution. The last of the usual colorations is that caused by lithium, visible through the spectroscope.

The degrees are marked about in the same manner as for the experiments in the dry way (see the table).

The platinum wire must of course be tried first alone for the coloration of the flame, and then moistened with the same acid, which is used for the decomposition of the mineral powder.

On the accompanying table we find the leucite as another potash mineral, having similar characters as the adularia in the dry experiments; but if we consider the flame reaction of the hydrochloric solution, then the great difference between the two will be evident. The same is the case with nepheline, nosite, sodalite or soda minerals; we find in the columns of the hydrochloric solution the greatest quantity of sodium, together with a high degree of potash, while the soda feldspars color with their hydrochloric solution the flame for soda very feebly, and for potash not at all.

For finding the calcium in such compounds, which are not acted upon by hydrochloric acid, for instance to make a distinction between oligoclase and andesine, I make use of the microchemical method of Boricky, by attacking the feldspar by hydrosilicofluoric acid, which shows the peculiar forms of calcium-silico fluoride in the andesine in larger quantity, and very little or none in the evaporated drop of the oligoclase solution.

Concluding remarks.

The method of determining the feldspars can be employed also for other classes of minerals with equally good results, but of course the alkalies giving the coloration, for minerals not containing alkalies, the result will be only the determination of fusibility. For the zeolites it gives some groups not only according to their volatile constituents coloring the flame, but also for the quality of the fused material, some being fusible with intumescence, some without it. It is very important also for the great class of the bisilicates (anphibole, augite, diallage, hypersthenite, enstatite, etc.), and it shows that some of them contain in more cases potash or sodium, or both of them, than has hitherto been supposed. There is a peculiar class of silicates however, the micaceous minerals, for which the result in respect of the quantitative indications is not the same as for the feldspars. The coloration caused by the potash never corresponds with its percentage, it is always less. For this class of minerals I make use of the method of Boricky.

The results obtained for the feldspars will give preliminary indications for their chemical analysis, knowing approximately the limits for the sodium and potassium; the limits for the other elements are given too. But its great importance in the lithological study of the crystalline rocks may be inferred by the fact, that it is the unanimous conviction with petrographers, that every kind of a rock mass can be best determined by the mineral association found in it, and of all minerals it is the kind of feldspar which is the most important. There was a time, when we have been contented to say feldspar generally; then came a time when we said orthoclase and plagioclase, but now we know that this is not enough; we must discern among the plagioclases according to their basicity at least an oligoclase-andesine, a labradorite and a bytownite-anorthite. When we take a certain cycle of a well defined eruption, for instance, the eruption of the trachytic family, we find a correlative between the feldspar and the relative age: the trachyte with orthoclase-oligoclase, having the most acid constitution, was the first product of eruption, and the trachyte containing bytownite-anorthite and correspondingly having the most basic constitution, was the product of the last eruption, while the labradorite-trachytes are to be found between these two extremes. And that is the case for the cycles of other eruptions too, as far as some instances indicate it.

In the normal state of a rock it is the whole mineral association which must be taken into consideration, but in many cases the dark colored minerals are entirely changed, and only the feldspar to be found chemically in its normal state; then the determination of the feldspar is the only possible way of saying something of the lithological and chronological character of the rock in question, and the flame experiments furnish us with the result required in a short time and with sufficient exactness for the purpose aimed at.[1]

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[1] Eine neue Methode die Feldspathe auch in Gesteinen zu bestimmen, von Dr. J. Szabó. Budapest, 1876. Berlin, Friedländer.

Elemente einer neuen chemisch mikroskopischen Mineral und Gesteinsanalyse, von Dr. E. Boricky. Prag, 1877.

APPLICATION OF ORGANIC ACIDS TO THE EXAMINATION OF MINERALS. *Third Paper.* By H. CARRINGTON BOLTON, of Hartford, Conn.

37. IN the two papers previously published under the same title[1] we have shown that citric acid has a power of decomposing minerals little less than that possessed by hydrochloric acid, and that the organic acid can be advantageously employed in field-work for assistance in determining species. In the second of these papers we stated that the results obtained by the *prolonged* action of citric acid on minerals differed greatly from those recorded in the accompanying tables, and gave notice of our intention to return to the subject at a subsequent period. We have continued our researches and gained a few additional facts which we deem of sufficient interest to warrant a third or supplementary paper on the subject.

The results given in the tables (here reproduced for convenience) were obtained by acting on the minerals in fine powder: first, in the cold for a few hours, and secondly, at a boiling heat for a few minutes. We find, however, that many species which resist the brief action of a boiling concentrated solution of citric acid are more or less completely decomposed by prolonged contact with the same solution at the ordinary temperature of the work-room, say 60° to 70° Fahr. But a small number of the two hundred minerals embraced in the tables were submitted to the prolonged action of the acid solution, and those tested were selected as representative of groups, sulphides, oxides, silicates, etc. The progress of decomposition was noted at various periods from a few days to more than two years. A solution of citric acid concentrated in the cold was used in all the experiments.

38. *Sulphides.* Chalcocite showed signs of decomposition at the end of ten days, and at the expiration of several months yielded a partial solution of a pure green color.

Ullmannite showed signs of decomposing in two days, and afforded in a month a very dark colored solution.

Arsenopyrite was attacked in a few days. Pyrite showed decided evidence of decomposition in eight days; one month later the solution acquired a reddish yellow color and reacted for iron and sulphuric acid.

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[1] Annals N. Y. Acad. Sciences. Vol. I, p. 1, 1877, and Vol. II, p. 1, 1880.

Chalcopyrite acts similarly, the solution being of a greenish yellow hue and reacting for copper and sulphuric acid. One gramme of the mineral lost about eleven per cent of its weight after fourteen months' contact with the acid solution. In general quantitative experiments were not conducted, for it soon became clear that the amount of decomposition was proportionate to the time and would require more attention than the results to be gained warranted.

Smaltite was decidedly attacked after eight days—the presence of sulphuric acid in the colored solution was noted. Tetrahedrite was strongly attacked in about four months. Cinnabar, orpiment, argentite and pyrargyrite resisted the acid solution altogether.

39. *Oxides.* The experiments recorded in this brief supplement were made with only those minerals which resisted the acid solution on boiling, and since it was obviously useless to test such bodies as quartz, corundum, rutile, etc., only the three oxides of iron remained of this class for examination.

Magnetite and limonite were strongly attacked in eight days; hematite yielded much more slowly, showing decided decomposition after several months.

Exposure to sunlight, as might be expected, diminished the intensity of the colored iron solutions.

40. *Silicates.* Of the silicates examined datolite is by far the most quickly decomposed by the acid solution; after twenty-four hours it yields gelatinous silica. Datolite should probably be transferred from column H of the tables to column D.

Hornblende, pyroxene, almandite, epidote, vesuvianite and serpentine were decidedly decomposed in eight days; after one month hornblende, almandite and epidote yielded colored solutions and slimy silica, and serpentine gave gelatinous silica. After fourteen months contact with the acid solution (occasionally agitated) the hornblende and pyroxene were very completely decomposed, the solutions having a dark brown color and full of floating silica. Actinolite seems to be less easily decomposed than common black hornblende. The serpentine after fourteen months yielded a dry gelatinous mass.

The feldspars are very unequally attacked under similar conditions. Labradorite yields the easiest, decomposition being noticed after eight days and slimy silica forming in the course of some months. Orthoclase and oligoclase showed marked signs of decomposition after eight months, but albite is doubtful. Tourmaline and staurolite yielded to the acid after four or five months, the latter forming a solution colored by iron and slimy silica. Talc and kyanite appear to resist the action of citric acid.

Muscovite and biotite yield very slowly, the latter showing signs of decomposition the sooner. After two years' subjection to the acid solution very minute scales of mica separate, and some slimy silica appears in the slightly colored liquid.

41. We have detailed these simple observations somewhat minutely because we believe they have an important bearing on geological disintegration and erosion. At the Saratoga meeting of the A. A. A. S., my friend Dr. Alexis A. Julien presented an exhaustive summary and analysis of all known observations, concerning the geological action of the humus acids[2]; in this paper he notes the negative character of the results I had obtained with a number of minerals, *e. g.*, pyrite, augite, hornblende, the feldspars, micas, etc., and says: "but the nature and strength of the solvent employed, and the brief duration of each trial seem at present insufficient to justify generalizations, notwithstanding the suggestiveness of certain results." My subsequent observations have confirmed his views. Generalization is still of doubtful value, yet we are tempted to make the venture. In the following table we have arranged the chief rock-forming minerals, with a few of their associates, under four heads indicating their relative disintegration by citric acid; the position of each mineral is of course only approximate, and it is not intended to indicate that all the species in one column are equal in degree of their decomposition. The application of this table to the consideration of

TABLE

SHOWING APPROXIMATE RELATIVE DISINTEGRATION OF ROCK-FORMING (AND ASSOCIATED) MINERALS BY CITRIC ACID IN SOLUTION.

Quickly decomposed.	*Slowly decomposed.*	*Very slowly decomposed.*	*Not decomposed.*
Carbonates, Phosphates, Prochlorite, Chrysolite, Nephelite.	Serpentine, Pyroxene, Hornblende, Labradorite, Garnet, Epidote, Vesuvianite, Pyrite, Limonite, Magnetite, Gypsum (?).	Orthoclase, Oligoclase, Albite (?), Biotite, Muscovite, Tourmaline, Staurolite, Hematite.	Quartz, Corundum, Spinel, Beryl, Fluorite, Barite. Talc (?), Kyanite (?).

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[2] Proceedings Am. Assoc. Adv. Sci., Saratoga Meeting, Vol. 28, p. 311, 1879.

geological phenomena is a subject which I do not venture to discuss; if we have a right to assume that the disintegrating effect of the acids of humus differs from that of the other organic acids in degree rather than in kind, the Table may prove a small contribution to chemical geology.

TABLES

SHOWING THE BEHAVIOR OF CERTAIN MINERALS WITH CITRIC ACID, ALONE AND WITH REAGENTS.

I.

DECOMPOSED (IN FINE POWDER) BY A SATURATED SOLUTION OF CITRIC ACID.

1. IN THE COLD.

A. *Without evolution of gas.*	B. *With liberation of CO_2.*	C. *With liberation of H_2S.*	D. *With separation of $Si\,O_2$.*
Clausthalite, Leucopyrite, Atacamite, Brucite, Gummite, Pyromorphite,* Mimetite, Triphylite, Triplite. Vivianite,! Libethenite,! Olivenite,! Pseudomalachite, Wavellite, Pharmacosiderite,! Torbernite, Autunite, Ulexite,! Cryptomorphite,! Anglesite, Brochantite.!	Calcite,! Dolomite,* Gurhofite,! Ankerite,* Rhodochrosite,* Smithsonite,* Aragonite,! Witherite,! Strontianite,! Barytocalcite,! Cerussite,! Malachite,! Azurite.*	Stibnite, Galenite, Alabandite,! Sphalerite, Pyrrhotite.	Wollastonite, Rhodonite,! Chrysolite, Willemite,!‡ Nephelite, Lapis lazuli, Chondrodite, Pectolite,!‡ Laumontite,! ‡ Chrysocolla,! Calamine.!‡ Apophyllite, Thomsonite,! Natrolite,!‡ Mesolite,! Analcite, Chabazite, Herschelite,‡ Stilbite. Deweylite, Prochlorite.

2. ON BOILING.

E. *Without evolution of gas.*	F. *With liberation of CO_2.*	G. *With liberation of H_2S.*	H. *With separation of SiO_2.*
Cuprite,! Zincite, Melaconite, Goethite,* Limonite,* Allanite, (?) Apatite,* Wolframite,* Wulfenite, Crocoite, Gypsum.*	Hausmannite,† Pyrolusite,!† Manganite,† Psilomelane,!† Wad,!† Magnesite,! Siderite.!	Bornite, Jamesonite,* Bournonite.* Boulangerite, Kermesite.	Tephroite,‡ Ilvaite, Phlogopite,* Datolite,†‡ Prehnite,* Heulandite, Serpentine, Chrysotile, Retinalite, Bastite, Genthite, Gieseckite,* Jefferisite, Masonite.*
and those in A.	*and those in B.*	*and those in C.*	*and those in D.*

II.

DECOMPOSED BY A BOILING SOLUTION OF CITRIC ACID MIXED

I. WITH SODIUM NITRATE.

Silver.
Mercury,
Copper,
Arsenic,
Antimony,
Bismuth.
Sulphur.*
Bismuthinite,
Domeykite, !
Argentite,
Hessite,
Chalcocite, !
Tiemannite, !
Millerite, !
Niccolite, !
Pyrite, !
Chalcopyrite, !
Linnaeite,
Smaltite, !

Cobaltite, !
Ullmannite, !
Marcasite. !
Arsenopyrite, !
Nagyagite,
Covellite, !
Berthierite, !
Pyrargyrite,
Tetrahedrite, !
Tennantite, !
Stephanite,
Polybasite, !
Enargite, !
Uraninite, !
Hübnerite.

and those in C and G.

K. WITH POTASSIUM IODIDE.

Realgar,*
Orpiment,*
Cinnabar, !
Hematite,*
Menaccanite,*
Washingtonite,*
Magnetite,*
Franklinite,
Braunite,
Enstatite,
Hypersthene,
Augite,
Spodumene,*
Hornblende,*
Actinolite.*
Pargasite,*

Olivine.
Almandite,
Pyrope.
Colophonite,
Epidote.

and most of those in A, B, C, D, E. F, G, H, and I.

III.

L. MINERALS NOT DECOMPOSED BY THE ABOVE REAGENTS.

Graphite,
Molybdenite,
Proustite,
Fluorite,
Cryolite,
Corundum,
Diopside,
Petalite,
Asbestus,
Beryl,
Zircon,
Vesuvianite,
Zoisite,

Iolite,
Biotite,
Muscovite,
Lepidolite,
Wernerite,
Leucite,
Andalusite,
Fibrolite,
Kyanite,
Topaz,
Titanite,
Staurolite,
Bowenite,

Talc,
Kaolin,
Ripidolite,
Columbite,
Samarskite,
Spinel.
Chromite,
Chrysoberyl,
Cassiterite,
Rutile,
Quartz,
Hyalite,
Anorthite,

Labradorite,
Oligoclase,
Albite,
Orthoclase,
Tourmaline,
Scheelite,
Barite,
Celestite,
Anhydrite.

(Two hundred species.)

N. B.—The gases evolved are examined with acetate of lead test paper, the solutions with appropriate reagents.

The kind and degree of action are indicated in the above Tables by the following signs:—

! Completely decomposed or dissolved.
* Feebly attacked.
† The CO_2 evolved is derived from the Citric Acid.
‡ Gelatinizes.

CARBON DIOXIDE AS A CONSTITUENT OF THE ATMOSPHERE. By ERNEST H. COOK, of Newton Villa, Southville, Bristol, England.

[ABSTRACT].

ALTHOUGH occurring in such a relatively small amount in the atmosphere, this substance has taken a leading part in effecting geological changes and in building up strata of immense thickness. We are apt to underrate its influence. When it is remembered that were it not for its presence in the atmosphere, no coal, very little limestone, probably no vegetable, and hence little animal life could exist, we at once see its importance.

In the present communication the author calculates the absolute amount of dioxide existing in the atmosphere, notices the sources whence the supply is derived, the amount yielded by each, and the causes at work abstracting dioxide from the air. On many of these points our knowledge is so unsatisfactory and fragmentary that the author hopes the attention of scientific men may be called to a subject of such importance to the geologist, meteorologist and chemist.

Amount of Carbon Dioxide in the Atmosphere.

Two things are necessary in order to determine this, viz.: the capacity of the atmosphere and the percentage of CO_2 contained by it. Taking the lengths of the polar and equatorial diameters of the earth and of the height of the homogeneous atmosphere, it is easy to calculate the capacity of the atmosphere. This is found to be equal to 591,647,337 cubic miles, or 2,439,987,200,000 millions of kilolitres. With regard to the average amount of dioxide contained in air, experimenters differ. The older chemists Dumas and Boussingault, Loëwy and Saussure, have published results which yield a mean of 4 volumes in 10,000 of air, or 4½ parts in 10,000 by weight. On the other hand, more recent experiments conducted by Thorpe, Fittbogen and Hasselbarth, Farsky, Reiset and others, indicate that 3 volumes in 10,000 of air is the correct number.

Assuming the first of these numbers to be the true one, we find that the amount of CO_2 in the atmosphere is very nearly 1,913,686,000 millions of kilogrammes, while if we take the more recent determinations, we find 1,435,265,000 millions of kilogrammes. These numbers differ considerably from those previously given. Thus Dumas and Boussingault say[1] that the air is equal in weight to 581,000 cubes of copper, each having a side of 1 kilometre — a number which is 40 per cent higher than the greater of the two numbers stated above. Again Roscoe and Schorlemmer[2] state that "the amount of CO_2 in the atmosphere reaches to upwards of 3,000 billions of kilogrammes" which is again 33 per cent above the same number.

Sources whence the air derives its Carbon Dioxide.

These may be grouped as follows:

(1). Combination of carbonaceous bodies.

(2). Respiration of animals.

(3). Decomposition of animal and vegetable substances.

(4). Volcanoes and other subterranean supplies. Under the first heading is included the amount produced by the burning of coal, wood, peat, etc. Taking the statistics which have recently been issued with regard to the amount of coal raised in the world, it will be found that for the last three years at least 280,000,000 of tons have been raised annually. Assuming 75 per cent of this consists of pure carbon, and allowing a further 10 per cent for the carbonaceous material thrown away with the ash, we leave 182,000,000 of tons which are annually converted into carbon dioxide. Thus there is daily added to the atmosphere from this source 1,800,000,000 of kilogrammes of dioxide. By the combustion of wood, peat, oil, etc., we may assume one-third more to be added, thus making a total of 2,400,000,000 of kilogrammes per day derived from the combustion of carbonaceous substances.

In the case of the respiration of animals we can only form an approximate estimate. The population of the world is at present about 1,500,000,000, and experiment shows that an average individual produces about one kilogramme of CO_2 in a day of twenty-four hours. Thus the human race in respiring adds daily to the air 1,500,000,000 kilogrammes. From the lower animals we may consider twice this amount to be derived; thus from the whole

[1] Ann. Ch. Phys., iii, 257, 288.

[2] Treatise on Chemistry, Vol. I, p. 449.

animal kingdom about 4,500,000,000 of kilogrammes of dioxide are daily added to the atmosphere.

The decomposition of vegetable and animal substances must obviously yield a large quantity of dioxide; but practically nothing is known of the nature of the change or of the amount produced. In order to fix our ideas we may assume that this source yields the same quantity as is yielded by man, viz.: 1,500,000,000 kilogrammes.

The last source mentioned is by far the greatest. Not only do active volcanoes produce, but extinct and volcanic districts join in increasing the quantity. Considering the immense area covered by these districts and the immense quantity of gas evolved, we must conclude that by far the greatest amount of atmospheric dioxide has been obtained from this source. In fact, Poggendorff calculated that ten times as much is produced by volcanoes as by all the other sources put together. Allowing for the amounts previously given being larger than similar numbers used by Poggendorff, we may take it that 40,000,000,000 kilogrammes are daily given to the atmosphere from these subterranean sources.

Finally, if we add together these various amounts, we find that the air is daily receiving an addition of about 50,000,000,000 of kilogrammes of carbon dioxide. Dividing the absolute amount contained in the whole atmosphere by this number, we find *that the amount of carbon dioxide in the atmosphere would be double what it is at present in about one hundred years if there were no means of compensation.* Poggendorff, who made a similar estimate, gave three hundred eighty-six years as the time required to double the amount of CO_2 in the air. The difference is explained by the absolute amount of CO_2 being at that time considered greater than it really is, and also by the amount of carbonaceous matter burnt being greater now than in his time.

The disagreeable nature of the reflections called up by this last calculation disappear, however, when we come to consider the

Compensating Influences.

The known causes at work removing the dioxide may be considered under three heads, viz.:

(1). Fixation of carbon by growing plants.
(2). Removal of dioxide by zoophytes.
(3). Absorption of dioxide by inorganic chemical actions.

The first cause here mentioned is one which is essential to vegetable growth. In estimating its magnitude we are met by the want of reliable experimental data, making it almost impossible to arrive at any definite conclusion.

Certain experiments have shown that speaking approximately, a square metre of leaf will decompose one litre of CO_2 per hour in sunlight. The area of the land capable of bearing vegetation amounts to 49,400,000 square miles. Assuming the area of leaf borne by this land to be 50 per cent of this amount, we find that about 63,726,000,000,000 of square metres of leaf are hourly engaged in purifying the atmosphere. This decomposing effect will go on for about ten hours out of every twenty-four, but will be less active in the autumn and winter months than in spring and summer.

Taking all things into consideration (allowing 25 per cent for diminution in winter) we find that vegetation daily decomposes about 478,000,000,000 of kilolitres of CO_2, or about 900,000,000,000 of kilogrammes, an amount which is much greater than the amount produced from all sources. The magnitude of the action exerted by plants is surprising, but it must be remembered that during darkness plants expire CO_2. No account is taken of this action here, as no quantitative experiments seem to have been made upon it. The author thinks that this action goes on to a greater degree than is usually supposed.

The second great action going on in nature is effected by the interposition of animal life. It consists in the removal from sea-water of the carbon dioxide held by it in solution, by certain low forms of animal life, such, for example, as the coral-builders and the foraminifera. (Certain other forms, such as brachiopoda and lamellibranchiata also fix CO_2 as calcic carbonate; their effects are, however, comparatively insignificant.) The immense influence exerted by these organisms is evident when we look at the vast masses of limestone occurring in geological formations of all ages which have an exclusively organic origin. Nor is this influence confined to the past. Recent deep-sea soundings have revealed the fact that foraminiferal life still flourishes in the depths of the ocean, while we know that coral-reefs are still being constructed in the warmer seas. In short, this cause must be considered as the chief one which is active in removing dioxide, but want of data prevents our estimating its amount.

The third action going on in nature, effecting the purification of the air, is a purely inorganic one. Included under this head are such processes as the conversion of feldspar into kaolin, the decomposition of such silicates as hornblende, pyroxene, etc. The large deposits of kaolin and decomposed feldspar, which are met with in the earth, sufficiently prove the magnitude of this action. Thus "a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 metres, an amount which evidently represents but a small proportion of the results of feldspathic decay in the sedimentary strata of the globe."[3] Evidently then here we have a cause which has an important influence in effecting the removal of atmospheric carbon dioxide. It is impossible, however, to estimate its amount.

General Conclusions.

It is evident that if the compensating influences are just equal in total effect to the productive ones, the amount of carbonic dioxide in the air remains constant, but an insufficiency of data prevents a definite answer being given to this. We know, however, that these processes have been going on for a very long time. In fact, there is already fixed in the earth an immense quantity of CO_2 equal to many hundreds of times the amount contained in the present atmosphere. This must have been derived from the atmosphere, but it is not necessary to assume that it *all* formed part, at one time, of the atmosphere. It must, however, be evident that at one time more CO_2 existed in the atmosphere than at present.[4] If this be so, the compensating influences must be more powerful than the producing ones, and the proportion of atmospheric dioxide must be diminishing.

[3] Dr. T. Sterry Hunt. *Chemical and Geological Relations of the Atmosphere.* American Journal of Science, May, 1880.

[4] Unless we accept Dr. Hunt's theory that space is filled with an atmosphere similar to our own, and that CO_2 fixed in the earth at one time formed part of this universal atmosphere.

A SIMPLE LABORATORY DEVICE. By ERNEST H. COOK, of Newton Villa, Southville, Bristol, England.

[ABSTRACT.]

IN this communication the author described an improvement in water and air ovens which he has employed with advantage in his own laboratory. This consists in applying the principle of the Bunsen water pump to these instruments. Thus instead of being closed boxes as is usually employed, a current of air is drawn through them and over the substance to be dried. With this simple addition, the time of drying is greatly reduced; and without any alteration of apparatus, substances can be completely dried at a low heat, and in fact at the ordinary temperature, by simply increasing the rapidity of the current of air traversing the chamber.

The same principle is sometimes usefully applied to small portable stink cupboards which stand upon the laboratory bench. The practical details are so simple and self-evident that a description is unnecessary.

THE ACTION OF WATER AT 100° C. ON THE β PHENYLTRIBROMPROPIONIC ACID. By LEONARD P. KINNICUTT and GEORGE M. PALMER, of Cambridge, Mass.

[ABSTRACT.]

THE action of boiling water on the *a* phenyltribrompropionic acid has been described by one of us in the American Chemical Journal for April. Since that time the study of the β acid has engaged our attention.

This acid is obtained by the action of bromine on monobromcinnamic acid, and is a crystalline solid soluble in alcohol, ether, bisulphide of carbon and chloroform. Its melting point is 151° C. and not 48° C. as given by Glaser (Ann. 143, P. 339), nor does it first separate out as an oil. The acid obtained by Glaser was evidently an impure substance.

On boiling with water the acid is decomposed, a bibromstyrol, whose structure we have not as yet determined, and the α monobromcinnamic acid being formed; also a small quantity of a new acid, very soluble in water, melting at 184°, which on analysis gave amounts of carbon, hydrogen, and bromine, corresponding very closely to what would be obtained if the substance was a phenylbibromlactic acid.

The formation of the same α monobromcinnamic acid, from both the α and β phenyltribrompropionic acid is not easily explained. It is probably caused by a change of position of the atoms within the molecule at the moment of its formation.

We are at present engaged on a further study of the above reaction, together with the products formed by the action of chlorine on both the α and β monobromcinnamic acids. *Chemical Laboratory, Harvard University.*

THE ACTION OF BARIC HYDRATE ON CHLORTRIBROMPROPIONIC ACID.

By C. F. MABERY and RALPH WILSON, of Cambridge, Mass.

[ABSTRACT.]

IN a previous paper by H. C. Weber and one of us, it was stated that chlortribrompropionic acid was decomposed when heated with baric hydrate. The products of this decomposition were not sufficiently studied to enable us to characterize them with any degree of accuracy, although the formation, to some extent, of tribromethylen was observed. In repeating these experiments we find that chlortribrompropionic acid, when heated with two molecules of baric hydrate in aqueous solution, is almost completely decomposed into baric bromide, carbonic dioxide and tribromethylen. The latter product was absorbed by bromine water, the excess of bromine allowed to evaporate and then crystallized from hot alcohol.

The following analysis was made:

0.1556 product gave 0.3657 grms. Ag Br + Ag Cl, which corresponds to 93.30 per cent 4 Br + Cl. Calculated for $C_2 Br_4 Cl H$ 93.40 per cent.

This substance melted at 33° and was probably identical with monochlortetrabromethan, melting point, obtained by Wallach and Bischof (Berlin Berichte, 1878–751) from the decomposition of dichloracrylic acid. Since monochloracetylen obtained in this reaction must have the constitution $C H{=}C Cl$, the product obtained by us must have the hydrogen and chlorine atoms attached to adjacent carbon atoms. This would give for chlortribrompropionic acid the structure $C Cl Br_2 C H Br COOH$, and for chlorbromacrylic acid the structure C Cl Br C H COOH, which H. B. Hill has already inferred from the relations of the two disubstituted acrylic acids.

On Certain Substituted Acrylic and Propionic Acids. By C. F. Mabery, of Cambridge, Mass., and F. C. Robinson, of Brunswick, Me.

[ABSTRACT.]

By the action of bromine on the dibromacrylic acid first obtained by Fittig and Petri from dibromsuccinic acid, we have made a tetrabrompropionic acid which differs essentially in its properties from the tetrabrompropionic acid obtained by H. B. Hill, and one of us (C. F. M.), from the dibromacrylic acid derived from mucobromic.

We have studied the salts of this acid sufficiently to characterize it. When heated with baric hydrate the acid is decomposed, tribromethylen being the chief product of its decomposition. By the action of alcoholic potash hydrobromic acid is eliminated with the formation of tribromacrylic acid which melted at 117–118°.

We have also tried the action of bromine monochloride upon brompropiolic acid. Although the investigation is not yet completed we have reason to believe that the elements of bromine monochloride are taken up with the formation of dibromchloracrylic acid.

Bromine monochloride was made by the action of chlorine upon bromine at 0° in a chloroform solution.

ON THE PRODUCTS OF THE DISTILLATION OF WOOD AT LOW TEMPERATURES. By C. F. MABERY, of Cambridge, Mass.

[ABSTRACT.]

THIS paper contains a description of the results which I have obtained in an examination of the volatile products of the dry distillation of wood. I have isolated acetic aldehyde and identified it by conversion into silver acetate which has been analyzed. Methyl formiate also was obtained in considerable quantity. It was saponified with plumbic hydrate and the plumbic formiate analyzed. I have found in the course of this investigation that methyl alcohol obtained by this process contains a much smaller per cent of aceton than is usually found in commercial wood spirit.

PRELIMINARY NOTICE OF A NEW ORGANIC BASE. By ALBERT R. LEEDS, of Hoboken, N. J.

[ABSTRACT.]

WHEN xylidine in alcoholic solution is treated with acrolein, the two bodies enter into combination to form acrolein-xylidine (its properties, analysis, etc., given), and water is eliminated. By sublimation, a limpid oil is obtained, which is cryptidine.

Description of its properties, chemical relationships, and certain of its salts.

ESTIMATION OF DEXTROSE IN SOLID COMMERCIAL STARCH SUGAR BY LOSS OF ROTATORY POWER ON SOLUTION. By HARVEY W. WILEY and C. A. CRAMPTON, of Lafayette, Ind.

[ABSTRACT.]

IT is not generally recognized that the amorphous starch sugars of commerce have the same property of losing rotatory power on solution and heating as if in a crystalline state. This depends on their large content of dextrose. The principle of determination used by the authors depends on the well known fact that a freshly prepared solution of dextrose has a rotatory power twice as great as after standing a few hours or heating to the boiling point for a few minutes. The authors show by tables of analysis that the dextrose can be determined in this way with reasonable satisfaction in all starch sugars made by high conversion. Sugars, however, which still have a large percentage of maltose and dextrine will not permit of their dextrose being determined in this way. This probably arises from the fact announced by Meisl that maltose solutions increase in rotatory power in the same circumstances in which dextrose solutions decrease. The authors exhibit curves showing the rate of fall in rotatory power of dextrose solutions.

DIRECT ESTIMATION OF DEXTROSE, DEXTRINE AND MALTOSE IN COMMERCIAL AMYLOSE (STARCH SUGAR). By HARVEY W. WILEY, of Lafayette, Ind.

[ABSTRACT.]

THE process is founded on a method of double reduction and polarization proposed by me at the Cincinnati meeting and since that time practically worked out.

The specific rotatory power and percentage of reducing matter of the sample are first determined. In another portion of the sample the whole of the reducing matter is destroyed by a slight excess of mercuric cyanide and the residue, after neutralization with hydrochloric acid repolarized in a tube two and a half times longer than the first and the number thus obtained multiplied by two. This gives the specific rotatory power due to *dextrine* alone, from which the percentage of dextrine present is easily calculated from formulæ given in my paper at the Boston meeting. Having determined the dextrine, the maltose and dextrose are calculated from formulæ given in the paper.

The paper contains tables of analyses showing the results obtained by the direct method compared with those obtained by best approved methods heretofore known.

On the Constitution of Benzole. By L. W. Andrews, of Brooklyn, N. Y.

[ABSTRACT.]

Claus maintains that Hübner's benzole formula is the one which best accords with the known facts of isomerism in the aromatic series.

This position is untenable, because the formula fails to explain the existence of three isomeric forms of bisubstitution products of benzole, unless on the basis of a method of interpretation which cannot consistently be carried out in all cases. Several considerations show that according to this formula the compound 1–2 should be identical with 1–4. These points are mutually interchangeable without rupture of the bonds or change in their distribution.

This fact is an infallible criterion of the identity or non-identity of the bonds 1–2 and 1–4. A model is shown to render apparent to the eye the peculiarities and imperfections of the "spider web" formula of *Hubner* and *Claus*.

SOME TEA ANALYSES, WITH SPECIAL REFERENCE TO THE DETERMINATION OF CAFFEINE. By W. HODGSON ELLIS, of Toronto, Canada.

[ABSTRACT.]

THE variation in the percentage of caffeine in tea leaves, reported by various chemists, is considerable, a circumstance due to the various methods of analysis.

After a number of trials of different methods, I have adopted the following process which gives in my hands the best results and which I have now used for several years.

1. The powdered tea is exhausted with boiling water under an inverted condenser.
2. The filtered solution is evaporated to dryness on the water bath with magnesia.
3. The residue is exhausted with absolute alcohol, and the filtered solution mixed with sand and evaporated on the water bath.
4. The residue is exhausted with hot distilled water and the solution again evaporated.
5. The residue is taken up with ether and the ethereal solution evaporated in a tared beaker and weighed.

By this method the caffeine is obtained colorless, and well crystallized.

Commercial tea as met with in Canada yields by this process from 1.5 to 3.5 per cent of caffeine.

In exhausted leaves, *i. e.*, leaves treated with boiling water, as in the ordinary method of making tea, the caffeine is nearly all removed, as in the following examples of three Japan teas and their exhausted leaves.

	1		2		3	
	Tea	Exhausted leaves.	Tea	Exhausted leaves.	Tea	Exhausted leaves.
Caffeine	2.5	.6	2.4	.5	3.7	.9
Ash	7.2	4.1	6.8	4.0	6.5	3.9
Soluble ash	3.8	.8	3.5	.8	3.4	.7
Silica	.9	.8	.7	.8	.5	.7
Extract	37.8	16.8	40.3	13.4	33.2	8.8

In the following analyses the caffeine was determined by this method; and the extract, by boiling the powdered tea under an inverted condenser with successive portions of water. The ash, soluble ash and silica were also determined in each case.

GREEN TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	30.8	5.0	3.6	0.5	2.2	19	28.7	6.8	3.0	0.5	2.4
2	43.4	4.1	3.7	0.2	2.4	20	31.8	6.4	3.8	0.5	2.6
3	43.4	6.2	3.6	0.4	2.3	21	34.7	6.1	3.7	0.3	2.6
4	45.2	3.6	3.6	0.3	2.0	22	34.2	6.8	4.1	0.5	2.7
5	38.0	4.4	3.8	0.3	2.5	23	33.7	6.2	3.4	0.3	1.9
6	39.2	3.0	3.3	0.5	2.6	24	33.0	7.7	3.9	1.1	2.1
7	45.2	5.2	3.5	0.5	2.3	25	34.8	6.8	4.0	0.3	2.6
8	40.2	4.0	3.4	0.3	1.9	26	35.2	6.7	3.0	0.9	2.1
9	41.2	6.1	3.9	0.5	1.9	27	35.0	6.9	3.7	0.8	2.4
10	39.4	3.8	3.6	0.7	1.8	28	36.4	6.9	3.2	0.9	2.9
11	37.9	4.1	3.7	0.9	2.8	29	33.8	6.0	3.3	0.2	2.1
12	38.6	4.6	3.6	0.3	1.9	30	27.4	6.3	3.8	0.4	2.5
13	45.8	4.6	3.6	0.5	2.4	31	34.2	6.3	3.6	0.6	1.7
14	38.5	3.7	3.6	0.4	2.3	32	36.0	7.3	3.1	1.3	2.0
15	35.2	4.3	3.1	1.0	2.6	33	39.0	9.9	2.7	3.5	2.8
16	37.4	5.0	3.8	0.3	1.8	34	38.0	6.4	3.7	0.4	2.1
17	41.3	4.7	3.8	0.4	1.9	35	45.0	6.1	3.4	0.4	3.0
18	34.1	6.2	4.1	0.4	2.3						

BLACK TEAS.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	27.8	6.3	3.1	0.8	2.4	6	33.0	5.6	3.4	0.2	2.6
2	29.8	6.4	3.6	0.6	2.4	7	37.6	5.8	3.5	0.2	1.9
3	31.1	5.7	3.5	0.4	3.7	8	37.2	6.0	3.9	0.6	2.3
4	31.1	6.8	2.9	1.0	2.3	9	31.0	6.5	3.5	0.7	2.3
5	36.5	6.4	3.8	0.4	2.6	10	27.2	6.3	2.9	0.8	2.3

MIXED OR NOT DISTINGUISHED.

No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.	No.	Extract.	Ash.	Soluble ash	Silica.	Caffeine.
1	29.1	5.7	3.6	.4	2.1	11	30.6	5.9	3.1	.4	3.0
2	35.6	6.9	3.8	.9	1.6	12	30.0	6.2	3.9	.1	2.3
3	26.5	5.9	3.7	.3	2.2	13	31.1	7.0	3.2	.1	2.6
4	20.2	6.8	3.0	.3	2.2	14	29.8	6.6	3.5	.4	2.2
5	26.2	5.7	3.2	.6	1.9	15	24.8	6.5	2.6	.8	1.7
6	38.2	6.2	4.0	.5	2.1	16	26.8	6.1	3.1	.5	2.1
7	35.4	6.2	3.5	.8	2.5	17	35.6	6.0	3.7	.3	2.0
8	37.2	5.6	3.7	.3	1.5	18	34.8	5.8	3.4	.2	2.6
9	23.8	6.5	3.5	.8	2.2	19	30.2	6.2	2.8	.5	2.0
10	20.2	5.4	3.0	.7	1.6	20	26.2	6.5	3.4	.2	1.8

The above analyses may be summarized as follows:—

35 GREEN TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	40.6	6.5	3.5	0.6	2.3
Maximum .	45.8	9.9	4.1	3.5	3.0
Minimum . .	28.7	5.5	2.7	0.2	1.3

10 BLACK TEAS.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	32.6	6.2	3.4	0.6	2.5
Maximum .	38.0	6.5	3.9	1.0	3.7
Minimum . .	27.2	5.6	2.9	0.2	1.9

20 TEAS MIXED OR NOT DISTINGUISHED.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	30.0	6.2	3.4	0.5	2.2
Maximum . .	38.2	7.0	4.0	0.9	3.0
Minimum . .	20.2	5.4	2.6	0.1	1.5

ANALYSIS OF THE WHOLE 65 SAMPLES.

	Extract.	Ash.	Soluble ash.	Silica.	Caffeine.
Mean . . .	36.2	6.5	3.5	0.5	2.2
Maximum .	45.8	9.9	4.1	3.5	3.7
Minimum .	20.2	5.4	2.6	0.1	1.3

PEMBERTON'S METHOD FOR THE VOLUMETRIC DETERMINATION OF PHOSPHORIC ACID. By PROF. G. C. CALDWELL, of Ithaca, N. Y.

[ABSTRACT.]

MR. H. PEMBERTON has given[1] a method for the volumetric determination of phosphoric acid in fertilizers, by means of a standard solution of ammonium molybdate, which consists simply in adding this solution as long as a precipitate is produced, the final tests for further precipitation being made with small filtered portions of the solution under examination. The standard solution is prepared by dissolving 89.543 gms. of ordinary ammonium molybdate in 1000 cc. of water; each cubic centimetre of such a solution precipitates

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[1] Journal Franklin Institute, March, 1882, p. 184.

3 milligrammes of phosphoric anhydride. The solution of the phosphate, first freed from organic matter and silica, is finally made up with as small an excess of acid as possible, and just neutralized with ammonia; 2 cc. of strong nitric acid and 10 gms. of ammonium nitrate are added, and the solution is heated to 60 deg., or above, preparatory to the addition of the molybdate; from the total quantity of standard solution required 0. 5 cc. is deducted for the excess necessary to neutralize the solvent action of the nitric acid.

With two modifications of the details as described by Mr. Pemberton, the process has satisfactorily sustained a careful trial in my laboratory. First, as to the preparation of the standard solution, the author found that the ordinary salt of the druggists was sufficiently pure to give the standard directly; but in order to clear away the slight turbidity which it usually leaves he added a little ammonia; it was however observed by Dr. S. B. Newbury, then analyst to the University experiment station, that in the case of the molybdate supplied to us, a notable degree of alkalinity was communicated to the solution by the necessary quantity of ammonia to clarify it; consequently the acidity produced by the 2 cc. of acid, added to the solution of the phosphate in the beginning, became so much reduced towards the end of the titration, that the results were unsatisfactory; in some instances the solution even became slightly alkaline before the precipitation was completed. Therefore, in order to secure perfect neutrality, the molybdate solution was filtered, after the addition of a few drops of ammonia at the most, and then standardized by a solution of phosphorus salt of known strength.

The second modification refers to the method of taking out the test portions to be tested for complete precipitation; even when this is done as described by the author, this method of determining phosphoric acid is unquestionably more expeditious than the usual gravimetric method with molybdate and magnesia mixture; but the necessity of filtering out each of these test portions in the ordinary manner through a filter is objectionable, and might deter many from using the method. For this filtration I used with great success a small suction filter, somewhat after Carmichael's idea, which could be plunged directly into the liquid, and through which a small portion of the same could be drawn at once into a small test tube in which the test for further precipitation was made.

The results of our trial of the method are set forth in the following table: three of the sets of determinations were made by three experienced chemists with solutions prepared by one of the number, each one being entirely ignorant of the results obtained by the others; the fourth set was obtained by another chemist with solutions of molybdate prepared and standardized by himself, and with his own solutions of the same phosphates that were examined by the others.

NO. OF PHOSPHATE.	I		II	III	IV
	Gravimetric.	Volumetric.	Volumetric.	Volumetric.	Volumetric.
237		11.18			
		11.20			
235		13.14	13.27		13.13
		13.14			13.18
					13.15
					13.12
259	12.47	12.33	12.39	12.44	12.34
	12.41	12.33		12.45	
				12.47	
				9.20	
261	9.15	9.26	9.08	9.05	9.34
	9.21	9.26		8.97	9.37
				8.97	
272	13.69	13.57	13.75	13.82	
	13.82	13.63		13.81	
		13.57		13.72	
		13.69			
		13.63			

From these results it appears that the method is capable of great accuracy, but, as in the case of No. 261, is liable to some source of error, which makes great care necessary in the preparation of the solution of the phosphate, according to the directions given by the author.

TITLES OF OTHER PAPERS READ IN SECTION C.

EXPERIMENTAL RESEARCHES ON THE SOURCES OF NITROGEN IN CROPS. By J. B. Lawes and J. W. Gilbert, of London, England.

OBSERVATIONS ON THE CONTAMINATION OF CITY WELLS. By R. B. Warder, of North Bend, O.

ON THE CAUSES WHICH RENDER FLOUR AND ORGANIC DUST EXPLOSIVE, WITH SUGGESTIONS FOR THE PREVENTION OF SUCH EXPLOSIONS. By Thomas W. Tobin, of Louisville, Ky.

SOME DERIVATIVES OF ISOPICRAMINIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

A BENZOYL ANHYDRO ACID FROM B-METAMIDOSALICYLIC ACID. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTES ON EFFECTS OF DIFFERENT SOILS UPON SOLUBLE PHOSPHATES. By Charles W. Dabney, jr., of Raleigh, N. C.

NOTE ON THE ABSORPTION SPECTRUM OF HUMIC ACID. By H. Carrington Bolton, of Hartford, Conn.

NOTES ON BONE OIL.[1] By Arthur H. Elliott and Fred. Sands, of New York, N. Y.

ON NITRO-SACCHAROSE.[2] By Arthur H. Elliot, of New York, N. Y.

TRANSMISSION OF GASES THROUGH LIQUIDS OF DIFFERENT DENSITIES. By C. G. Wheeler and F. Menzel, of Chicago, Ill.

THE SOLUTION AND LATE CRYSTALLIZATION OF GOLD HEATED WITH CHLOROHYDRIC ACID IN A SEALED TUBE. By Henry Carmichael, of Brunswick, Me.

REMARKS ON THE APPLICATION OF THE IRIDIUM KNIFE-EDGE TO ANALYTICAL BALANCES. By William Dudley, of Cincinnati, Ohio.

FIRE-DAMP INDICATORS. By Isidore Kitsee, of Cincinnati, Ohio.

NOTES ON THE ANALYSIS OF SUPERPHOSPHATES.[3] By THOMAS S. GLADDING, of New York, N. Y.

REPORT OF THE AGRICULTURAL CHEMISTS. By Chas. M. Stillwell, of New York, N. Y., Secretary.

[1] Published in full in the Journal of the American Chemical Society for Aug., 1882.

[2] Published in full in the School of Mines Quarterly for Nov., 1882.

[3] Published in full in the Am. Jour. of Chemistry, for 1883.

SECTION D.

MECHANICAL SCIENCE.

ADDRESS

BY

PROFESSOR W. P. TROWBRIDGE,

VICE PRESIDENT, SECTION D.

MEMBERS OF THE SECTION OF MECHANICAL SCIENCE:—

WHEN we look upon mechanical science as an abstract science, having for its foundation only a few simple axioms, dynamical laws, and geometrical theorems, and divest it of many of its complicated mathematical applications, we can hardly help regarding the science as complete.

The controversies and discussions which marked its growth, and which for centuries absorbed the attention of the scientific world, have ceased, and there is left to the present generation a clear, well defined science treating of the forces of nature and the movements which they produce, or are capable of producing, in the ordinary phenomena of force and motion.

Even the last great extension of this science in the development of the true theory of heat, which in its conception and importance was but a fit culmination of the labors of a long line of illustrious men, has now become universally accepted in all its significant features; and although some of the men are now living who were chiefly instrumental in establishing the purely mechanical nature of heat, and in bringing all its manifestations into harmony with mechanical laws, yet this result seems now to have been only the realization of anticipations long looked for, and long before predicted.

No such anticipations now lie before us to excite the zeal of the investigator. There are now no such controversies or opportunities for discovery to stimulate analytical investigation as marked the last and the preceding centuries. Whatever may be the nomenclature chosen, the forms used, or the classification of subjects, there is no longer any contention regarding the theorems established by Benedetti, Descartes, Leibnitz, Newton, D'Alembert, Bernoulli, and others; and more recently in our own generation, by Clausius, Rankine, Helmholz, and Sir William Thomson.

But while we inherit in all its completeness a science so comprehensive and exact, in its generalizations, as to embrace the movements of the bodies of the solar system on the one hand, and the infinitesimal motions of the molecules of bodies, producing the phenomena of heat, on the other, there is still an unlimited field open for experimental research.

As far as mechanical science is concerned it may be said that we live in an era of *applied science*,— an epoch which has already produced astounding and unexpected results, modifying and changing all the relations of civilized life.

Concerning abstract principles, no kind of knowledge is more widely diffused or universally accepted than that which relates to the subject we are considering. We live and move through the medium of appliances which are but the exemplifications of well known mechanical principles; and with the firm foundations of an exact science to build upon, there is an increasing activity of thought and action, directed, not to the discovery of new principles, but towards new methods, new applications, greater mechanical achievements.

To employ the language of mechanics, there exists at the present time an immense momentum of intellectual force, in this respect, which must either be properly directed to overcoming obstacles in the way of progress, or dissipated in costly failures.

Speculations as to what may be the results of this stored up mental energy, in future developments of the mechanic arts, are unprofitable; but well-directed efforts towards the determination of new applications, or the experimental verification of inductive demonstrations, are seldom without interest; and the more thorough and complete such investigations are made, the greater will be their subsequent value.

There is a wide range of applications of mechanical science to engineering, and in the arts, for which such experimental researches are now demanded.

The recent united appeal of the three principal engineering societies of this country, urging the continuance of appropriations by the United States Government for experiments on the strength and properties of the materials used in engineering structures, is evidence of the want that is felt of a better knowledge of this particular subject.

Among the forces of nature with which we have to deal, there is one class of which the laws have not been subjected to exact determination.

Whatever may be the nature of the forces of gravity and inertia, the laws governing their action are definitely determined; but of the molecular forces which resist the disruption of materials, and which govern their action under the influence of external forces, but little is definitely known: all that is known has been derived from special experiments on specific forms and qualities of materials, which, from the very nature of these materials, are not yet sufficient for general or universal applications. And when it is considered that not only the cost, but the absolute safety, of all engineering structures depends upon this uncertain knowledge, the importance of the question becomes evident. So little has been done in this country in many branches of this subject that reliance is placed almost wholly on the results of investigations made abroad—investigations which were never entirely applicable to the material found here; and which, on account of the rapid changes in the arts of manufacture and fabrication, have become to a certain extent obsolete, or of very uncertain value.

In relation to metallic products, it may be said that a new field of technical inquiry has sprung up, lying between the science of metallurgy and the profession of engineering.

It can hardly be said that the metallurgist is required to answer all questions relating to the properties of the metals in the forms in which they reach the engineer. These properties are determined or modified greatly by the processes of fabrication required to transform the raw materials into the shapes in which they are used. It does not belong to the metallurgist to trace these materials through all the transmutations which they may undergo before being put to use. The very conditions of use often require special treatment, and do certainly enter largely into the inquiries which must be instituted to prove their fitness for special purposes. On the other hand the engineer has not usually the appliances nor the opportunities, for fully testing the materials he employs. Time, money, and apparatus are necessary, neither of which can usually be spared from his ordinary professional engagements.

For the action of the ordinary materials used in this country reference can only be had to the results of researches by private individuals made on a limited scale, or to more general and systematic, but less applicable, results of experiments instituted by foreign governments.

It is not to be expected that any system of experiments, however carefully conducted, on the properties of the metals, will cover every special case, but there are standard products, and standard processes of fabrication, for which exact determinations of absolute strength, ductility, elastic limits, and other properties, under given conditions of use, may be ascertained.

The rapid changes which are taking place in the production of iron and steel, especially, render such investigations on a scale commensurate with the importance of their useful applications a matter of the highest interest.

It is becoming, in fact, a reproach to our country, that for such investigations, which, on account of the magnitude of the work to be accomplished, can only be undertaken at public expense, so little has been accomplished here. Our engineers are often obliged to refer to experiments made nearly half a century ago for the numerical data necessary for designing the most simple structures.

The names of Hodgkinson and Fairbairn will always be mentioned with veneration, and held in honor, as men whose experimental researches have for more than a quarter of a century formed the groundwork of our structures in iron. But even the youngest engineers become restless under the necessity of constant reference to results which they recognize as belonging to the past and as not wholly applicable to the present. Much useful knowledge has been, from time to time, contributed by our own engineers on this subject, but with some exceptions their results have been generally limited in their range of applications, and not sufficiently comprehensive to become of universal use, on account of want of time and means for prosecuting extended investigations.

It is often argued that it is essential for an engineer to make special tests of the materials he employs in the construction of all important works. While this is undeniably true to a certain extent, it is also true that an engineer cannot *design* a structure without *assuming* certain data, among which, and the most important of which, are the properties of the materials he proposes to use.

Truly scientific construction demands that all the parts of a structure shall be in harmony, in weight and strength, with the external forces which act upon them, under the conditions of use to which they are subject; whether such a structure be a machine, a bridge, or a public building, the designer must base his construction on these truths, and the want of correct data in any element of the design must entail either loss or damage.

The influence of experimental research in building up the science of pure, or abstract, mechanics shows the impossibility of progress without direct reference of probable laws to the test of experiment.

The dynamic theory of heat remained a speculative question until a few simple and decisive experiments settled all controversies, and opened the way for Rankine and Clausius to develop the new and before unknown science of Thermo-dynamics. We find La Place enunciating this theory in specific terms, and in language identical with that now employed, when merely stating the opposite views of heat entertained in his day; the great mathematician himself upholding until his death the material theory — a theory which is now mentioned only to illustrate, what seems to us in the light of later developments, almost unaccountable ignorance on the part of some of the most learned scientists of the past age. It is true that years of controversy and discussion finally prepared the minds of scientific men for the acceptance of the dynamic theory; but, nevertheless, it required the experiments of Joule to furnish the proof of the theory by showing the absolute identity of heat phenomena, of the most simple kind, with those of ordinary mechanics. And when, finally, through the experiments of Regnault on the specific heats of gases, means were afforded for an independent determination of the mechanical equivalent of a unit of heat, controversy and discussion ceased. These two independent determinations, one by direct experiments and the other through analysis employing the specific heat of air as finally determined by Regnault, were indisputably, when taken together, and especially in view of the importance of the results, the most interesting and important instances of the value of experiments in establishing exact theories in the whole history of science.

The student of mechanics of to-day may not see before him the prospect of brilliant discoveries of new principles, but by patient and faithful labor in the field of experimental investigation, there are still opportunities of adding to mechanical science a knowledge of truths which may have a lasting and beneficial influence in promoting the applications of this science to the Arts and Industries.

I need but refer in this connection to that other series of experiments of Regnault on the elastic force and temperature of vapors, as one of the most signal instances of the value of experimental research, when properly conducted, which can be mentioned. Although by these experiments nothing was added to establish the theory of heat, yet in connection with the universal use of steam as a motive power, and for other purposes, the results have been of vast importance. For accuracy and completeness, these investigations stand unrivalled. They form the basis of all the important applications of the theory of heat to vapors, and thus constitute the foundation for all calculations relating to steam in its various uses. They became not only of immediate use to the practical engineer, but they enabled Rankine, Clausius, and Zeuner to extend the dynamic theory to the study of vapors, and thus to cover all ordinary technical grounds.

Of all the subjects in which experimental research is now demanded, there is none more important than that to which allusion has been already made: the strength and properties of materials used in constructions. It is needless to specify the various kinds of investigation demanded, for our knowledge of the subject, as far as the materials produced in this country are concerned, is in a most uncertain and unsatisfactory state. Boldness of design, and a necessary increase in magnitude of the various parts of structures, are the inevitable tendency of engineering progress. Even in commercial buildings, in which iron scarcely entered as an important element twenty years ago, single cast iron columns have recently been introduced which are required to sustain, permanently, nearly one thousand tons. Similar instances of the use of wrought iron and steel in tension and compression might be given in which for the large cross-sections employed no reliable data for strength is available.

A most important question to be considered where experiments involving large expenditures, expensive apparatus, and much time, are to be made, is the constitution of the body or commission having charge of such investigations. It is doubtful whether any body of scientific and practical men, however competent they may be, can conduct and control properly the details of such important and laborious work, if they are at the same time burthened by the cares and demands of other private or professional duties. Success in scientific work of this character depends largely on each specific investigation being committed to one person employed solely for the purpose, and whose acquirements and enthusiasm for his subject, need the least amount of advisory aid.

A commission of experts may well direct the character and aim of special investigations, and to some extent the methods to be employed; but without the stimulus of personal responsibility, combined with ardent zeal and special aptitude, of some one who has entire control of details, and who through his constant devotion to one object may reap the principal rewards of his own discoveries, experimental investigations of this kind are apt to end in failure.

Other subjects on which further researches are desirable will readily present themselves. Among these may be mentioned, as of much practical interest, the flow of air and steam through pipes and conduits.

The transmission of power by compressed air, and the adaptation of apparatus for ventilating and heating public and private buildings, require a knowledge of the resistances and other phenomena connected with the flow of air and steam, the available data for which are meagre, and rest upon very incomplete experimental foundations. The complications of this subject, arising from the changes of temperatnre of the flowing air or steam, and the difficulties thereby caused in attempts to measure quantities of the flowing liquid, render experiments in this direction so tedious and expensive as to operate almost as a bar to ordinary individual efforts.

The importance of the questions involved, especially in connection with sanitary appliances, is, however, becoming greater each year. In this connection also the laws of the transfer of heat, and their technical applications, assume new interest. In many of our large cities works have been erected or are in progress, involving great expense, having for their object the supply of steam for heating and ventilating purposes, and for power, by transmitting the steam in underground pipes throughout large districts from a few central stations.

These undertakings present numerous problems which have not heretofore been encountered in engineering operations. Correct data concerning the laws of the flow of gases and vapors, and the transfer of heat, constitute an important element in many of these problems. While compared with similar data for the flow of liquids, it can hardly be said that the knowledge available to the engineer is reliable or satisfactory.

In the construction of public buildings, private dwellings, and factories, the necessity of including in the first design complete arrangements for warming and ventilation, has of late years been forced upon architects and engineers by a public demand for healthful arrangements; and all the scientific and technical knowledge that can be brought to bear on the subject is desirable. In this direction but little has been accomplished in this country, except in connection with private practice. For information that cannot be obtained from this source, recourse must be had to foreign works, some of which, notably those of Peclét and Morin, may be taken as examples of the great value of such experiments as were made by these distinguished experimenters and authors.

It would be impracticable to present in detail all the principal subjects of practical mechanics in which more exact knowledge is needed in technical applications. There is a universal activity at the present time, everywhere, towards overcoming obstacles to what is called progress.

The demand is constantly for greater achievements. The public calls imperiously for greater speed on railways and steamships, and the engineer must meet the demand. Greater speed requires heavier rails, heavier engines, larger ships, and stronger machinery. But the same imperious public also insists on absolute safety, and reasonable cost. A train must fly at high speed across long bridges, built at the least possible expense, but woe to the engineer if a calamity occurs. No explanations are received; the public will accept no part of the blame.

This increasing ambition for something faster, grander, more convenient, or more healthful, brings with it increasing risks and responsibilities to professional men. The members of the engineering profession, especially, have to assume great risks, and it is not unreasonable that they should ask public aid, as our engineering societies have done, to arrive at a kind of knowledge that can be procured in no other way than through public aid and expense, and without which their best efforts may often result disastrously. If we include in these general propositions other branches of technical science, especially Chemistry and Physics, it would not be hazarding too much to say that the continuance and permanence of what is usually called modern progress depend greatly on a general recognition of the importance of this kind of scientific research.

There is one field of research which alone may well occupy the attention in this respect of the best minds of the age, and which ought to receive the most generous public support. That field is sanitary science, or sanitary engineering. Wordy controversies can never satisfactorily settle questions of ventilation, which involve force and motion, the heating of air and its conduction and distribution; nor questions of poisonous emanations or solutions, in air or water, nor questions of house and street drainage. Connected with these subjects there are abundant opportunities for systematic experiments, not only in the domain of physics and chemistry but of mechanics.

We cannot expect to rival the nations of antiquity in architectural achievements, although it would seem that lavish expenditures on monumental buildings most readily satisfy public ambition even in this age; but it is to be hoped that the generous impulses which have begun to manifest themselves in the direction of sanitary improvements may absorb a full measure of this ambition and direct it towards the most perfect provisions for the health of communities, so that social life for the poor as well as for the rich may rest on the highest possible plane of physical as well as mental and moral excellence. A small share of the millions expended by the people in the architectural masonry and decorations of public buildings, would, if properly devoted to mechanical, physical, and chemical researches connected with sanitary engineering, accomplish much towards this end.

There is undoubtedly much force and wisdom in the suggestive remark of one of the most distinguished sanitary engineers of England, that "if he were permitted to build the homes of a people, he would not care who should write their songs, or make their laws."

There is a branch of mechanics which has from time to time excited considerable public attention, but which, from its not presenting features of great public utility, has not been ranked among those having a commanding importance.

It is only necessary to mention *animal mechanics* to bring to mind the beautiful experiments of Marey in determining the movements of animals in the ordinary processes of locomotion. A study like that of Marey of the mere motions of the locomotive appendages of animals — the curves described by the extremities in the operations of flying, walking, and swimming — must always remain barren of useful results unless connected with the dynamical features of the problems involved. These Dr. Haughton, of Trinity College, Dublin, has supplied, by a most interesting treatment of the subject, and by careful experiments, extending over a period of ten years.

Muscular force is so related to life that it appears almost to furnish the only unmistakable evidence of vitality. When the involuntary and voluntary muscles of the animal system cease to act, life can no longer be said to exist. Dr. Haughton has discussed the whole subject of muscular forces not only in their relations to the skeletons through the medium of which they act, but in their properties of strength, endurance, refreshment, and fatigue, in such a manner as to throw new light on this interesting subject, and to suggest its real useful aspects in connection with the laws of health and physical development.

The conformity of the phenomena observed and the principles established, in regard to movements connected with animal life, with the laws of ordinary mechanics, is complete. The four elementary machines, which constitute either singly or in various combinations the bases of all artificial machinery, are not only found to be represented in the various kinds of animal locomotion, but, except perhaps in some of the very lowest orders of animals, no other means of locomotion can be discovered.

In relation to the expenditure of energy, it appears to have been demonstrated also, that the sources of muscular, and probably of nervous energy, are, as in the ordinary expenditures of force, subject to exhaustion at rates which increase in a high degree with the rapidity with which work is performed.

The space which a steamship can traverse, with a given amount of coal for consumption, varies inversely as the square of the speed; and the number of days required for the exhaustion of the fuel, inversely as the cube of the speed. Similar laws are applicable, as Dr. Haughton has shown, to human exertions and furnish scientific and mechanical reasons, why the prolonged, intense nervous activity of business life, so much complained of in these days, results in premature and permanent disabilities. Doctor Haughton was the first to demonstrate experimentally the laws of fatigue and refreshment of muscular action, and to suggest, at least by very natural inferences, that there are limits of exertion within which alone these processes may be expected healthfully to balance each other.

In one sense all great works, successfully executed, furnish data and precedents for the future. Each one reaches out beyond its predecessor, the result being the gradual building up of an art, an industry, or perhaps a profession.

In exceptional cases a single construction, or a single exploit, revolutionizes practice. Two episodes of our late war present signal examples in this respect. The conflict between the Monitor and Merrimack in Hampton Roads furnishes the first real experimental test of the invulnerability of iron-clad vessels, and the efficiency of the turret system devised by Ericsson; while the demolition of Fort Pulaski by General Gilmore, by batteries placed at one and two miles distance, destroyed, in a day, all faith in defensive works of masonry, which up to that time had been regarded as the bulwarks of seacoast defence. History furnishes no instance of so complete and universal a change in practice caused by one gigantic experiment occupying hardly more than twenty-four hours. The echoes of Gilmore's guns had scarcely died away before orders were issued from the war offices of all nations having seacoasts to defend to suspend work on masonry fortifications.

These two events, the conflict of the Monitor and Merrimack, and the reduction of Fort Pulaski, were the beginnings, however, of a contest which has lasted to the present time — the contest between rifled ordnance and defensive armor. In this war of force, of impact and resistance, if we take into account the cost of armored vessels, hundreds of millions of dollars have been expended.

In the course of ordinary events, however, isolated results do not furnish the most useful collection of facts; there are few branches of art or industry, involving mechanical appliances, which may not be advanced by investigations systematically undertaken, and which are made through a careful observance of all phenomena and conditions to bring out general laws.

It will not be considered inappropriate in connection with this subject to make a brief allusion, and render tribute to the works of one who, though not now living, may be regarded as having been among the foremost investigators of the present age in the department of Mechanical Science. No name is more frequently mentioned, wherever the English language is spoken, as authority in matters relating to applied mechanics than the name of RANKINE. His original investigations in founding, and building up, the science of thermo-dynamics, were sufficient to establish his fame; but the peculiar fascination of his works, to the engineer, lies in the wonderful scope of his genius in the solution of problems of applied mechanics in every branch of engineering, and the remarkable skill and perception with which he introduced experimental data into his researches; thus giving to his deductions the most unerring scientific exactness combined with the nearest approximate practical solutions.

Unlike the writings of many scientific authors, Rankine's voluminous works will not bear reduction or condensation. Conciseness of style and the shortest modes of demonstration are their marked characteristics. Every paragraph, and almost every line, seems to contain the expression of an idea, a valuable description, or the enunciation of a principle.

As an investigator he appears never to have touched a subject without throwing new light upon it, either by his mode of treatment, the discovery of new principles, or in extending its practical applications. Although not an experimenter himself, no investigator has made more constant use of well established results of experiments made by others, or has been more skilful in bringing useful rules of practice out of the combinations of such results with exact theories. He has left to the present generation the most complete exposition of the principles of the science of applied mechanics that can be found in the English language.

England has not yet done full justice to the memory of one whose labors in science ought to be regarded as having added to her national fame, and whose practical expositions have contributed so much to the progress of all nations in the Mechanic Arts.

PAPERS READ.

ON A NEWLY DISCOVERED ABSOLUTE LIMIT TO ECONOMICAL EXPANSION IN THE STEAM ENGINE AND IN OTHER HEAT-MOTORS.

By ROBERT H. THURSTON, of Hoboken, N. J.

[ABSTRACT.[1]]

IT has been universally assumed by engineers, and probably by all physicists familiar with the theory of the steam engine hitherto accepted, that the best ratio of expansion, whether for maximum efficiency of fluid, of engine, of capital, or of plant,[2] increases with increase of steam pressure without limit, and that such ratio may be indefinitely increased with decrease of the ratio of back-pressure for any one kind of engine, notwithstanding the fact that the value of the ratio of expansion is modified by variation of the conditions of working, even where the ratio $\frac{p_b}{b_1}$ is the same. The writer proposes to show that, in every actual engine, in every engine operated under the conditions of real work and of usual practice, there exists a limiting value of any of these "ratios of maximum efficiency" beyond which it cannot be economically raised even with an infinitely elevated boiler-pressure. It will be further shown that this "absolute limit" may be readily, and probably often is, passed in everyday practice, that in the usual forms of steam engine an absolute limit exists within or not far beyond the customary working range of expansion, beyond which expansion cannot be carried with economy, however high steam pressure may be adopted; in other words, with infinite pressure, the economical value of the ratio of expansion will be found often not merely finite, but sometimes probably within the limits of familiar practice.

In earlier papers[3] the writer has shown that the conditions under which engines are worked in actual practice differ in a very important degree from those assumed in the simple thermo-dynamic theory of heat engines, and, especially, that the unjacketed steam cylinder, which is usually assumed to be of non-conducting material, is to such a serious extent a reservoir and a transferrer of waste heat that the efficiency of the engine is greatly and sometimes enormously reduced.

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[1] This paper is published in full in the JOURNAL OF THE FRANKLIN INSTITUTE, Sept., 1882.

[2] *Vide* a paper "On the Several Efficiencies of the Steam Engine," etc., JOURNAL OF THE FRANKLIN INSTITUTE, May and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1882.

[3] JOURNAL OF THE FRANKLIN INSTITUTE, May, 1881, February, May, and June, 1882; *Transactions of the American Society of Mechanical Engineers*, 1881-2.

The equations which would give values of the ratios of maximum efficiency for the ideal "perfect engine," therefore, do not apply to real engines," and the author has deduced[4] modified equations, which are at least approximately correct for real practice. He has also produced true "curves of efficiency" from real engines, which were found to be practically the same in form and location, to be practically identical in character, with those inductively obtained, so far as the two could be compared.

"THE GENERAL EQUATION OF ALL STEAM ENGINE EFFICIENCIES," is

$$\frac{1}{E}=\frac{A v_2+B v_2 r^{-1}}{2R\left(b p_1 v_2 \dfrac{n r^{-1}-r^{-n}}{n-1} r^q - p_b v_2\right)} \qquad \text{(A)}$$

which becomes a minimum, and makes the *commercial efficiency* of an engine doing the required work a maximum when, to obtain r, we have made

$$r^q+\frac{q}{M(q-1)}r^{q-1}-\frac{q-n}{n(q-1)}r^{q-n+1}$$

$$\frac{q-n+1}{Mn(q-1)}r^{q-n}=\frac{n-1}{Mnb(q-1)}\,\frac{p^b}{p_1} \qquad \text{(B)}$$

The ratio $\frac{p_b}{p_1}$ is the quotient of the total back pressure by initial pressure, all useless resistance being included in p_b. A is the measure of annual costs, variable with size of engine, B of costs variable with quantity of steam demanded, v_2 the volume of steam-cylinder, r the ratio of expansion, R the revolutions per annum, b the proportion of steam in the cylinder when $r=1$, p_1 is initial steam pressure, n is the exponent of the familiar equation of the steam line, $pv^n=$ const., and r^q is the rate of variation of losses with increase of r. $M=\frac{A}{B}$.

When instead of M, N is inserted in the equation, N being the quotient of *all* annual expenses, independent of fuel supply, by all such expenses variable with fuel and steam supply — the expression determines the ratio of expansion at "maximum efficiency of capital invested in a *given plant*."

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[4] "On the Several Efficiencies of the Steam Engines," etc.

When M or N is made zero, the expression reduces to:

$$\left(r^{q-1} - \frac{q+1-n}{q\,n}\,r^{q-n}\right) \div \frac{n-1}{b\,q\,n} = \frac{p_b}{p_1} \qquad \text{(C)}$$

and gives the ratio of expansion at which maximum "duty" or "efficiency of engine" is attained.

When $b = 1$ and $q = 0$, the case becomes that of the ideal engine working the fluid in a non-conducting cylinder, and these equations become, for efficiency of capital and of engine,

$$r^{-n} - \left\{\begin{matrix} M \\ \text{or} \\ N \end{matrix}\right\} \frac{n - n\,r^{1-n}}{n-1} = \frac{p_b}{p_1} \qquad \text{(D)}$$

$$r^{-n} = \frac{p_b}{p_1} \qquad \text{(E)}$$

Studying these equations, it will be seen that, in all except the last (E), it is possible to find finite values of r such that their left-hand members shall reduce to zero; as in them n is nearly always equal to unity; q varies from $q = 0$ to $q = 0.3$ in good practice, and b usually ranges between $b = 0.8$ and $b = 0.9$; M or N is usually between 0.02 and 0.15, and the form of the function is such that the first member may always be made to disappear for some finite value of r. Then we shall have in (B)

$$r^{q} + \frac{q\,r^{q-1}}{M(q-1)} - \frac{(q-n)r^{q-n+1}}{n\,(q-1)} - \frac{q-n+1)}{Mn\,(q-1)} = o; \qquad \text{(F)}$$

and in (C)

$$r^{-n} - M\,\frac{n - n\,r^{1-n}}{n-1} = o$$

$\frac{p_b}{p_1} = o;\ \frac{p_1}{p_b} = \infty$ and the value of r, at which this condition is obtained, constitutes an "absolute limit," for the case taken, beyond which expansion cannot be carried economically, even with steam increased to infinite tension; beyond this point $\frac{p_b}{p_1}$ becomes negative, indicating the assumption of impossible conditions.

Examining equation (E) we find no such limit; the sign of the first member remains positive for all values of r, and can never become zero for a finite value of that quantity. We are thus taught that an important difference exists between the *ideal* engine, with its non-conducting cylinder, and the *real* engine working steam in a metallic cylinder, as well as between the case of maximum efficiency of engine and that of maximum efficiency of capital. In the cases of maximum efficiency of fluid and of engine for the ideal perfect engine, equation (E), only, is it true that indefinite increase of steam pressure permits indefinitely increased expansion. In all other cases an absolute limit exists, fixed for each case, beyond which expansion cannot be economically carried.

The above equations for real engines are approximate, and are practically exact — the values of the constants being determined with accuracy — within the range met with in practice, and the conclusions here deduced are, therefore, correct, although the exact values of the constants, or even the precise form of the variable function, r^q, here may not be fully ascertained.

Should it seem desirable, it is easy to deduce the same conclusions — and thus to confirm, by independent proof, the above proposition — by the examination of the "Curves of Efficiency" of ideal and of real engines.[5]

Thus it is seen that, while the ratio has no limit for the ideal case, it has such a limit for the real engine, and that this limit may be found at a low ratio of expansion. The writer has made this comparison for the U. S. steamers "Michigan," "Georgiana," and "Bache," for which three cases the real curves have been obtained by him,[6] and finds that, these curves remaining unchanged,[7] it is impossible economically to increase the ratio of expansion in such engines beyond three, five, and ten respectively, even with unlimited steam pressure; *i. e.*, even when $\frac{p_b}{p_1} = 0$.[8]

We may therefore conclude:

(1) That in all engines there exists an "absolute limit to the economical expansion of steam," whether considered with reference to efficiency of fluid, of engine, or of capital, which limit can not be passed, whatever pressure of steam may be carried up to the point of cut-off.

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[5] With such curves the ordinates measure the work done at various ratios of expansion in any given engine, by quantities of steam proportional to the abscissas.

[6] *Vide Transactions of the American Society of Mechanical Engineers*, 1882; JOURNAL OF THE FRANKLIN INSTITUTE, June, 1882.

[7] The form and location of these curves at very high pressure would undoubtedly be somewhat altered, but it is evident that there must still be found this absolute limit so long as condensation increases with extended expansion. The writer is inclined to believe that the form of the curves will be substantially as shown, however, at any attainable pressure.

[8] The "Michigan" has an unjacketed condensing engine, using saturated steam; the "Georgiana" a similar class of engine, with superheated steam; the "Bache's" engine is jacketed and compound.

(2) That this limit is found at higher ratios of expansion as the type of engine is more efficient, but that the limit is indefinitely removed only in the ideal engine, and then only as affecting the ratios of expansion at maximum efficiency of fluid and engine.

(3) That this limit is found at a small value of the ratio of expansion in ordinary engines, and therefore may be readily passed in every-day practice.

The limit is not far from three in the common, unjacketed condensing engine, four or five in the same engine using superheated steam, and ten or twelve in the ordinary compound engine.

(4) It is evident that the general propositions of this paper are true of all heat engines having fluid working substances, whether vapors or gases worked in metallic cylinders.

TRANSVERSE STRENGTH OF LARGE SPRUCE BEAMS. By GAETANO LANZA, of Boston, Mass.

[ABSTRACT.]

THE moduli of rupture and the moduli of elasticity of timber, given in our text-books and engineers' hand-books for practical use in construction, the former for determining the breaking weight of beams, and the latter for determining their deflections under given loads have, in almost all cases, been deduced from experiments on small beams, not at all comparable in size with those in common use in building.

While such a course is, to-day, recognized as misleading by our best engineers, it has been and is still followed very extensively, because, with a very few exceptions, we have had no experiments on the strength or deflection of full size wooden beams subjected to a transverse load.

Again, no experiments, as far as I know, have been made on the large scale on the strength of such timber joints as are used in framing, where mortises and tenons are employed, or where notches are cut, and we have had no experimental basis from which to deduce their strength, so that the loads which have been supposed or assumed to be their breaking loads are probably very far from the true breaking loads.

In view of these facts, and of the practical importance of experiments on full size pieces to the engineer, the architect, and the builder, I have been so desirous of having, at the Massachusetts Institute of Technology the means of carrying them on, and of giving my students the opportunity of performing them, that I have had constructed a machine capable of testing a beam transversely loaded with a load not greater than 50,000 pounds, provided the beam has a length not exceeding twenty-five feet.

By its means we are also able to test most of the framing joints used in practice. The results actually obtained by testing spruce beams are given in the following table:—

Number of test.	Width and depth in inches.	Distance between supports.	Manner of fixing the ends and loading.	Breaking weight in pounds.	Modulus of rupture in lbs. per square inch.	Trautwine's form. Central breaking weight of beam 1" square and 1' long supported at ends.	Barlow's form. End breaking weight of beam 1" square and 1" long, fixed at one end only.
1	2 x12	6' 8"	Framed at ends.	6,304	....	....	....
2	4 x12	6' 8"	Header.	10,338	....	....	....
3	2 x12	15'	Load at middle.	5,794	5,432	302	906
4	2 x 9	6' 7.5"	" "	7,227	5,320	295	887
5	2 x12	15'	" "	5,586	5,237	291	873
5 (*a*)	2 x12	7'	" "	8,982	....	....	....
6	2.75x 9	6' 8"	" "	7,586	4,082	226	680
7	3 x 9	4'	" "	11,086	3,285	183	548
8	3 x 9	10'	" "	6,086	4,508	250	751
9	3 x 9	15'	" "	5,066	5,651	314	942
10	3.88x12	20'	" "	6,566	4,253	237	709
11	2.5 x13.5	10'	" "	9,585	3,787	210	631
12	3.75x12	16'	Load 4.5' from one support.	7,585	3,271	182	645
13	3.75x12	16'	Ditto mortised.	9,085	....	....	....
14	7 x 2	7'	Load at middle.	1,944	8,748	486	1,458
15	1.75x 6.75	7'	" "	4,785	7,562	420	1,260
16	3 x 9	6' 8"	" "	9,985	4,931	274	822
17	3 x 9	6' 8"	Load distrib. at 4 points, 16" apart.	16,744	4,961	276	827
18	3.9 x12	16'	Load 4.5' from one support.	12,585	5,218	289	869
				13)59,936	4,610	256	768

As to the quality of the timber, it was, with the exception of Nos. 14 and 15, all common merchantable stock, its average modulus of rupture being 4623 pounds per square inch, the largest being 5651.

Nos. 14 and 15 were cut in 1877 and had been seasoning on the wharf since that time till they were tested in April, 1882.

Nos. 1, 2 and 13 are tests of framing joints, and hence from them we cannot determine a modulus of rupture. No. 5 (*a*) was a piece sawed from one end of No. 5, after this had been broken; hence, from this no modulus has been deduced.

The question must now be answered how these moduli compare with those commonly given in our text-books and engineers' handbooks.

	Max.	Min.	Mean.
Hatfield gives	12,996	7,506	9,900
Rankine gives	12,300	9,900	11,100
Laslett gives	9,707	7,506	9,045
Trautwine gives			8,100
Rodman gives			6,168
These tests have given for merchantable stock	5,651	3,271	4,623

Hatfield's, Laslett's, Trautwine's and Rodman's figures are from their own experiments, and, as is evident from the above table, they differ very considerably. Trautwine advises for practical use to deduct one-third, on account of knots and defects, hence after making this allowance he would use 5400. My own figures would give this only for the best of the stock in common use, and in the case of knotty and poor timbers would give much lower values; I obtained as low as 3271 and an average of 4623.

It will be observed that although Rodman's result is not at all as small as mine, it is smaller than any of the others; this is to be accounted for by the fact that the pieces which he experimented on were 5¾ inches deep, 2⅞ inches wide, and 50 inches between the supports, being larger than those generally employed by other experimenters, though not at all as large as those used in practice.

Only three tests have been made to determine the modulus of elasticity, and these have given respectively as follows:—

No. 3	1,237,215
No. 4	1,067,893
No. 6	938,453
	3) 3,243,561
Average	1,081,187

The machine has only been at work about two months, and hence no more work has yet been done, but these and other series of tests will be made in the future, and it is hoped will furnish us some more information of practical importance.

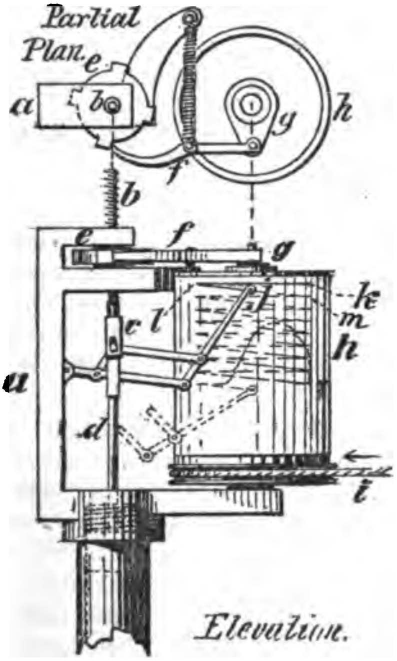
NOTE.—Since the above paper was delivered a much larger number of tests have been made, and a great deal more information obtained on the subject, a part of which was published in the Franklin Institute Journal for February, 1883.

A METHOD OF CUTTING SCREWS OF INCREASING PITCH. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

A SCREW whose pitch increases uniformly from one end to the other may be cut by the following method.

Mount a suitable cutter, say one formed like a short reamer, with teeth also on the end surface (a "Brown and Sharpe" cutter for the teeth of wheels could also be used), in a milling machine, and on the bed of the same fasten two journals, so arranged that the blank for the screw may either revolve or slide in them without shaking. The screw-blank as it rests in these journals must, of course, be in the proper relation to the cutter, so that if it were moved in the direction of its axis, a groove parallel thereto would be milled in the blank, and if it were revolved a groove would be milled around the blank, while by a suitable combination of these motions any desirable screw would be milled. Suppose now that by some means, we get, say, a quarter of a turn of a screw of any form milled, and that we then attach to the bed of the machine a pin, of the same size as the cutter and somewhat in advance of it, to act as a nut. The end of this pin must fit in the groove in exactly the same way as the cutter itself, so that by turning the screw-blank it will feed past this pin as if it were a small portion of a nut. It will now be seen that by arranging a feed to revolve the screw-blank we can make the latter screw forward past the fixed pin, and that, therefore, the cutter will extend the groove indefinitely. It is also clear, that the form of the original portion of the groove (the form of the section of the groove is not the form meant) will be repeated throughout the screw.



In order now to make a screw whose pitch shall increase in arithmetical progression, we have only to make the original portion in accordance with this law, and then to arrange a second feed (the ordinary feed of the milling machine) by which the bed with all that is fast to it may be uniformly moved a certain distance in the direction of the axis of the screw-blank for every revolution of the latter.

By reversing this latter feed, on passing the middle of the blank, a screw-cam can be cut, which shall give an oscillation of its nut, and by suitable variations of this feed many other forms can be cut.

INDICATOR ATTACHMENT FOR HIGH SPEEDS. By J. BURKITT WEBB, of Cornell University, Ithaca, N. Y.

[ABSTRACT.]

THE steam-engine indicator, in the form given it by Watt, is essentially a low-speed instrument, and momentum and friction will ever be present to vitiate its indications for the high speeds coming into use. A new principle must, therefore, be adopted, which shall make the action of the instrument independent of the speed of the engine.

This new principle has been embodied in an instrument quite different from the ordinary indicator, and also in an attachment to be applied to the latter; as the former contains, besides the essential and radical improvements, several conveniences, the shorter and simpler description of the latter will be given.

The new principle may be thus explained:—in the ordinary instrument we have a positive, horizontal, oscillating motion of the paper, coinciding with that of the piston, and a non-positive, or free, vertical motion of the pencil dependent on the steam pressure in the cylinder; in the new form of instrument the first motion is the same, but it is made more certainly positive; for the second motion, however, is substituted a positive vertical-feed of the pencil, dependent on the first motion. These two motions form on the paper what we have called the "blank-diagram," which will, from the manner of its formation, and in the figure, be seen to consist of a parallelogram of zigzag "pressure lines," *k*, *l*, *m*, etc., such as would be made by oscillating (instead of continuously revolving) a chronograph cylinder. This arrangement evidently eliminates

the troublesome effects of momentum, and the blank-diagram is perfectly accurate.

The volume and pressure curve is formed upon this blank-diagram by a third or "registering motion," which introduces what we may call a "chronograph" jog in the pressure-lines as they cross this curve. This motion is dependent, of course, on the steam-pressure, and being exceedingly small it allows of the employment of a diaphragm, instead of the ordinary piston and cylinder, thus eliminating the injurious effects of friction and making it possible to get rid of the cock and of most of the waste room between the indicator and the steam-engine cylinder.

We think it no disadvantage that this new form of instrument requires a number of strokes of the engine for the completion of a diagram; in fact, in the ordinary instrument no attempt is made to confine its action to a single stroke.

EXPERIMENTS TO DETERMINE THE STRENGTH OF CYLINDERS WITH DOMES ATTACHED. By SAMUEL MARSDEN, of Saint Louis, Mo.

[ABSTRACT.]

THESE cylinders were made in imitation of a locomotive boiler with a dome, and the object of the experiments was to ascertain one of the ways in which such a boiler gives way under pressure. Five cylinders were tested, they were about four inches in diameter, and their domes varied in size. They were of lead and the domes were soldered to them; this material was chosen on account of its uniformity. The holes through the walls of the cylinders, which connected their interiors with those of the domes were concentric with the latter, but not in all cases of the same diameter, and this proved to be the point of main importance. When this hole was made as large as the dome the cylinder and its dome burst by cracking in a plane passing through the axis of both, the crack starting on both sides of the dome, at the angle between dome and cylinder; but when this hole was made small the cylinder burst on the side opposite to the dome, much the same as an ordinary cylinder would have done.

AËRIAL NAVIGATION. By JOS. DE L'ETOILE, of Ottawa, Canada

[ABSTRACT.]

THIS paper was a review of the subjects of atmospheric currents, electricity and gases with a view to practical aërial navigation by means of balloons: to take from the air the means to navigate through the air, and thus do away altogether with the ballast and all such dead weight material which has been and still is the real cause of the little or no progress at all, having been made with regard to the guidance of balloons. The balloon should take the form of a fish and be provided with a propeller, a rudder, an air compartment, gas and air pumps or exhausters, electric battery, electric meter, and two safety valves. The balloon should be made to rise slowly and to descend with ease to the earth. All danger from electric flashes coming in contact with the escaping gas through the envelope of the balloon has been obviated by using material which prevents such escape. The above plan was tried, in the year 1869, and it worked satisfactorily. When the ensemble of this new aërial vessel shall be put to a test, it will, no doubt, prove as important to the aëronaut as the steamboat and locomotive are to the engineer.

Atmospheric currents vary at different altitudes, and by ascending through high atmospheric regions, aëronauts cannot fail to meet with favorable winds. Observations for scientific purposes, taken during ascensions in balloons, have hitherto been but imperfectly made. Globated balloons usually rise in an oblique direction and under the combined influence of the direction of the wind. The change of temperature in the atmosphere in a vertical ascension could not naturally be the same as that met with in an oblique direction and sometimes at twenty or fifty miles horizontally from the starting point, as the atmosphère above New York could not be the same as that met with above Boston at the same altitudes, at the same hour and minute, and under the same influence of aërial current. For want of practical steerable balloons, the regions of the two poles of our globe are yet unknown to geographers. The different atmospheric currents at certain altitudes, and at different places in a given hour and minute, are still a puzzle to meteorologists. The capacity of telescope glasses

has not as yet been ascertained at different altitudes and under different atmospheric conditions.

Astronomers seem to have overlooked the fact that a glass cannot be as true to the eye when looked through on the surface of the earth, as it would if above atmospheric electric currents. European and American governments have, without forethought, sent brave men, on board of ships which cost millions of dollars, to the arctic regions, and what has been the result? Of course, great loss of lives and the sinking in the deep of all such valuable property. Let any one beg government aid for the minimum sum of say, four thousand dollars, which would be sufficient to meet the expenses of building a steerable aërial ship; what would probably be the answer?

THE FUTURE OF THE BALLOON AS A PRACTICAL MEANS OF AËRIAL TRAVEL. By W. H. LYNCH, of Danville, P. Q., Canada.

[ABSTRACT.]

The two systems of aërial navigation, or rather the system of actual aërial travel by balloon and the theory of aërial navigation by utilizing the flying principle, are quite opposite.

In the flying machine the propeller or wings are required to act upon the air at once to lift the dead weight of the machine, and to propel it or direct its course. The balloon, on the contrary, is itself a lifting power, lifting itself and carrying foreign weight that may be a force to direct or propel it.

We do not forget that the self-lifting machine is in one respect at a disadvantage with the inert or dead-weight machine. To be lighter than air means to be bulky, and to offer in motion greater resistance to the air, while what is heavier than air is more compact, and its motion more easily caused or directed. This one advantage of the flying-machine over the balloon counts, however, but little so long as it cannot both lift itself and carry the little extra weight of the force required to propel it. The balloon will carry no mean weight besides its own. It will carry, besides possible passenger or freight weight, the extra weight of a force that may direct or even propel the whole.

In answer to the question from which principle may the best results be expected, we may note that the dead-weight principle has given no promise that we shall get from it, alone at least, any practical result. The balloon principle, on the other hand, has already given us so much that no one may positively negative the opinion of Glaisher that it is the "first principle of some aërial machine that remains to be suggested." The peculiar disadvantage of the balloon, that of bulkiness, may be minimized, and the flying principle itself may doubtless be utilized to its full value in its application to the direction, and perhaps actual propulsion of the balloon.

Is the general ridicule of efforts to utilize the balloon a reason to believe that it has no practical future? We think not. Wonderful has been the progress in means of travel, on land and water. Our cars and boats are palaces for comfort and models of strength. Steam-engines are of giant power and perfectly under arm control. Road-beds are built with wonderful engineering skill, through rock or over rivers. Water ways are charted and known, to be followed easily by night or day. Yet everyday's newspaper has its record of accidents and disasters. The time is doubtless approaching when we shall travel with an approach to absolute security and safety. Still greater achievements of science and ingenuity will make even swifter travel more safe, as well as more comfortable. Explosions on river boats once so common are even now quite rare. Broken rails which were an everyday occurrence when rails were iron, are hardly known to-day when rails are steel. Collisions will be less frequent when our roads shall have double tracks. Running off the track may degenerate to quite a safe excitement by the possible common use of inexpensive girders, which are suggested by those already used on the New York elevated roads. The risk of wreckings of trains will be minimized by improvements in switches, in our system of crossings, etc. All this shows that even in surface travel the great progress already made leaves much to be desired, and the accomplishment of much to be expected. In aërial travel there has been almost no progress, and instead of hopeful expectation, the question is raised whether attainment in this direction is possible. We have only to take the history of ballooning, showing, as it does, the unscientific character of most efforts made to solve the problem of aërial travel, to negative all such assumption, and to encourage the expectation that the future has great results in store in the line of progress.

What the compass was to the mariner, making possible the discovery of a continent; what the electric telegraph was to the railway, making possible the running of trains on single tracks, modern science and invention may be to balloons. The batteries for storing electric energy, the knowledge being gained of the properties of gas, the remarkable and great advance in meteorological science, are all particular factors in the solution of the problem. The aëronaut of the future, like the driver of the steam-engine of to-day, must have his power at his hand, his gauges under his eye, and his route, with its advantages and dangers, in his mind. He will not depend upon the unscientific helps of spare ballast and gas to be utilized only by an uneconomic waste, or even upon the long ropes that dangle over the chimneys of houses or toy with the crests of icebergs. With the application of his abundant and ever-ready power to a suitable propeller he will in some measure direct his course at will. With a knowledge of air currents and analogous matters, he will move up or down to suit his need, and either seek the help of wind or evade its power.

When the problem shall be solved it will doubtless be through the acceptance and improvement of the balloon principle. When the balloon, that has already proved itself a carrying power of no mean value, has had turned upon it all the helps that science can give to utilize its possibilities, the difficulties of aërial navigation may vanish.

That the problem will be solved, men sanguine of any great future improvements and changes will not question. The fact of no advance for a century does not, to the minds of thinking men, annihilate the possibilities in the full application to any problem of modern science, ingenuity and skill.

TITLES OF OTHER PAPERS READ IN SECTION D.

THE PERMEABILITY OF THE LININGS OF HOUSE-WALLS TO AIR. By Thomas R. Baker, of Millersville, Pa.

ST. LAWRENCE BRIDGE AND MANUFACTURING SCHEME. By F. Foster Bateman, of Montreal, Canada.

SECTION E.

GEOLOGY AND GEOGRAPHY.

PAPERS READ.

THE GLACIAL FLOOD OF THE CONNECTICUT RIVER VALLEY. By C. H. HITCHCOCK, of Hanover, N. H.

[ABSTRACT.]

IN 1878, Mr. Warren Upham's report upon the "Modified Drift of New Hampshire" appeared in print. He sketched in full, with maps and sections, the terraces and the kame of the Connecticut valley in New Hampshire and Vermont.

The phenomena were explained by supposing the various deposits to have been accumulated by the flood, attendant upon the melting of the Quaternary ice-sheet. The kame was formed first, and then the higher terraces, which constituted part of an enormous flood plain. When the ice was gone the waters diminished in bulk and the lower terraces were formed. Two kinds of terraces were specially noted: first, the normal highest lateral terraces deposited by the Connecticut flood; second, the deltas of the tributaries to the main stream — the latter being very commonly higher than the others.

Three years later, Prof. J. D. Dana went over the same ground with Mr. Upham's report in hand, in order to procure the additional facts required to estimate the size and velocity of the ancient river. With spirit levels he verified the accuracy of the report, and estimated what the velocity of the current must have been upon the basis of his estimate of the width and depth of the flood. For the country between Massachusetts and Wells river the mean width was thought to average 4,000 feet, and the mean depth 140 feet. The formula for the calculation of the velocity was that given by Humphreys and Abbot.

The mean velocity of the flood from Haverhill, N. H., to Middletown, Ct., must have been over twelve miles per hour; allowing for retardations of various kinds, the rate may be assumed to be ten miles per hour,— this velocity must have been far too great. A study of the materials actually transported indicates an actual velocity of three to four miles per hour. In order to bring the calculated and observed velocities nearer together, it is suggested that the land pitched to the south less than now; for marine fossils occur at increasingly greater elevations from south to north, and consequently the ocean must have been higher in the north. At Montreal it stood 520 feet higher than now; and by proportioning the height to the distance, the Connecticut river at Haverhill must have flowed over a floor 120 feet nearer the ocean level than it is now. By using these figures Professor Dana estimated that when the flood was at its maximum the rate of velocity must have been as much as eight miles per hour. This he allows is much too great, but he does not state how the velocity can have been reduced to the proper limit.[1]

In the statement of the facts, exceptions are taken to Upham's doctrine concerning the method of determining the height of the water. Attention is called to these discrepant views and to the presumption of the correctness of those advanced by Upham, because they reduce the magnitude of the flood and consequently the velocity of the stream, and thus better harmonize our theories with the facts. Dana bases his estimates for the depth of the flood upon the present altitudes of the deltas of the tributary streams. Upham claims that the height of the tributary deltas should not be taken as the measure for the main stream because it is in excess of the truth. The height of the usual lateral terrace between the tributaries is regarded as the more satisfactory measuring rod. This deposit was confessedly made by the main stream.

The following table, as compiled from the sources cited above, will show the differences required for the height of the water according to the diverse theories. Only that part of the river's course between Barnet and Massachusetts is considered.

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[1] See Amer. Jour. Sci., III, Vol. XXIII, Feb., March, May, 1882.

	River above Ocean.	Highest Normal Terrace above river.	H. N. T. above Ocean.	Deltas above river (Upham).	Deltas above Ocean (Upham).	Deltas above river (Dana).	Deltas above Ocean (Dana).	Deltas above H. N. T.	
Barnet...........	452	166	618	223	675	225	677	57 U.	59 D.
Wells river......	407	123	530	253	660	253	660	130 both	
Haverhill.......	392	83	475	238	630	263	655	155 U.	180 D.
Piermont........	387	78	465	113	500	258	645	35 "	180 "
Orford...........	383	52	435	177	560	247	630	125 "	195 "
E. Thetford......	378	167	545	157	535	232	610	10 below	65 "
Hanover.........	373	172	545	191	564	207	580	19 U.	35 "
W. R. Junction..	333	177	510	217	550	242	575	40 "	65 "
N. Hartland.....	323	177	500	227	550	242	565	50 "	65 "
Hartland........	311	189	500	309	620	227	550	120 "	50 "
Windsor........	304	196	500	236	540	216	520	40 "	20 "
N. Charlestown..	293	157	450	182	475	210	500	25 "	50 "
S. Charlestown..	284	166	450	216	500	206	490	50 "	40 "
Bellows Falls....	283-34	191-142	425	203-252	486	207	480	61 "	55 "
Walpole.........	226	169	395	239	465	224	450	70 "	53 "
Putney..........	215	135	350	150	365	210	425	15 "	75 "
Brattleboro......	200	141	341	290	490	210	410	149 "	69 "
Northfield, Mass.	177	128	305	213	390	213	390	85 both	

This table shows us first, that the terraces slope downwards at about the same angle with the river's bed. A lake or ocean is therefore not necessary for their formation, and for similar reasons the tributary deltas may represent floods greater than that of the main stream. A study of the strata of the deltas shows that they slope rapidly as if deposited upon a considerable pitch towards the main stream. Secondly, the tops of the highest normal terraces do not slope continuously and uniformly according to the figures in the second and third columns. This would seem to indicate a variation in the magnitude of the flood or of obstructions. It is most marked between Thetford and Wells river, where the height is less than half that of the same terrace above and below. It is possible that the greater altitudes of the terraces below Thetford may have been occasioned by an inequality in the bottom of the river. The kame gravel beginning at Thetford may have constituted an obstruction to the free flow of the river. The gradual rise northwards of the deltas in column seven does not agree with the inequalities of the same as stated in column five. Thirdly, the last column shows the differences in the height of the deltas and normal terraces given by both authors. If we are not required to accept the deltas as a measuring rod, we shall largely reduce the depth of the stream and thus learn why the velocity as calculated by Dana is far too great. If the value of a theory consists in its ability to explain phenomena, then certainly the view that the lateral normal terraces indicate the height of the glacial flood is to be preferred to that which would make the tributary deltas the index.

The other point of difference between these authors concerns the existence of a kame between Thetford and Windsor. The one describes an arched ridge of coarse gravel and sand following the lowest level of the valley and dipping beneath all the terraces. The other insists that this gravel is a series of beds capping the higher terraces.

The best locality to settle this question lies about a quarter of a mile from my residence. I have passed it hundreds of times, and always observe the relation of the two deposits in passing, and remark how the kame-gravel dips beneath the terrace loam. I have examined this and several other similar sections described by Professor Dana — as upon both sides of White river — and with this gentleman's printed paper in hand, so as not to misunderstand his views, and find that Mr. Upham's sections and descriptions are entirely correct.

Dana accounts for the presence of the very coarse gravel in the middle of the valley, by supposing it to have been brought down by tributaries. Thus the coarse gravels at White River Junction are thought to have been brought down White river and dropped where the tributary joined the Connecticut. If so, then the gravel should be composed of pebbles corresponding in character with the rocks in Vermont; but the most of them consist of material derived from the north. As the glacial current came from the northwest, these fragments of northerly origin could not be distributed much to the west of the Connecticut. Scarcely any are to be found west of the kame, thus proving the gravel to have been deposited by the Connecticut flood and not by the White river. Furthermore, this tributary enters the main stream through a gorge-like channel pointing south of east. Its sediment could not be distributed to the north up and against the Connecticut current, but this gravel bed is continuous for *one-and-a-half miles up the Connecticut* above the mouth of White river. Hence it could not have been brought by the White river.

Accepting, therefore, the existing of a kame in the Connecticut, we have still further reasons for reducing the size of the glacial flood, and thus of harmonizing our theories with observed facts.

Mr. Upham's careful studies of the modified drift of the Merrimack and the other New Hampshire rivers do not indicate such differences between the normal and delta terraces as have been pointed out in the Connecticut. Calculations of the volume and velocities of the glacial Merrimack, Contoocook and Saco rivers would be based upon the altitudes of their highest normal lateral terraces.

CONTRIBUTION TO SEISMOLOGY. By RICHARD OWEN, of New Harmony, Indiana.

[ABSTRACT.]

THE object of the paper is to point out the relation between solar and terrestrial dynamics, especially during the shorter secular changes: to show that when there is even a slight increase or diminution of motive power in the sun, there is, both as regards time and space, either directly or indirectly, a corresponding increase or diminution of the earth's kinetic energy, bearing on inorganic and also on organic matter.

DEFINITIONS, EXPLANATIONS AND ABBREVIATIONS.

The words force, power, energy are used as synonyms, denoting the inherent attribute of bodies, which gives rise to the various forms of motion, whether molecular or mass. The adjectives motive, kinetic, dynamic are also used as interchangeable, without essentially varying the meaning. "Seismic" is employed in its most extended sense; embracing the phenomena of earthquakes, volcanic outbursts — whatever the material erupted — geysers, hot springs, gaseous emanations from solfataras and fissures, as well as land elevations and depressions. Great circles are abbreviated to G. C., and are considered as belts having a surface width about fifty miles each side of the theoretical line. The five equidistant G. C.'s described, in the Boston paper[1] for American Association for the Advancement of Science, as east continental trends, and which are secondaries to the ecliptic at our summer solstice, will be designated, beginning at Macassar's Straits, as A, B, C, D, E; while their mates, defining chiefly western trends, and being secondaries to the ecliptic at our winter solstice, will be called A′, B′, C′, D′, E′. The two terminators for noon and midnight at the meridian of land centre, described on p. 445 of said paper, are abbreviated as F and F′, while the meridian of Mount Rosa (land centre, in about 8° E. long.) may be called P. M., and its prime vertical, whose seismic importance will be shown, is designated as P. V.

In an abstract, necessarily, only a few illustrative examples can be given, thereby detracting much from the materials for a general deduction, and lessening the probability of its acceptance.

I. *Distribution in space of kinetic energy, whether molecular or mass-motive.*—Let us trace (best with charcoal) on any Mercator's map, which exhibits the volcanoes and earthquake regions, all the G. C.'s given in the above paper on "Land-forming," as well as the P. M. and P. V. To these add the concentric circles for each continent, as described in same paper, as well as for Mt. Rosa, the centre of the land hemisphere.

Additional, and not of least import, are the radial fissures (resulting probably from cooling and contraction) diverging from these land centres. The most important usually form with each other, angles[2] at the focal centre of $\frac{360}{3}^\circ = 120^\circ$; while others are $\frac{360}{6}^\circ = 60^\circ$, or sometimes even $\frac{360}{12}^\circ = 30^\circ$.

Having traced these enumerated circles and fissures, we shall find that we have now covered, with our charcoal belts, almost every portion marked in the maps as seismic, except Jan Mayen in the north, and Mt. Erebus in the south. Furthermore the paper shows that the G. C.'s follow the mountain ranges and the junctions between geological formations, besides several other points, in illustration of which a few examples are subjoined under their appropriate headings.

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[1] Boston Proc. A. A. A. S., pp. 437–446.

[2] The discussion of the above law being found too extensive, as at first designed, for a foot-note, and being considered to apply with equal force in plant development, is made the subject of a short, separate paper.

1. *These great circles accord with mountain ranges.*—Witness the Appalachians and western Ghauts parallel to D; the Rocky Mountains, Great Khingin and Stanovoi to F; the Sierra Nevada to D′; the mountains of Greece, Turkey in Europe, Bohemia, etc., to B′.

2. *These G. C.'s mark geological trends between two formations.* D passes along eruptive rocks, separating Palæozoic from later formations in the United States; B′ in Greece and Turkey runs between Cretaceous and Tertiary, in Bohemia between Palæozoic and newer rocks; then again going N. W. passes between Mesozoic and Cenozoic.

3. *The intersections of G. C.'s are highly seismic.*—Santorin is at the intersection of B′ and E′; Caracas at that of A and P. V.; Sumatra and Quito at the junction of three G. C.'s; Iceland of two; San Salvador of two; Lisbon of two; the extinct Eifel volcanoes of three, viz., P. M., B′ and C; Japan, at its V. Fusi-yama, of two; the Cutch, with its Ullah Bund, of two, viz., D and P. V.; the submarine Atlantic volcanic region of two, C and C′.

4. *They mark linear volcanoes, rising and sinking areas, also geysers, hot springs, etc.*—The volcanoes of Kamtchatka, the Kuriles, Japan, etc., are on A; and those of South and North America chiefly on F. The Greenland subsidence is on B′ and E; east end of Candia sinking is on B′; the west end on E′ is rising. The area in north of Scandinavia, now rising, is traversed by D, F′ and A′; the south portion, sinking, is crossed by C. The great subsidence in the Pacific seems mainly due to E and E′; also to C and C′; while lake depression of central North America[3] is due especially to A. The oscillations in Belgium,[4] as well as those in the Danish Island of Möen[5] are on belt C. The geysers of Iceland are found on D and B′; those of our "National Park" on B′; the fires of

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[3] Dana's Man. of Geol., p. 391.

[4] Encyclop. Brit. (9th Edit.), Vol. III, p. 444.

[5] Lyell's Antiq., p. 393.

Baku are near P. V.; the gaseous emanations (now extinct) of Delphi and Livadia on B′; the dog-grotto carbonic di-oxide, near Naples, is on the fissure from Rosa, through Stromboli to Messina.

5. *They connect places of synchronous, earthquake tremblings.*—Quito and Ochotzk[6] are on F; Malta and Scotland on the direct fissure connecting those two places through Rosa; Milan and Tornea on fissure from Rosa to Baltic; Messina and Upsala connect at Rosa by two fissures forming an angle of 120°; yet perturbations at Berlin did not extend to Paris[7]. Scandinavia is connected with Iceland by D, but not with Great Britain.[8]

6. *The areas of these seismic belts seem favorable to metamorphosis and crystallization of rocks and minerals.*—In the Grecian Archipelago, along B′ and E′, the limestones appear to have been all converted into marble — it is so abundant that even the curbstones at Athens are marble — and along D we observe similar effects in North Carolina, Virginia, Maryland, New York, and Vermont; and the radial line of force from Rosa, skirting between the west coast of Italy and the left flank of the Appenines, through erupted rocks, aided to give Carrara and other cities, near that line, their marble, they being on the radial belt from land centre to Calabria, Messina and Etna. The belt A passes through the diamond region of Brazil; E runs close to the African diamond district, while D is near the Golconda and Ceylon areas, noted for gems.

7. *The magnetic force exhibited in the earth's crust to the east of Kew and Lisbon*, north of Vienna and northwest of Petersburg, described[9] by Prof. Grylls Adams, is fully explained by following great circle C; also *Low Barometer*, possibly due to tension *prevailing sometimes along belts of G. C.'s, seems to predispose those areas to linear storms.* No. VII, of the Signal Office records,[10] followed the track of D; the hurricanes of our coast, originating near Florida, run parallel to D; and the typhoons of Japanese waters are parallel to F and A.

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[6] Mallet's Cat. of Earthq., 5th Rept., pp. 55-6; also Plate X *bis*.

[7] Cosmos, Vol. IV, p. 143.

[8] Mallet's Cat. of Earthq., 4th Rept., p. 3.

[9] Lecture delivered at Royal Institution, June 3, 1881 (Nature).

[10] Houston's Phys. Geog., p. 140.

8. *At least three of the foci of magnetic intensity are at the junction of two G. C's.*—The sub-Australian focus is at the intersection of D with E; that of Siberia is near where B and E′ meet; that in the South Pacific is at the junction of A′ with E′.

9. *These belts traverse chiefly fertile tracts of land, and often pass through regions of extinct gigantic animals.*—The densest forests of South and North America are on A and D; while the giant Sequoias of California are on D′, not far from its junction with B′. The extensive forests of Bohemia, Thuringia, etc., are also on B′. Belt D′ covers the Patagonian area of Glyptodon, etc.; P. V. and E that of the New Zealand Dinornis, etc., A′ and C′ of the Australian Diprotodon and the like; A and D and B′ of the gigantic remains in the United States; B and E′ those of N. Siberia; F′ those of the sub-Himalayas.

10. *The chief civilization appears often to follow continental trends marked by G. C's.; and in Europe also the P. M.*—The civilization of eastern Asia is covered by A and F; that of Africa mainly by B and B′; in South America F marks the older civilization of the Incas, the Yucatan architecture, then passes near the Aztec, Toltec, and perhaps Arizonian developments; while the mound-builders spread along A, and later Anglo-Americans along D; latest on belt D′. In Europe the palæolithic, and neolithic evidences in Scandinavia and Denmark, the *find* at Neanderthal, the lake dwellings in Switzerland, and the remains in the caves of Engis, Mentone, etc., are all on or near the P. M.

11. *National and individual development seem greatest on some of these belts.*—Grecian and Macedonian history followed B′ into Austria and Germany; Rome spread her power along the fissure which traverses Sicily, Rome and Rosa (Helvetia), through Gaul to Britain; the civilization of Spain, France, Belgium, Holland, Prussia and Denmark is found on C; while F′ traverses Scandinavia and western Russia to near Gangetic Hindostan (Benares, etc.), as does the P. V., the Caucasus and Persia.

Should this law be found to prevail, as it has thus far, in numerous individualized applications, it would point to an additional important factor in the influences of environment; and when we find the belt of B′ passing through *eastern* Greece, the law may aid us to understand (especially in connection with the distribution of kinetic energy in *time*) why the inhabitants of Attica were the ablest of the Greek sub-races, and why, between the years 530 and 430 B. C., Attica should have given to the world fourteen of the most illustrious men in history out of about 135,000 males, counting three generations to a century.[11]

II. DISTRIBUTION OF SEISMIC PHENOMENA AS REGARDS TIME.

The tabular synopsis, exhibited with the unabbreviated paper, shows that seismic phenomena group themselves around the sun-spot periods; for instance, Etna was quiescent from 1694 to 1755; the years 1698 and 1753 being two of the consecutive fifty-five year minimum periods. The synopsis further demonstrates that, by computations from Mallet's "Catalogue," earthquakes were much more numerous, relatively to the whole number for a given period, in middle Europe about the year 1663 (when there was no declination of the needle in middle Europe), than for the same number of years before and after 1814 (the period of greatest westing of the needle in middle Europe), apparently indicating that maximum seismic activity gradually follows the changes in the magnetic meridian of a continent.

The tabular view shows also that sun-spot periods coincide with meteorological anomalies (such as a severe winter followed or preceded by a hot summer). The geothermal changes, in middle latitudes, perhaps following the atmospheric maxima and minima after six months,[12] may, by differentiated heat (as when at the heated junction of the Bismuth-Copper Thermo-multiplier, the Bi., unable to transmit heat as rapidly as Cu., converts a portion into electricity), produce magnetic anomalies in those years of thermal anomaly, as indicated by the magnetic needle standing still, or even retrograding for a year or more, instead of pursuing its regular secular march.

Although the whole magnetic system is undergoing secular changes, the mean plane of oscillation seems to coincide with the meridian of land-centre, and to pass through the terrestrial equator between St. Thomas Island and the coast of Guinea. There

[11] Lyell's Antiquities, p. 547.

[12] Cosmos (Sabine's Trans.), Vol. IV, p. 38.

the equator of dip intersects the equator of rotation, and the equator of intensity crosses this same meridian of land-centre farther south about 10°–15° ; both at present running somewhat parallel to our winter ecliptic, while in perhaps three hundred and thirty years (as estimated below) these equators will run nearly parallel to that of our summer solstice. Additional evidence of the land-centre meridian being also the mean central plane for the magnetic system is found in the fact that (on using this meridian as the dividing plane), the sum of the magnetic intensities in the eastern hemisphere is almost exactly equal to the sum of the intensities in the western ;[13] also that the measurements from the region of least magnetic intensity south of St. Thomas to at least three of the foci of maximum intensity, as well as to Boothia Felix, are as nearly equal as are their relative intensities. Again the equator of dip near St. Thomas does not differ in position much from the mean equator of heat (mean isothermal of 80° F.) and the northern foci of greatest intensity do not vary far from being the poles of maximum cold in North America and Siberia. The close conformity of isocheimal lines to isoclinics is also pointed out, and some explanation is offered why in our Middle States and Alaska the declination E is diminishing, while in California and some adjoining regions it is increasing.

Eminent astronomical authority admits not only the above discussed variation in the sun's activity, but also its bearing on the earth, thus :[14] 1. "The equatorial regions of the sun have at least $\frac{1}{16}$ higher temperature than the portion beyond the thirtieth parallel of lat. 2. The temperature of the sun is a trifle higher in the northern than in the southern hemisphere. 3. The sun-spots emit less heat and light than the other regions." Another astronomer[15] says : "In 1637 sun-spots were so numerous that the heat and brilliancy of the sun were perceptibly diminished ;" and again :[16] "Sun spot periods coincide . . . with variations of magnetic force and the periodicity of auroræ boreales. The periodicity of the spots indicates, as Father Secchi remarks, a periodicity in the solar activity . . communicated to the earth."

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[13] Polyt. Review for Dec. 14, 1878, p. 247.

[14] Father Secchi on the sun, p. 133.

[15] Rambosson's Astronomy, p. 70.

[16] *Ibid*, p. 75.

Any gradual change in the sun's power, which would modify the thermo-electrical influence, might directly or indirectly produce the secular change in the needle from greatest easting to greatest westing and *vice versa*. As to the time in which this takes place, although we have not yet sufficient data for any certain calculations,[17] some coincidences seem to point to a period of about three hundred and thirty years, thus:

30 × 11, the minor sun-spot period = 330 years.
6 × 55, " major " " = 330 "
2 × 164, twice the period of Neptune = 328 "
4 × 84, four times the period of Uranus = 336 "

Possibly these combined influences may have some bearing in connection with other forces (perihelion, perigee, etc.) in effecting an electrical and magnetic predominance, thus, as remarked, producing greatest westing when the maximum force prevails in our winter solstice, and greatest easting when the most energy exists in our summer solstice.

By tentative calculation, taking the ratio of the secular period at Paris from 1663 to 1814 (the arc of variation from Boothia Felix to N. Siberia forming at St. Thomas Island an angle of about 23°–24°, and at Rosa fully double that arc, while at Paris it is greater, but unequal for easting and westing), the secular period of magnetic oscillation for middle Europe might be about as above, nearly . . = 330 years.

An evident relation is also traced and exhibited in the synopsis between those centuries marked by Mallet as seismic (first, fifth, ninth, twelfth, and eighteenth), showing them also to be historically of great importance; while, on the contrary, few stirring events occurred in those centuries recorded by Mallet as non-seismic, viz., the first and second B. C., as well as the third, seventh, tenth and fourteenth centuries of our era.

This suggestive thought is worthy of further investigation, in order to trace the influence of environment on anthropographic and ethnographic development.

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[17] Report of 1881 by Chas. A. Schott, Esq., Assistant U. S. Coast Survey.

LAW OF FRACTURE, OR FISSURING, APPLIED TO INORGANIC AND ORGANIC MATTER. By RICHARD OWEN, of New Harmony, Indiana.

[ABSTRACT.]

As the unabbreviated paper applies the law to inorganic and also to organic bodies, this abstract commences with the former.

I. *Inorganic.* If pellets of clay, or other plastic spheroids, be dropped into a bowl, the lateral pressure compels them to assume, and present to the eye, an approximately hexagonal form.[1] This is the polygon of greatest sides, susceptible of lateral junction with similar polygons, so as to leave no intervening spaces. We find, on examining this junction of spheroidal bodies, compressed while plastic, that the lines of contact present, where any three of these polygons come together, three fissures or lines of fracture, diverging at angles of $\frac{360}{3}^\circ = 120^\circ$. If, however, these hexagonal fissures again break up into equilateral triangles, by the prolongation of these sutural chord fissures into the adjoining hexagons, then these six prolonged fissures meet at a central focus.

Further, if we break a pane of glass, or a cake of cap cement, by a smart blow of a sharp instrument, placed centrally on the pane or cake, we find that the radiating cracks or fissures, thereby produced, present angular fragments having, at the focal point a measure of 60° or 30° or occasionally of 120°. This law can also be traced in the cooling and shrinking of basalt: thus giving rise to a set of vertical fissures diverging 120°, as in the separation between the columns of the Giant's Causeway. The same tendency can be traced in mud when drying, although often sun and wind, acting chiefly in two directions, may give rise to right square prisms. Fissures resulting from an upward force have radial earth fractures, as after the earthquake in Calabria.[2] If the columns separate also horizontally, then we have such exhibitions as at Bertrich Baden Cheese Grotto.[3] Should the superincumbent layers be so distributed, as in bee cells, that the unit of an upper layer is directly over the triangular fracture between three contiguous units below, and in its turn be pressed upon by three units above, each such unit would in the upper layers be enclosed by nine rhombic planes, and in the intermediate layers would assume the dodecahedral form.

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[1] Key to Geology of the Globe, p. 85.

[2] Lyell's Prin., II Vol. p. 124; also pp. 127 and 128.

[3] Lyell's Elem. (ed. 1856) p. 485.

In the separation, however, of contracting bodies, as well as in the compression of adjacent plastic spheroidal bodies, if some cells or units are smaller than others, or if the pressure is unequal, or the ingredients not homogeneous, the result may present a pentagonal cross-section. The hexagonal and radial system of fissuring has apparently nothing in common with the so-called joints, well described by Prof. G. K. Gilbert,[4] which are often at right angles to the planes of stratification, and parallel to each other; nor with slaty cleavage, probably due to such compression as forces all the planes of the fragments (*e. g.*, plates of mica in mica slate) into the same plane with the compressing force.

Although, on our globe, the five equidistant great circles of least resistance (or greater seismic force from interior) and their mates, in consequence of being secondaries to the ecliptic at the solstices, would point to solar influence as at least contributing to give them importance, yet the tendency of the solid materials in the earth's crust, particularly at continental centres, to separate by definite radial fractures, when acted on by erupting force from beneath, exerted at various geological periods, evidently follows the same law of fissuring stated above: the fissural lines being sometimes marked by a chain of lakes (as from the centre of North America running N. N. W.); or an extensive gulf (as that of Guinea or Genoa); or occasionally by interrupted elevations, islands and mountains, such as the line from the centre of Africa, near St. Thomas, running N. N. E. through Prince's Island, Fernando Po to the Cameroons and other mountains, as well as S. S. W. to St. Helena; or the 120° divergence in the Carpathians from the Tatra centre of Europe; or the Altai range running S. W. from the centre of Asia, while another range, diverging from it 120°, trends S. E. toward Lake Baikal. By bringing Mt. Rosa to the zenith, we can readily, on extending a string thence successively to every 30° around the globe, trace twelve radii, marking usually depressions, some radii more important than others because passing through highly seismic regions, thus:

1. Calling the meridian of Rosa which runs close to Corsica, Sardinia and Tunis, the Prime Meridian (although Greenwich Meridian is eight degrees W.), we have the termination in the reentering angle at the Gulf of Guinea, the true centre of Africa. 2. Diverging thirty degrees E. we have a highly seismic belt

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[4] Am. Jour. of Sc., July, 1882, p. 50.

from Rosa, covering Elba, Rome, Naples and Etna; broad enough to take in also Calabria and Malta, thence to lakes Albert and Victoria Nyanzi, and the west coast of Madagascar. 3. A seismic belt, marking the Adriatic and Santorin, passing between the Dead Sea and Mt. Sinai to Cape Gardafui. 4. The E. extension of the Prime Vertical, through the Black Sea, the S. shore of the Caspian, and the Demavend Volcano, to Cutch and the reëntering angle N. of Madras. 5. The line through Tatra, Europe's centre, to the base of the Urals. 6. Through the Baltic to the White Sea. 7. The N. end of our Prime Meridian running through Behring's Straits, etc. 8. A belt, through the E. coast of England and W. of Scotland to Iceland and Greenland thence through Hudson's Bay to Vancouver's reëntering angle. 9. A belt through Mt. Blanc to the Land's End, England, through the Bay of Fundy and along the Appalachians to the Gulf of Mexico and the Volcano of Colima. 10. The W. end of the Prime Vertical, along the Cantabrian Mountains, through the Azores to Caracas, Quito, etc. 11. A S. W. belt through Cape Vincent, probably between Madeira and Funchal to near the Canary Islands, and through Brazil in a line parallel to the mean trend of its mountain ranges, finally to the disturbed region of Valparaiso. 12. A S. S. W. belt through Minorca, Liberia and Ascension, to the Falkland Islands.

Pieces of pasteboard, cut with angles of 120°, 60° and 30° will be found convenient for measurements, when applied to globe, map or organism. The five great circles, which in the paper on land forming were shown to be 72° apart at the equator, form angles of 60° at their intersection near the polar circles, and enclose successively: 1, Asia; 2, Europe with Africa; 3, S. America; 4, N. America; 5, Oceanica and Australasia.

The above law of fracture seems also to apply to the confluence of rivers: primary tributaries usually forming, with the great continental drainage stream, angles of 60° (as the Missouri and Mississippi) or even 120° when belonging to separate drainage systems (as the Ohio and Mississippi rivers); while the secondary confluents anastomose sometimes at angles of 60° (as Wabash and Ohio); but occasionally at 30°; third-rate tributaries usually enter those of secondary importance at angles of about 30°, as White river at its junction with the Wabash, or Washita with Red River.

The paper next passes to an examination, how far the law applies to plants:

II. *Plants.* If the primordial cell, with its protoplasm and central or subcentral nucleus be in juxtaposition with similar cells, and somewhat crowded, each cell assumes a more or less hexagonal form;[5] and a unicellular plant, such as protococcus, offering an epitome of vegetable life, presents, when young, the division into six equal triangles, whose sides radiate to the central nucleus.[6] In all cellular tissue,[7] as well as in vascular tissue, we can trace more or less of the hexagonal cross-section, and even the tendency to radial fissuring. From this cause, apparently, in the verticillate forms of plant growth, such as that of the pines, we find sometimes three branches thrown out horizontally, each at an angle of 120° from the other, oftener six at equidistant angles of 60°. Whereas, in most of those plants having a spirally ascending growth, we find branches, or petioles, given off, whether opposite or alternate, at angles which form an aperture of 120° with the lower part of the trunk or rachis, and angles of 60° with the ascending stem, and which in the alder cycle (fourth leaf above first) diverge from each other at angles of 120°, when viewed in horizontal section. The axillary bud, whether leafy or floral, emerges from the stem at an angle of 30°. When the hexagonal type prevails, as in many monocotyledonous plants, it seems natural that at those six angles, where the least resistance is offered to dehiscence (the most pressure being near the centre of each hexagonal line of cell-junction) the greatest facility should occur for gemmation; the same applies to the pentagonal arrangement, so prevalent among exogens; also to the four-parted or cruciform type; from whatever cause these modifications may arise. The angular divergence indicated above can, by means of the pasteboard angles, be very readily measured in young timber or among the commonest flowers and weeds: the 30° growth probably predominating in excurrent trees, while the 60° branching is characteristic of the trees with solvent axis; and is found in the great majority of common plants. The divergence of secondary branches, at an angle of 60° from the primary, brings the former approximately parallel to the ascending axis, thus lessening the tendency to breakage of tree limbs, while presenting leaf surface well distributed to air and sunlight.

The coalescence of adjacent side walls of cellular tissue, running as medullary rays, from the central pith to the epidermal layer of the plant, gives rise, by the union of two triangles of cellulose-enclosing parenchyma, to a rhombus, which by lateral pressure and upward growth becomes very acute, and presents, in the bark, those diamond shaped areas found on hickory, black locust, and the like, while the bark markings on some trees of less rapid growth, approach, in their obtuse rhombic form, almost to squares. The crowding of each separate seed against its neighbors, in the large cone, borne by the Italian pine, exemplifies also the hexagonal system; and the tricarpellary dehiscence of the seed vessel in Köhlreuteria (a Maplewort) with its three midvein dissepiments, exhibits three equal compartments for the seeds, which in other pericarps, by duplication, may become hexamerous.

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[5] Ency. Brit. (9th edit.) p. 73. [6] Ibid, p. 81. [7] Gray's Les. in Bot., p. 145.

III. Animals. Among the Actiniæ,[8] we find the hexameral arrangement of primary mesenteries, after the normal type; their six fleshy septa extending from the outer edges of the column wall to the centre, while the secondary or intermediate mesenteries do not reach the centre. Among corals the disposition of the six primary septa, and additional forty-two intermediate septa, especially of the corals usually assigned to the order *Zoantharia sclerodermata*[9] (as well as the crowded corals in some specimens of the extinct *Cyathophyllum rugosum* is strikingly conformable to the hexagonal type, with strongest radii to points of least resistance. The gemmation among some of the Radiates, as when "the Hydroid Zoophytes develop, at a diverticular prolongation of the soft interior substance of the stem,"[10] a Medusa bud, closely resembles plant-budding; and the bud prolongation diverges at an angular aperture of either 60° or 30°.

Among Articulates the *Coronula diadema* is a symmetrical six-sided prism, divided into six triangles not quite equal. The same beautiful provision for strength and susceptibility of contraction and expansion can be traced in the scoriaceous integument of the trunk fish (*Lectophrys camelinus*); and even, although not with such perfect regularity, in the attenuated snout of the paddle fishes (Spatularia).

The law is wonderfully illustrated in spiral univalve shells; depressed spires, as those of Solarium, Patella, Natica and Pyrula, showing 120°; medium spires, as those of Strombus, Ranella and Struthiolaria, forming an angle of 60°; more elongated spires, like

[8] Ency. Brit., Vol. vi, p. 328. [9] Ibid, Vol. iv, p. 333.

[10] Carp. Prin. Comp. Phys., pp. 544-6.

those of Cerethium, Mitra, Terebra and Melania, diverging 30°. Even the nodes, on the spirally ascending whorls (showing greater power or facility of development where they occur) when we place the vertex of the 30° pasteboard angle at the spire-apex and extend the sides to the nodes, evidently form with the axis of the columella usually angles of 30°. In some large Pyrulas, however, on the outer whorl, we find at 30° only an almost obliterated nodule; but the 60° aperture reaches a prominent node.

Oken recognized the vertebral bone as the type of the skeleton: and in the neural arch, as well as the hæmal,[11] we find six bones, while the atlas and pelvis have an evident tendency to the hexagonal form; and in the enamel of the teeth is shown, in the beautifully regular columns with six sides, an arrangement giving great strength for resistance to crushing force. The cell, however differentiated (*e. g.*, in mucous membrane[12]), is still the type of all organisms, and retains more or less of the hexagonal form.

In the animal, as in the plant, the five-parted form, here chiefly designed for prehension and locomotion, often prevails. Thus among radiates, the crinoids partake largely, both in body and extremities, of the pentagonal type, although some of the Cystidea (as Marsupites, Cariocrinus, etc.) are symmetrically hexagonal, even in detail. Of the Mollusks, the highest Dibranchiates exhibit the five tentacles duplicated. Among Articulates, the true insects conform to the hexagonal; but the Decapod Crustaceans exhibit the twice, or bilateral, five-parted type. Vertebrates give us often, among teleost fishes, five finny appendages (two pectoral, two abdominal, one dorsal, and sometimes, however, also one anal, to form the sixth). The highest reptiles (Crocodiles and Alligators) have five toes; among birds, the spur of the Gallinaceæ, added to the four toes, may point to a rudimentary fifth; the highest orders of Mammals, the Carnivora, Monkeys, and Man himself, all conform to the five-parted type of *ultimate* extremities. Indeed, if we include the neck with its sometimes elongated jaws, and the tail often prehensile, as both belonging to the *proximate* appendages, then we have in them also those two prevailing types, 5 and 6.

Again, as in the confluence of rivers, the paper shows the anastomosing of arteries, veins, and even nerves, as very generally

[11] Owen on the Skeleton and Teeth, pp. 27 and 28.

[12] Carpenter's Prin. of Comp. Phys., p. 408.

according with the law of 60° aperture, for important branches, and 30° for those of secondary importance. Occasionally the angle is one of 120°, where the veins, etc., come together from nearly *opposite* directions.

SUMMARY.

The law that homogeneous *inorganic* matter often fissures at angles of 120° seems derived from the fact that, when plastic clay-balls, or other spheroidal masses are acted upon by equal force from all directions inward, they are compressed into hexagonal prisms: each contiguous three separated by angles of 120°; and from the additional fact that again, when homogeneous matter is compelled to separate, by disruptive force acting equally in all directions outward, it fissures into hexagonal prisms. By the reciprocal prolongation (into adjacent cells) of the three fissures described above, each hexagon is liable to be separated by fissures (or in some organisms by partitions) into six equal triangles. In the case of Basalt, any three hexagonal columns, resulting from disruption, would offer at their focal junction, apertures for subsequent outflows: thus adding new matter to the external earth's crust. Such fractures, from whatever cause, may occur at different periods of the earth's history, often giving rise to "faults" of hundreds and even thousands of feet;[13] and may then close either permanently or temporarily. This applies especially to the opening of cracks from below:[14] such as those in Iceland, caused by an earthquake,[15] and reported to be forty miles long. The five equidistant great circles, on our globe (described in another paper), supposed to be belts of least resistance, probably owe their origin, at least in part, to external forces, inasmuch as they are secondaries to the solstitial ecliptic; some are also terminators for important continents during the sun's meridian fervor.

Plants exhibit the same hexagonal tendency in their cells, etc.; and send out their buds at symmetrical apertures of medullary tissue in the stem or branch:[16] the constriction at each end or joint apparently giving greater activity to the side buds.

[13] Fisher's Phys. of Earth's Crust, p. 218.
[14] Ibid, Phys. of Earth's Crust, p. 201.
[15] Ibid, Phys. of Earth's Crust, p. 189.
[16] Carpenter's Comp. Phys., p. 519.

Among *Animals* instinct prescribes the hexagonal form to some insects for their nidus, because it gives most internal space, combined with greatest strength,[17] from least material; while externally every three adjoining cells (and so on throughout the whole structure) unite closely without loss of space. Some of the lower animals give off, from regular apertures as in plants, buds, also organs of prehension and locomotion, etc.; while among higher animals, such as the mammals, the various complicated endo-skeletons, and sometimes exo-skeletons, as in Glyptodon, present modifications in which the whole or a part appears to derive its origin from the typical hexagonal tissues, differentiated by the protoplasm.[18] The rhombic scales of Ganoid fishes, the polygonal plates of the carapace and plastron of the tortoise (*e. g.*, box turtle) as well as similar dermal integuments, often result from the coalescence of two adjoining triangular plates.

The least resistance of compressed polygonal units would seem naturally to be at the angular junctions, because, at each of the six chords, the material, originally comprised in areas enclosed by the two arcs of circles, is now compressed to form a double suture-line, while at the angles there is the entire uncompressed length of the radius: consequently along those six radii (or five, in a pentagon) which enclose somewhat equal triangles, there would seem to be the least resistance to any material emanating from the central focus: whether magma, through fissures; buds where medullary tissue reached the periphery, ripened seeds at the carpellary walls of a dehiscing seed vessel; or protoplasm through ruptured cells in the prolonged connective tissue.

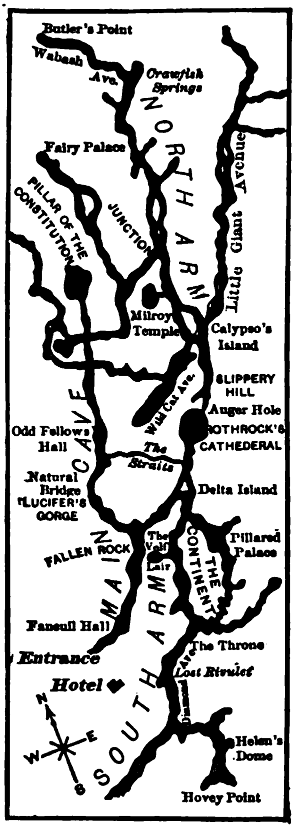
[17] Rev. J. G. Wood's Homes without Hands, pp. 447 *et seq.*

[18] Lecture by Frances E. White, M. D., in Pop. Sc. Mon. for July, 1883, p. 366.

SUBTERRANEAN MAP-MAKING. By HORACE C. HOVEY, of New Haven, Conn.

[ABSTRACT.]

THE difficulties of subterranean map-making are peculiar. The task is very different from that of surveying a bit of meadow or prairie. Were it the more complicated one of mapping a region diversified by hills, lakes and rocky glens, yet there would be the advantage of doing the work by daylight. But an underground survey must be managed while creeping under low arches, following rugged and tortuous paths, skirting the edge of deep pits, fording miry pools, or climbing over rocky fragments piled under lofty domes. And all this diversified realm is wrapped in absolute darkness, but imperfectly scattered by lamps. Imagine a map of the Adirondacks made by torchlight on a series of moonless nights, and it will give some conception of the hindrances to a complete survey of such a place as Mammoth Cave.



MAP OF WYANDOT CAVE.

Add to this the fact that the singular barometric conditions make tests of altitude doubtful; and that the compass is often affected by magnetic disturbances. Consider, also, the natural reluctance of cave owners to have measurements taken that may show the limits of their surface rights to be less than their subterranean claim. No wonder that such a formidable array of obstacles should long have hindered the attainment of even approximately satisfactory results.

In exhibiting, therefore, maps that have been made of three of the most noted American caverns, namely, Mammoth, Wyandot and Luray, it should be understood that little more is claimed for them than that they are helpful diagrams, and that they are the best results thus far secured from subterranean map-making in this country.

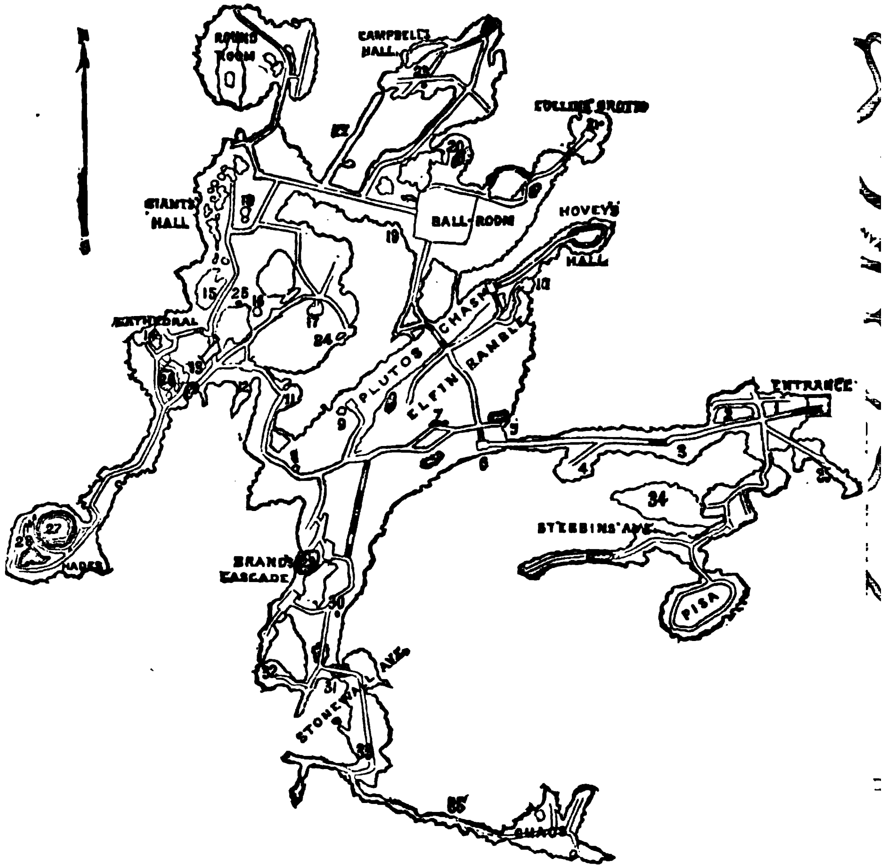
1. *Wyandot Cave*, in Crawford county, Indiana, was purchased by H. P. Rothrock from the U. S. government in 1820; but no attempt was made to survey its avenues till 1851, when the bearings and distances were taken by H. C. Grosvenor. Another survey was made in 1853, by D. L. Talbot, M. D. From these materials, supplemented by further explorations in 1854, I prepared the first complete map of this cave, the original draught of which is still in my possession. Mr. G. I. Langsdale afterward laid down some later discoveries; and the map, thus jointly constructed, was published, in 1860, in Owen's "Geological Reconnoissance of Indiana." My work was carefully revised by myself, in 1878, and published. About the same time Prof. J. Collet made a new map, from notes of his own, which appeared in the Rep. Geol. Sur. Ind., 1878. While differing from each other in minor points, the main features of these two maps agree, thus proving their general accuracy.

2. *Mammoth Cave*, discovered in 1809, is located in Edmondson county, in Kentucky, 85 miles S. S. W. of Louisville. It has been repeatedly mapped, but with remarkable variations. The first rude outline was prepared in 1814, probably by Mr. Gratz, the early owner of this cave, and published in the Med. Repository, vol. xvii, p. 391. It abounds in inaccuracies. A careful survey was made of the portion then known, in the winter of 1834–5, by Edmund F. Lee, C. E., who devoted three months to the task. His map, published by James and Gazley, of Cincinnati, O., is out of print and I have never seen any one who has examined a copy. Next came a map made from notes supplied by Stephen Bishop, the guide, and published by Morton and Griswold, of Louisville, Ky., along with a manual entitled "Rambles in Mammoth Cave." This, too, is out of print, the only copy extant being in my possession. It is said that the plate from which it was printed is destroyed. Dr. C. R. Blackall, of Philadelphia, made a map at a subsequent date, which, however, he was not permitted to publish, being restrained by an injunction. Another, but less accurate, map was prepared to go with Forwood's "Historical and Descriptive Narrative of Mammoth Cave." (Lippincott, 1870).

In the new map, prepared under my supervision, and completed this year (1882); all the portion open to the public, as far in as the Echo river, is drawn after the recent survey made by Col. Francis Klett, manager of the cave, who had the advantage of previous experience in connection with the U. S. Geographical Survey. The portion beyond Echo river was drawn from a general survey of courses, the details being filled from older surveys, corrected by my repeated observations. I have not attempted to indicate more than 50 or 60 of the 223 avenues that are said to have been explored. The work is confessedly incomplete; but, such as it is, it comes nearer the truth than anything previously issued. The different levels are indicated by shading, the lighter shades denoting the higher galleries, and the darker marking the lower tiers and the pits and domes. The relative width of enlargements is only suggested as an aid to the imagination; the average being about 60 feet, and the greatest width not exceeding 400 feet. The vertical distance between the surface and the drainage level is about 328 feet. The general plan of the cavern is that of a vast labyrinth, with galleries crossing each other at various levels, the aggregate length of which is supposed to be 150 miles.

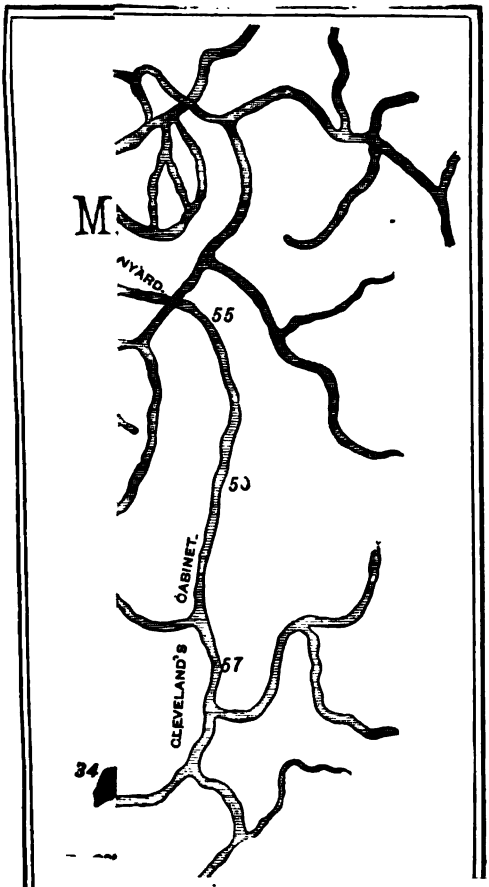
3. *Luray Cavern*, in Page county, Virginia, was discovered in 1878 by Mr. A. J. Campbell, and shortly afterwards was hastily mapped by Mr. A. Y. Lee, for the N. Y. Herald. Another map was made in 1880 by Mr. S. Z. Ammen. These were taken under the disadvantage of having to work by candle-light. This cave is now lighted by electric lamps throughout its entire extent, and by this aid a new survey was made this year for my use, with the endorsement from the manager, Mr. R. R. Corson, that "this is the only correct map of Luray Cavern ever made." The great room including the Elfin Ramble and Pluto's Chasm is about 500 feet long and 200 feet wide. Other large halls, *e. g.*, the Cathedral, and the Giants' Hall, are so filled with stalagmitic pillars as to prevent the visitor's getting a true conception of their vastness. Winding in and out among these mazes of stone forest and jungles, one is liable to magnify distances. The areal diameter of Luray Cavern does not exceed 2,000 feet, but the visitor usually imagines himself to have gone several miles under ground. The place has been aptly compared to the Centennial Buildings, when in their full glory at Philadelphia, amid whose embellished halls the visitor could ramble for days without satiety; and from which, on growing weary, he could at any moment withdraw, hoping to come again.

MAP OF LURAY CAVERN.



1. The Vestibule.—2. Washington's Pillar.—3. The Flower Garden.—4. The Amphitheater.—5. Natural Bridge over Muddy Lake. 6. The Fish Market.—7. The Crystal Spring.—8. Proserpine's Pillar. 9. The Spectral Column.—10. Hovey's Balcony and Scarfs.—11. Oberon's Grotto. 12. Titania's Vail. 13. Saracen's Tent, and Fallen Column.—14. The Organ and Throne.—15. The Tower of Babel.—16. The Empress Column.—17. The Hollow Column.—18. Henry Baird (or Double) Column.—19. Chalcedony Cascade.—20. The Coral Spring.—21. The Dragon of Luray.—22. Bootjack Alley.—23. The Mermaid, or Scaly Column.—24. The Lost Blanket.—25. Helen's Scarf.—26. Chapman's Lake.—27. Broaddus Lake.—28. The Castles on the Rhine.—29. The Imperial Spring.—30. The Skeleton.—31. The Twin Lakes.—32. The Engine Room.—33. Miller's Room.—34. Hawes' Cabinet.—35. Specimen Avenue.—36. Proposed Exit Avenue.

NOTE. In connection with the above paper, maps were exhibited by the author, showing the results accomplished in surveying Mammoth, Wyandot and Luray caverns, copies of which are here reproduced. At the evening reception given by President Dawson at the Redpath Museum, Mr. Hovey gave illustrations of cave scenery, by means of a lantern, from recent photographs taken by means of electric light.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DEEP-SEA SOUNDINGS AND TEMPERATURES IN THE GULF STREAM OFF THE ATLANTIC COAST, TAKEN UNDER THE DIRECTION OF THE U. S. COAST AND GEODETIC SURVEY. By J. R. BARTLETT,[1] of Lonsdale, R. I.

[ABSTRACT.]

I HAVE come to this meeting of the Association at the kind request of Mr. Hilgard, the Superintendent of the Coast and Geodetic Survey, to tell you of the investigations made by the steamer "Blake."

The hydrographic party of which I have charge has been investigating the Gulf Stream for the past four years, the last two summers having been passed on our immediate coast. All of you have ideas or pet theories in regard to this great river of the ocean, as Maury has happily called it. Many of our greatest scientists have written learnedly about it. Professor Bache, while he was Superintendent of the Coast Survey, caused extensive and systematic investigations to be made of its physical features. From the data obtained, the Stream was assumed to start from the Caribbean sea, making the circuit of the gulf of Mexico, and to issue with great velocity and high temperature from the straits of Florida, continuing to about latitude 45°N. It was described as a superficial stratum of warm water flowing over one of cold, with banks of cold water, and divided or bifurcated into warm and cold bands.

The bifurcation was explained by the presence of hills and valleys which were developed by the soundings, the cold water being said to be in the valleys and the warm water on the elevations. As no soundings were obtained in the strength of the current, it was supposed that the stream ran over, if not in, a great trough or depression of the sea-bed. At the time the investigations were made under the direction of Professor Bache, the means of obtaining the depth of the sea were very imperfect; now we have the most improved apparatus and soundings can be taken with wire in any current and to any depth with almost perfect accuracy.

The investigations made on board the "Blake" show conclusively that the above assumptions are wholly erroneous. My soundings give no channel for the Stream to run in, there are no hills or valleys, and I have never found any bifurcation or warm and cold bands. The temperatures obtained do not show cold water beneath the Stream, but a current of warm water between the Florida straits and cape Hatteras down to the very bottom.

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[1] Commander U. S. Navy.

As has already been stated to this Association by Mr. Hilgard, the current entering the gulf of Mexico from the Caribbean sea does not make the circuit of the gulf as formerly supposed, but passes to the northward and eastward in the same general trend as the Yucatan plateau, and thence to the straits of Florida.

The lines of soundings taken by the "Blake" were twelve in number, beginning at Jupiter inlet, Florida, and extending to Currituck, N. C. They were run normal to the general coast line.

The soundings give very interesting data in regard to the physical features of the bottom of the ocean over which the Gulf Stream flows. Instead of a deep channel in the course of the Stream, as reported by Lieutenants Maffitt and Craven, and published in the Coast Survey Reports, our later soundings with *wire* show an extensive and nearly level plateau extending from a point to the eastward of the Little Bahama banks to cape Hatteras. Off cape Canaveral this plateau is nearly 200 miles wide, and gradually contracts in width to the northward until reaching Hatteras, where the depth is more than 1,000 fathoms within 30 miles of shore. The plateau has a general depth of 400 fathoms, suddenly dropping off on its eastern edge to over 2,000 fathoms.

The course of the Gulf Stream can be traced by a study of the specimens of the bottom obtained; on each side of the Stream the sounding cylinder brought up ooze, but in the strength of the current the bottom was washed as bare of ooze and all living things as the bed of a mountain torrent. Instead of a cushion of cold water at the bottom, the temperatures were the same as those found at the corresponding depth of 400 fathoms in the Windward passage and in the course of the current through the Caribbean sea and gulf of Mexico.

The temperature of the surface-water was taken every mile on all lines and in no case were there found any warm or cold bands. There was a slight rise of temperature on entering the current, at the surface, and also a corresponding rise at the bottom in the same locality. The surface temperatures found in the Stream were much below those generally given in published works on the subject. The average temperature in the axis of the Stream rarely exceeded 83°F. in June and July. On one or two occasions the thermometer read as high as 86°, and once 89°, but it was at high noon in a dead calm. The temperature at five fathoms did not range above the average of 81½°. The surface temperatures did not indicate a cold wall inside of the Stream; the water between the one hundred fathom-line and the shore seemed to be an overflow of the Stream, as the temperatures at five, ten and fifteen fathoms were nearly as high as those found in the Stream.

Lines for series of temperatures from the surface to the bottom were run during the past summer from Block island to the Bermudas, and thence to Hatteras. The isothermals show the Labrador current until nearing the Stream, when they descend gradually and in the stream itself abruptly, to their greatest depths. Instead of the warm stream-water thinning away as it was reported to do when spread out, it was not much over fifty miles in width at the time of our crossing, as shown by the current and high surface temperatures. The temperatures below the surface were much higher than at the same depths off the coast.

The ordinary temperature at the bottom off Savannah and Charleston in 400 fathoms was 45°. In the Stream between Block island and the Bermudas it was as high as 55° at 400 fathoms. The isothermals remained at almost the same depth to the southward, as in the Stream, on the entire line to the Bermudas. Just north of the Stream the temperature at 400 fathoms was 39½° and 40°. At a point well in the Labrador current away from the Stream, the temperature at 400 fathoms was 38½°.

From the Bermudas to Hatteras the isothermals were at the same depths as were found south of the Stream on the previous line, but when in the current off Hatteras, where the Stream trends to the eastward, they rose to the same depth as off Charleston and Savannah on the plateau.

These temperatures below the surface seem to suggest that the Labrador current underruns the Stream at Hatteras, but at no other point. It probably keeps its natural boundary or western wall along the 1,000 fathom curve, and thus follows around the plateau towards the equator. No definite conclusion can be drawn from the data obtained thus far; but we have at least a clear field to work upon in the future, in regard to temperatures and currents, and the contour of the bottom as far as Hatteras is well portrayed.

Tables are appended giving the depths, etc., found on the severa lines.

PREGLACIAL CHANNEL OF EAGLE RIVER, MICHIGAN. By CHAS WHITTLESEY, of Cleveland, Ohio.

[ABSTRACT.]

IT is to little purpose that an abstract of this paper is give without the illustrations. Before the glacial era, the Eagle River at the Phenix Mine, on Keweenaw Point, ran on the back of vein carrying native copper, which bears about north 20° wes The depth of the channel was forty to sixty feet, very straight, an as it wore away the vein matter, masses of copper were left at th bottom, the largest weighing nearly a ton. On Point Keweena the drift striæ are about north 50° east, nearly at right angles the old channel, and as the ice covering moved southwesterly pressed the gravel, sand and hard pan into the channel, with gre force. It was not only filled, but covered, to the depth of thirt to ninety feet, with the material of the moraines.

By mere accident Mr. Martin Coryell, M. E., who was in 1845 superintending the mine, while driving an exploring adit in th rock from the bottom of a shaft, struck the ancient channel, th vein, and the copper boulders. The water of the present chann was running on the gravel thirty to forty feet overhead. Abo 200 feet northerly, the present river makes a right angle to the we at the foot of a high drift ridge which crosses its course. Mr. Co yell followed the ancient channel straightforward under the ridg which must have joined the present channel between the mine a Lake Superior. He left such plans of the underground work, th in 1854 I was able to construct a map of the two rivers which w presented to the section. Evidently there had been little loose s face material in the preglacial ages.

RECENT DISCOVERIES OF FOSSIL FISHES IN THE DEVONIAN ROCKS OF CANADA. By J. F. WHITEAVES, of Ottawa, Canada.[1]

[ABSTRACT.]

DEPOSITS containing remains of fishes remarkably like those of the Old Red Sandstone of Scotland and Russia were discovered by Mr. R. W. Ells, M. A., of the Geological Survey of Canada, in 1879, at Scaumenac Bay, on the north shore of the Restigouche River and almost immediately opposite Dalhousie. Mr. A. H. Foord, of the same Survey, has since devoted the whole of the summer seasons of 1880 and 1881, as well as part of the present summer to a systematic exploration of these fish-bearing beds, from which he has succeeded in obtaining a large and instructive series of specimens. Prior to 1879 all the rocks at Scaumenac Bay were regarded as belonging to the Bonaventure division of the Lower Carboniferous, but the fossils recently collected show that at this locality the conglomerates and red sandstones of the Bonaventure series are underlaid by shales and sandstones of Devonian, and most probably of Upper Devonian age. The genera and species of fishes collected from these shales and sandstones may be thus briefly indicated.

1. *Pterichthys Canadensis.* Extremely abundant and first described under the above name in the American Journal of Science for August, 1880. It may possibly prove to be identical with the *Bothriolepis ornata* of Eichwald, from Scotland and Russia, but that species, of which only one or two isolated plates have been found, has never been defined with sufficient accuracy to be recognizable. The Canadian specimens show most conclusively that there is no essential difference between *Bothriolepis* and *Pterichthys*, a fact to which Dr. Pander had called attention in 1857. The divergent, slightly curved barbels on the front margin of the head, which are indicated in Agassiz's restoration of *Pterichthys*, but whose existence was doubted by Pander, are well seen in one of the Scaumenac Bay specimens. The singular flattened-conical dermal appendages of the helmet, one on each side of the orbit, and represented in Pander's more recent restoration of the genus, are equally well exhibited in two other specimens from the same locality.

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[1] Palæontologist to the Geological Survey of Canada.

2. *Diplacanthus.* An apparently undescribed species, remarkable for the small size of each of its fin-spines in proportion to the general dimensions of the body.

3. *Acanthodes Mitchelli?* Egerton. A few examples of a diminutive Acanthodean, which seems to be barely distinguishable from the Scotch species named above. The entire length of the largest individual is not more than one inch and three-quarters.

4. *Phaneropleuron curtum.* A shorter and deeper form by far than the only previously known species of the genus, *P. Andersoni* of Huxley, from Dura Den. Dr. Gunther has shown that of all the Devonian fishes *Phaneropleuron* comes nearest to the living *Ceratodus Forsteri*, of Queensland, both in its internal and external characters, though the dentition of *Phaneropleuron* was very imperfectly ascertained. Last summer Mr. Foord was fortunate enough to obtain specimens of *P. curtum* at Scaumenac with the cutting and crushing teeth well preserved. The maxillary teeth are short, curved-conical, smooth and somewhat compressed. The palatal teeth, like those of *Dipterus*, as figured by Hugh Miller in the "Footprints of the Creator," are triangular in outline and bear parallel rows of short, erect, conical denticles, thus making the affinities of *Phaneropleuron* with the living *Ceratodus*, and to the Dipnoi generally, still more apparent.

5. *Eusthenopteron Foordi.* Many large fragments of a fish, which when perfect must have been more than three feet in length, and which appears to be the type of a new generic form allied to the *Tristichopterus* of Sir Philip Egerton, from which it differs only in the following respects. In *Tristichopterus* the vertebral centres are ossified, the osselets of the lower lobe of the tail spring from 8 or 9 interspinous bones, and the teeth (according to Dr. Traquair) are conical and circular in section. But in *Eusthenopteron* the vertebral centres are not ossified, the caudal osselets of the lower lobe of the tail are articulated to the dilated extremities of the hæmal spines, and the teeth are compressed conical with sharp lateral cutting edges. The entire jaw of *Eusthenopteron*, in fact, is singularly like that of the so-called *Asterolepis* of Hugh Miller, as represented in the "Footprints."

6. *Glyptolepis.* A single specimen, not very well preserved, of a small scaled species, which is very similar to and possibly identical with the *G. microlepidotus* of Agassiz.

7. *Glyptolepis.* A second species appears to be indicated in the collection by large, isolated scales fully one inch in length, belonging to a fish more of the type of the *G. leptopterus* of Agassiz.

8. *Cheirolepis Canadensis.* A fine large *Cheirolepis* nearly related to the *C. Cummingiæ* of Agassiz from the Old Red Sandstone of Scotland, of which it may prove to be only a variety. The sculpture of the scales on the body and fins, and the general outline, are alike in both, but in the Canadian species the position of the fins is different to that of those of its European representative.

On the south bank of the Restigouche, about half a mile above Campbellton, another series of fish-bearing deposits was discovered by Mr. Ells in 1881. These also have been carefully explored by Mr. Foord, and a description of the species contained in them has since been published in the "Canadian Naturalist." At this locality the fish remains occur in brecciated limestones of about the age of the Oriskany and Gaspé sandstones, and much disturbed by trappean outbursts and overflows. The specimens, though often well preserved, are generally fragmentary, and the species so far recognized are as follows:

1. *Coccosteus Acadicus.* Cranial shields and detached post-dorsomedian, ventromedian and preventrolateral plates of a species of *Coccosteus* in which the ornamentation of the head shield is like that of *C. decipiens*, and the shape of the post-dorsomedian plate is the same as that of *C. cuspidatus.*

2. *Cephalaspis Campbelltonensis.* Large head-shields of a *Cephalaspis* of the subgenus *Eucephalaspis*, which show the orbits and the prominences and depressions in their vicinity very clearly. In all the other species of the genus the sculpture of the exterior consists of minute raised tubercles, but in this one the enamel is very finely punctate.

3. *Ctenacanthus latispinosus.* Fin-spines only, characterized principally by their regularly nodose, radiating ribs.

4. *Homacanthus* (species undetermined). Fin-spines, also, of a species of *Homacanthus* which seem to differ from the *H. arcuatus* of Agassiz only in size, the Campbellton specimens being much larger than the European ones.

At Scaumenac the only other fossils found associated with fishes are land plants, which latter have been described by Principal Dawson. The plants as well as the fishes found in the Campbellton breccias are entirely different from those at Scaumenac, and at Campbellton, entomostraca, *Spirorbis Erianus*, fragments of a large *Pterygotus* and two new species of *Cyclora* occur with the fishes.

The paper was illustrated by numerous specimens and by diagrams prepared by Mr. Albert Grignard, late draughtsman to the Canadian Survey.

NOTE ON THE OCCURRENCE OF SIPHONOTRETA SCOTICA (DAVIDSON) IN THE UTICA FORMATION NEAR OTTAWA, ONT. By J. F. WHITEAVES, of Ottawa, Ont.

[ABSTRACT.]

IN the spring of 1881 three single valves of a brachiopodous shell, whose margins are densely fringed with a single row of fine hairlike spines, were collected by Mr. J. W. H. Watts, R. C. A., from a band of impure limestone in the Utica Slate of Cumming's Bridge, near Ottawa City. These specimens have since been presented by Mr. Watts to the Museum of the Geological Survey of Canada. Upon examination with an ordinary simple lens it was at once apparent that they were referable to De Verneuil's genus *Siphonotreta*, and that in most respects they bore a very close resemblance to an English species, the *S. Anglica* of Prof. Morris. But the spines of *S. Anglica* are distinctly stated to be annulated, whereas in the Canadian specimens, as viewed under an achromatic microscope with an inch and a half objective, the spines appeared, to the writer at least, to be quite smooth.

Having occasion to send some Canadian brachiopoda to Mr. Thomas Davidson, F. R. S., a few months ago, for examination and comparison with British species, the three examples of the Ottawa *Siphonotreta* were forwarded to him at the same time, and in a letter received from Mr. Davidson in May last they are reported upon as follows:

"The *Siphonotreta* from near Ottawa interests me much. It is identical in shape and characters with the Upper Llandeilo species which I named *Siphonotreta Scotica.* I am very uncertain whether the Wenlock Shale species named *S. Anglica* by Morris is the same or not. Only one crushed specimen of the *S. Anglica* has been found and its spines are annulated as described by Morris. I could see no annulations in the spines of many specimens of *S. Scotica* found by Mrs. Gray in the Upper Llandeilo of Craighead, Ayrshire, nor do I see any in your specimens. As there is uncertainty as to the specific identity of the highest Upper Silurian form with the Lower Silurian one, and as none have been found in all that mass of intervening strata, I prefer provisionally to retain the two names, or until other Upper Silurian species shall have been found."

The interest of Mr. Watts' discovery lies of course in the fact that this is the first time that a species of the genus has been recorded as occurring in N. America.

On a Post-Tertiary Deposit containing Impressions of Leaves, in Cumberland county, New Jersey. By N. L. Britton, of New York, N. Y.[1]

[ABSTRACT.]

The deposit which this paper is intended to describe has long been known to a few geologists, but no detailed description of it has as yet been given. It is located near the city of Bridgeton, Cumberland county, N. J., near the shore of a large pond from which a stream flows and empties into the Cohansey Creek.

The material which bears the leaf-impressions is a rather coarse quartz sand, cemented together by hydrated sesquioxide of iron into a soft, friable sandstone. The stratum containing them is found immediately at the surface of the ground, apparently extending into the side of a low hill; it is at least four feet in thickness, and in portions of it the impressions are very abundant, while at other points only an occasional one is to be found. The outcrop of this leaf bed may be traced for some twenty-five feet along the hillside. Its inclination is very nearly horizontal. This sandstone belongs geologically to the superficial formation which covers the Tertiary and Cretaceous strata of New Jersey, and also extends northward to some extent, as far as the terminal glacial moraine on the red shales and sandstones of the Trias. The materials composing this superficial formation are gravel, sand and loam, uniformly of a light color, generally yellow, white or gray, and from this circumstance it is known as the yellow drift; it is also termed the Pre-glacial drift, because it is older than the drift deposits due to glacial action.[2] This yellow drift is generally plainly stratified; it varies from forty feet or even more to a fraction of an inch in thickness, being sometimes represented only by a few yellow pebbles; it varies in coarseness from gravel with pebbles some inches in diameter to a fine sand or loam. It is often found that this material is cemented together into a reddish-brown sandstone or conglomerate by hydrated oxide of iron precipitated from springs since its deposition. This rock forms the only building stone native to the region and it is extensively used for lining wells, cellar walls, etc. A few buildings have been constructed of it, as the large seminary at Bridgeton. Casts of marine mollusks have been found in it at Tuckahoe, Atlantic county, where it contains casts of what were apparently the shells of the common hard clam (*Venus mercenaria*) and others, and I have recently secured specimens of other mollusks, mostly too poorly preserved for identification, in the yellow drift near Marlboro, Monmouth county. It also contains silicified fossil wood at Bridgeton, Shark River, New Egypt, Mullica Hill, and other localities.

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[1] Of Columbia College and assistant in the Geological Survey of New Jersey.

The stratum bearing the leaf-impressions at Bridgeton is a rather coarse, soft sandstone — so coarse indeed that the margins of the leaves are often very indistinctly preserved making it difficult to determine the outline. The primary venation, however, and sometimes the finer subdivisions of the veins are easily to be seen.

The identification of the species represented is not yet completed. Among the genera we have *Liquidambar*, the sweet gum represented by several specimens of leaves resembling those of the living species (*L. styraciflua* L.) but only three-lobed, instead of five- or seven-lobed as the leaves of that tree are; numerous specimens of a species of *Viburnum*, as shown by the venation; fragments of a broad-leaved grass, perhaps *Zizania;* serrated leaves of a species of *Ulmus*, and several other genera.

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[2] For a full account of the Pre-glacial drift, see Annual Report of the State Geologist for 1880, pp. 87-97.

This is the only locality known in New Jersey where the preglacial drift contains leaf-impressions. The species will be figured and described in reports on the fossil floras of New Jersey, now in preparation.

TERRACES AND BEACHES ABOUT LAKE ONTARIO. By J. W. SPENCER, of Columbia, Missouri.

[ABSTRACT.[1]]

IN and about the Dundas valley, at the extreme western end of Lake Ontario,—topographically described by the author in his paper on the Origin of the Lakes, in the Proceedings of this Society for 1881,—there are the remains of a considerable number of terraces and beaches.

The lowest of these beaches is that forming the modern western shore of the lake. Burlington beach, which separates Burlington bay (a body of water about fifteen miles area and seventy-eight feet deep) from Lake Ontario, rises only eight feet above the water. It is composed wholly of sand and gravel (containing an abundance of fossils belonging to the Hudson river formation) which have been transported by ice and wave action, a distance of at least twenty or thirty miles, from a region northwest of the lake, along with a few crystalline pebbles. This beach and shore line contains no Niagara pebbles, although almost beneath the Niagara escarpment.

In elevation, the next terrace is south of the Dundas marsh, at from 70 to 80 feet above the lake, and forms a plane of considerable area, made up of fine, thinly bedded, arenaceous clay.

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[1] For the minute details of the subject of this paper, the reader is referred to the author's lengthy paper on the "Surface Geology of the Region about the Western End of Lake Ontario," in the Canadian Naturalist for 1882, copies of which can be obtained from Dr. Spencer. See also American Journal of Science for Dec., 1882.

The third and most conspicuous terrace is that at 116 feet above the lake and extends as a contour line, for long distances about the western end of the lake. It forms "Burlington height" which separates what is now the large Dundas marsh (at the level of the lake) from the lake. It is composed of the same material as the present lake shore — Hudson river sands and pebbles, without any admixture of Niagara pebbles.

Again in the Dundas valley we have isolated remains of former stratified shore lines at 180, 224, 261, 335, 440 (?), and 500 feet above the lake. All of these higher beaches are composed of sand and pebbles of the Hudson river period, with less or more *débris* of the adjacent Niagara formation. In fact, the study of the beaches about the western end of Lake Ontario shows a gradual recession of the water of the lake, with few sudden changes of level. Also the transportation of the greater proportion of the material of all the beaches was from the country northwest of the lake, and from lower elevations than those of the old shore line.

Between Toronto and Lake Simcoe, Mr. Thos. Roy measured beaches at 110, 210, 282, 310, 346, 402, 422, 502, 558, 626, 682, 734, and 764 feet respectively above Lake Ontario. In addition to these gravel beaches, others, at 600 feet, and on descending towards Georgian bay (along the Northern Railway) at 520, 388, and 354 feet, have been measured.

Along the Toronto, Grey and Bruce Railway, which extends in a direction north of westward from Toronto to and across the highest portions of the peninsula of Ontario, underlaid in part by the "Artemesia gravel" ridges, there are a number of conspicuous sand and gravel beds, which follow contour lines more or less closely. Edmund Wragg, Esq., furnished me with the following railway levels of many of these beds. They have been measured at 160, 280, 370, 710, 990, 1,120, and 1,340 feet above Lake Ontario. After crossing the summit of the road at 1,462 feet, there are extensive gravel beds at 1310, and from 1000 to 697 feet above the same datum along the main line of the railway, and along the western branch of the road, at 1299, 1130, 1050, 870, 850, and 830 feet above Lake Ontario. Near Owen Sound there are others at 546, 496, and 466 feet above Lake Ontario.

Along the Great Western Railway, in the valley of St. Davids, the interglacial outlet of the Niagara river is filled with stratified sand and gravel of the Hudson river formation from about 250 feet to 380 feet above the lake.

The elevations of terraces in New York State are imperfectly known to me. Eastward of Lockport there are the lake ridges from 158 to 190 feet above the lake (Hall). In the region of Seneca lake and Skaneateles lake, I have observed beaches of various heights to about 625 feet above Lake Ontario. In Michigan, there are beaches to 1350 feet above Lake Ontario (Rominger). Near Petit Écrit, Lake Superior, there are beaches at 398, 408, 458, 592, 627, 635, and 699 above the same datum (Geol. of Canada). Again, southwest of Lake Erie, there are beaches or ridges at 65, 90, 165, 195, 220, 350, 408, 386–490 feet above Lake Erie (Messrs. Gilbert and Winchell).

In the Ontario peninsula, the outline of the Artemesia gravel may be approximately represented by the 1250 foot contour line, though extending south of it. This great belt of gravel is generally parallel with the Niagara escarpment, and is mostly composed of Guelph dolomites, or, in some localities of *débris* from adjacent rocks. There are more or less fragments of Laurentian and Huronian crystalline rocks, intermingled with the other well water-worn material. Indeed, there are often rounded fragments of Hudson river pebbles (these coming from lower elevations).

From the eastern side of the Artemesia gravel a narrow ridge extends for nearly a hundred miles, nearly parallel to, and only a few miles distant from, Lake Ontario, known as "Oak Ridge." Its most conspicuous portion may be represented by the contour line of 650 feet above Lake Ontario, although the highest portion rises 893 feet. This ridge has an elevation of from 200 to 300 feet above the broad rock-bottomed trough on its northern side, which extends from Georgian bay to the eastern end of Lake Ontario. A spur of this ridge extends southward to Lake Ontario at Scarboro heights, a few miles east of Toronto.

There is a resemblance between the Artemesia ridges and the so-called Kettle moraines of Wisconsin, Coteau des Prairies and Coteau de Missouri. Between these ridges there is a general parallelism. The Artemesia gravel reaches 1700 feet above the sea—a height as great as portions of Coteau des Prairies.

The deposit of the Artemesia gravel is simply around the high rocky floor of a portion of country, and marks the recession of the waters, in more or less perfect contour lines, with most of the material of local origin and stratified.

Whatever barriers may have separated the lake region from the sea, there seems no doubt that the whole area was submerged beneath the sea level to at least 1700 feet, for no glacial lake could account for the high level beaches.[2]

As the continent was rising the waters of this inland lake had many channels communicating with the exterior sea, across Ohio and New York, besides that by way of the St. Lawrence valley. Local oscillation doubtless played an important part in letting out the confined waters, but to what extent, it cannot yet be determined. Below 1200 feet above the sea-level of to-day, the principal old outlets are by the valley of Cayuga lake, at 1015 feet; by the valley of Seneca lake, at 865 feet; by the Mohawk river, at 434 feet, and by the present St. Lawrence valley, at 247 feet. Dr. Newberry enumerates various other outlets in Ohio, at 936, 968, 909, 910, and 940 feet above the present ocean level.

There is a marked connection between some of these old outlets, and the beaches which rise a few feet above them, in that they are the most conspicuous and widespread.

The explanation of much of the transported material, and especially the rocks of Hudson river formation, at localities situated higher than their original sources, can best be accounted for by the theory that they were carried upward during the time when the continent was undergoing subsidence, and that they were re-arranged by the waves and shore ice of a subsequent period.

Let us now consider the origin of *Burlington beach* and *Burlington heights.* These beaches, separating bays from the lake at different times, owe their origin to common causes. The materials for both of these beaches (whether at 8 feet or 116 feet above the present lake), were brought some distance from the northwestern side of the lake, and not from local material; for had such been brought down, their composition would be pebbles of Niagara rocks in place of those of the Hudson river formation. Moreover, the transporting power, and also the quantity of water of the streams of the Dundas valley, had become very unimportant before any of the deposits succeeding the interglacial epoch. Formerly, for the higher ridge, when the lake was 116 feet above its present surface, as afterwards for the present Burlington beach, the material was transported by shore ice and wave action to the present position of the deposits, at a time when the winters need not have been colder than at present. The pebble laden ice would naturally get piled up by the action of the winter waves (as at the present day) at the more confined end of the lake, and eventually the ice would deposit its load to form beaches. The bays at the end of the lake are now frozen over in winter, while the lake has often no more than ice-fringes. As the streams now entering the lake all do so through marshes, there would not be the slightest power from them to act against the waves to produce the present beach separating Burlington bay from the lake. Moreover, the action which built up Burlington heights moved from the open lake, as shown by the inclined bedding of the deposits.

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[2] Since the paper of which the above is an abstract was written, Dr. Dawson, the President of this Association, has described the occurrence of the remains of a large whale, in gravels northeast of Lake Ontario, at an elevation of 440 feet above the sea—thus proving that during the formation of some of the widespread beaches of the lake basin, there was direct connection with the sea.

OCCURRENCE OF GRAPTOLITES IN THE NIAGARA FORMATION OF CANADA. By J. W. SPENCER, King's College, Windsor, N. S.[1]

[ABSTRACT.]

THE plant-like forms, that are included in the Graptolite family, have hitherto been regarded as essentially belonging to the Cambrian and Cambro-Silurian ages. They are generally preserved in the more muddy deposits of these eras. However, there has been discovered a Canadian locality in the Niagara formation, containing a graptolitic fauna, almost as large and variable as that of the Quebec group.

In the Palæontology of New York, Professor Hall described two species of Graptolites from the Clinton shales and three species from those of the Niagara formation. Subsequently, Professors Hall and Whitfield described one other species found in

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[1] Since the above paper was read, the author's address is changed to State University, Columbia, Mo.

Ohio, and Principal Dawson another occurring in Nova Scotia. The rich graptolitic fauna of the Niagara formation was discovered by the careful and continued search of Col. Charles Coote Grant (H. P. Sixteenth Regiment) at Hamilton, Ontario. From the material obtained at that city, principally by Col. Grant, I have been enabled to study out twenty-nine new species, in addition to those previously known, the whole fauna representing twelve genera, which lived in the Niagara and Clinton seas. Several other species doubtless exist, but their remains are in too fragmentary a condition for determination.

The graptolites at Hamilton occur mostly in nearly non-bituminous limestones, whose structure, under the microscope is seen to consist of recrystallized grains of dolomite. However, some of the best preserved specimens are obtained from the more shaly limestones. Owing to the condition of preservation, although the external forms are often perfect, and the chitinous substance in excellent condition, yet the cellular structure is exhibited in only occasional specimens. It sometimes happens that the impressions of the cellular orifices are well shown on the surface of the stone where the carbonaceous matter has been removed. In the study of these forms it has frequently been necessary to depend upon the relative sizes and modes of branching in order to determine the species. In many cases the solid axes of the stipes are shown to be lateral while in others they are central — a most important character.

The geological distribution is given by Professor Hall as follows: one species from the Potsdam group; fifty-three species, of twelve genera, from the Quebec group; four, from the Trenton; thirty, of eight genera, from the Utica and Hudson groups; two, from the Clinton; three, from the Niagara; one, from the Corniferous; and one, from the Hamilton group.

In the Canadian Naturalist for 1878, I described nine new species and three new genera. Of these genera, *Calyptograptus* closely resembles *Dictyonema*, with few or no cross-bars, but with branches overlapping. I have seen one species from the Niagara group of Kentucky (locally included in *Dictyonema*), and one from the Hudson group near Cincinnati, which evidently belonged to this genus. Another new genus then described was *Acanthograptus*, nearly related to *Inocaulis*, and the third was *Rhizograptus*, a cyathiform frond with slender axis and large bulbous root. A fourth genus I have recently called Cyclograptus, which consists of a small circular frond, composed of a central disk and numerous short celluliferous stipes. (Types of the new genera were here exhibited).

The specific arrangement of the Niagara graptolites in genera is as follows:

Phyllograptus,	one	species.
Retiolites,	"	"
Dendrograptus,	six	"
Callograptus,	four	"
Rhizograptus,	two	"
Acanthograptus,	two	"
Inocaulis,	seven	"
Thamnograptus,	two	"
Ptilograptus,	one	"
Cyclograptus,	"	"

besides one species of *Dictyonema*, and one of *Inocaulis* not found at Hamilton.

The study of these organisms is completed as far as possible up to the present time, and in the course of a few months I hope to have it published in Part 3 (Palæontology) of the "Geology of the Region about the Western End of Lake Ontario."

A MASTODON AMERICANUS IN A BEAVER MEADOW. By SAMUEL LOCKWOOD, of Freehold, N. J.

[ABSTRACT.[1]]

PROFESSOR Lockwood, in his paper, gave the particulars of his discovery and exhumation of the remains of a mastodon in a peat meadow about two miles west of Freehold, N. J. He had previously discovered in a peat-bog near the same village a fossil beaver-dam, which enabled him to detect some resemblances, and thus to suspect that this mastodon had been actually buried by beavers. This suspicion became an induction warranted by the facts which the digging disclosed, for fragments of beaver sticks were found lying across the neck of the skeleton, which lay on its right side, the tusks being deflected under the neck. The remains lay on the "hard pan" beneath the peat. It was shown that the monster died on the right bank of the ancient stream, that the beavers afterward dammed it and the pond covered the skeleton. In due time the pond was abandoned and the dam wall, as it sunk down by decay, drained the pond. As its area decreased, the area of swamp increased, and a growth of sphagnum peat buried the bones. The drainage finally became such that a meadow resulted.

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[1] Published in full in the Montreal Herald, Aug. 25, 1882. Also in the Popular Science Monthly, January, 1883.

The writer mentioned three instances of his finding in Monmouth County mastodon relics on the coast. Twice at long intervals were teeth obtained out at sea from Long Branch. In the other instance a part of a tusk and other bones were got from an ancient buried swamp about fifteen miles south of Long Branch, which swamp was uncovered by a severe storm from the sea.

The points made in the paper were: (1) The presentation for the first time of the evidence of the mastodon for the subsiding of a part of the eastern coast-line of the United States. (2) The long life range of the mastodon. Beginning with the Pliocene, it was shown that the individual in question was living at a period well up into very recent geologic time. (3) The mastodon came in with the great extinct fossil beaver, which it outlived, and became contemporary with the modern beaver. (4) The mastodon lived to be contemporary with the American autochthonic man, and, probably as a food supply, before him this Behemoth melted away.

Dr. Lockwood said that the tusks which were uncovered in perfection must have contained 400 pounds of ivory, but these and the bones, excepting four teeth, literally crumbled away on reaching the air, to his inexpressible grief.

CURRENTS OF AIR AND OCEAN IN CONNECTION WITH CLIMATES, REGIONS OF SUMMER RAINS AND SUMMER DROUGHTS. By J. BEAUFORT HURLBERT, of Ottawa, Canada.

[ABSTRACT.]

THE prevailing winds in the north temperate zone are from southwest towards the northeast. This great current of air may be said to be constant north of latitude 35°. In the upper region of the air it blows nearly every day in the year from some point near the southwest towards the northeast. With a constant movement of the air in high altitudes from the southwest there must be a return current from the north towards the southwest, as there are counter currents in the ocean; but these polar winds near the surface of the earth blow from all the colder points of the compass.

The warm currents of air and water falling upon western coasts, and aërial currents passing over the continents, elevate the temperatures of the western parts of the continents, while the cold currents pressing upon eastern shores lower the temperatures there.

The mean temperature of the Gulf stream in the Gulf of Mexico is 80° Fahrenheit; its maximum temperature is 86°, or 9° above the ocean temperature due the latitude. Increasing its latitude 10°, it loses two degrees of heat, and after running 3,000 miles towards the north still preserves the temperature of summer. With this temperature it crosses the fortieth degree of north latitude, and spreading out for thousands of square leagues over the cold waters of the ocean, does much to mitigate the rigors of winter in Europe. When it strikes the British islands it divides into two parts, the main current going to the Polar sea, the other entering the Bay of Biscay.

It has been estimated that the quantity of heat discharged over the Atlantic, from the waters of the Gulf stream in winter, would be sufficient to raise the whole column of atmosphere which rests upon France and the British islands from the freezing point to summer heat. Every western wind which blows (and the prevailing winds are from the west or from some point near the west or southwest in this part of the ocean) crosses the Gulf stream and carries with it a portion of its heat, discharging it in its passage over Europe. The isothermal lines of 60° and 55°, starting from the parallel of 40° on the American coast, run in a northeasterly direction, retaining nearly the same oceanic temperature on the European side in latitude 55° and 60° as exists on the American coast in latitude 40°.

In the Pacific there are tropic and arctic currents like those in the Atlantic, and from similar causes. The Japan stream, or Kuro-Siwo — black stream — a name derived from the deep blue color of its waters, flows from the southeast of Asia in a northeasterly direction, falling upon the western coast of North America. This stream, flowing many thousand miles further than the Atlantic tropic current, is not so hot nor its littoral waters so cold as those in the Atlantic, but it spreads over the entire Pacific coast of Canada. These two currents in the Pacific — the arctic and tropical — produce similar effects to those in the Atlantic; the one warming the western coast of North America, in high latitudes, and the other cooling the eastern shores of Asia.

Through the agency of these two currents in the Atlantic, the western countries of Europe are much warmer than the eastern parts of America in similar latitudes; the difference being about eight degrees in latitude 41°; eleven and a half in latitude 51°; and twenty-five in latitude 58°. Similar causes in operation in the Pacific ocean give an equal elevation of the temperature of the western coasts of America over the eastern coasts of Asia in the same latitudes — the arctic currents chilling the one and the tropical currents warming the other.

From Vancouver in latitude 49° to Sitka in 57°, the summer temperatures are as high and as uniform as in the west of Europe, except where the vicinity of mountains may modify the normal conditions of climate. Sir John Richardson says " the climate of Sitka" (on the Pacific coast) " is much warmer than that of Europe in the same parallel" (Arc. Ex., Vol. II, p. 279).

The isothermal of 60° for the three summer months rises as high as latitude 63° east of the Rocky mountains in the valley of Mackenzie river. Youkon, west of Mackenzie river and within the Arctic circle, latitude 67°, has a July 65° 7′, and an August of 60°.

In comparing the well known regions of the old world with the less known corresponding parts of the new, western coasts with western, eastern with eastern, and interior divisions with interior, we find a remarkable similarity in the climates of the two continents. Canton, in China, latitude 23°, has a summer temperature of 82°, and Key West, in Florida, latitude 24°, 32′, a summer of 82°. Pekin, latitude 40°, has a summer of 76,° which is only two or three degrees above that of Philadelphia of the same latitude. Mangas-aka, Japan, and Charleston, South Carolina, in the same latitudes, have summers of 80°. London, in the west of Europe, and Vancouver, in the west of North America, in similar latitudes, have the same mean summer temperatures, about 61½°; Sitka, in latitude 57°, Sir John Richardson says, has a climate much warmer than Europe in the same latitude.

The climates of the interior are warmer in summer and colder in winter than those on eastern and western coasts, but are somewhat similar on both continents, being, however, warmer on the Red, Saskatchewan, and Mackenzie rivers, than in the same parallels on the eastern continent. The isothermal of 65°, for the three summer months, crosses the Red river in latitude 50°, and rises on the Mackenzie to latitude 60°.

The summer rains, too, throughout Canada, are similar to those in Europe in the same latitudes from the Mediterranean to the Arctic, being somewhat uniform during the agricultural months, but more copious in Canada.

South of the boundary between Canada and the United States, west of the Mississippi, are the areas of summer droughts — a rainless, treeless region, similar in position on this continent, and in the character of the country to the desert areas of the old world — the one beginning on the western coasts of Mexico and California, and extending to British America on the north, and over half the continent eastward; the other beginning on the western coast of Africa near the same latitude as the American desert, and extending northeastward or east by north over Africa, Palestine, Independent Tartary and Manshire Tartary, nine thousand miles in the direction of the prevailing winds.

That portion of the North American continent extending from the Atlantic westward more than 2,000 miles in the latitude of Canada, and from the Gulf of Mexico for 1,500 miles northward, is covered with a mixed forest of coniferæ and deciduous trees, which is unparalleled in extent, and in the variety and value of its woods.

These forests, so beautiful and grand in their primitive state, have a value quite independent of their money worth. They have a most powerful and favorable influence upon the climates of the country, check evaporation, and keep the water longer in the soil, thus supplying the roots of plants, feeding the springs and streams, etc.

West of this vast forest, south of the parallel of 48° or 49°, is the treeless region. The Mississippi may be taken as near the boundary of these two zones marked with such different features, the forest in many places not reaching that river, in others extending beyond it, and again reappearing on the Pacific coast. East of the Mississippi trees appear first along the water-courses and on soils retentive of moisture, being still absent on the uplands and sandy plains. To the north this immense treeless region runs, in the country of the Upper Missouri (longitude 110° west), north of latitude 50°. Throughout these immense areas there is either a total absence of rain in summer, as in the desert parts, or an insufficiency, as on the prairies. The grasses which cover parts of these dry up in summer, but their roots, forming a deep matted sod, have vitality enough to put forth fresh shoots under the rains of spring and autumn; trees, however, which are withered up by the droughts and arid winds of summer, have no such vitality.

Vast portions of America, Africa, Asia and Australia, are destitute of trees, while other equally extensive regions are covered with forests. These treeless zones lie in similar positions on the continents, beginning near the same latitudes on the western coasts and running northeastward in the northern hemisphere, and southeastward in the southern, in the direction of the prevailing winds.

In the Old World (for Europe, Asia and Africa must, on the question of climates, be taken as one body of land) these treeless and mainly desert regions begin on the west coast of Africa, north of the twentieth parallel, and run northeastward or east by north, 9,000 miles over Africa, Palestine, northern Arabia, and Independent Tartary to latitude 50° in Mongolia, ending in the great desert of Cobi or Manshire Tartary.

In North America we have a similar desert-treeless region, beginning in old and new California and on the coasts of Mexico (in the same latitudes as the African desert), and extending to the Mississippi and beyond it, on the east, and to British America on the north. The winds over these desert areas on both continents blow almost invariably in summer from some point near the southwest towards the northeast. These portions of the continents are destitute mainly of summer rains, but have high summer temperatures. Upon the modifications caused in these desert areas by the high lands of Mexico and the vast Mediterranean sea we cannot here enter, but may merely state that the mountains of Mexico limit the deserts there, and that great sea north of Africa causes a more humid air in the south of Europe and limits the deserts in that direction; yet Spain, Italy, Sicily, and the whole country northeastward into Hungary, frequently suffer from summer droughts.

The reason, as a permanent cause, often assigned for these portions of the continent being destitute of trees—namely, the prairie fires — cannot for a moment bear investigation.

Fires were naturally suggested to the first rude settlers, to whom the evidence of sight is the chief guide, as the only cause. In many parts of the African, Asiatic and American deserts and prairies there are no fires, still they are treeless. In other countries, fires are as frequent where forests are permanent, or, if burned down, young trees immediately grow up again. The existence of forests over a region 2,000 miles by 1,000, and their failure *where, and only where*, the summer rains fail and the arid winds prevail, ought to have suggested the explanation.

That the prairie fires sweeping over extended areas may have kept trees from some localities, near rivers or on retentive soils, is possible; but such exceptions, limited and local, have no weight in opposition to the fact that millions of square miles have remained, through all the ages of history, desert and treeless on the borders of other equally extensive areas covered with dense forests. The climates which have produced these two distinct results over those regions have remained permanent for ages, and will remain permanent in the future, unless changes supervene in the entire solar system; but for the calculation of such imaginary phenomena astronomy furnishes no data. We may infer, therefore, that those conditions of climate — heat and humidity in the one case, and heat and aridity in the other — remaining the same, their effects — forests and treeless regions — will be permanent expressions of those fixed efficient causes.

The attempts to account for the deserts of the Old and New

World by the physical configuration of the continents, have led to many ingenious theories. The chief of these is the assumption that mountain chains to the west of those rainless regions condense the vapor brought in the southwest winds, causing heavy rains on the western sides of the mountains, but leaving the winds without vapor east of these mountain chains. The phenomena of rain on the west, but none on the east of the Ghauts in Hindostan and other places, have been taken as sufficient bases for this theory.

That those regions having no summer rains are on similar parts of the continents north and south of the equator, both in the eastern and western hemispheres, beginning on western coasts at about the same latitudes; that there are no mountains west of the Desert of Sahara, and the rainless regions in Australia; that the entire coasts of Mexico, old and new California, west of the mountains, are quite as destitute of rain in summer as the regions east; that those west winds give heavy falls of rain to the north and northeast throughout Canada, and in autumn, winter, and early spring, deposit heavy rain and snow on those interior desert areas east of the Rocky mountains, over which they are said to pass in summer, devoid of vapor,—these and other facts ought to have corrected the erroneous opinions on this subject.

That the great southwest currents of air — the tropical currents —bring the vapor which falls in rain and snow, is here assumed as admitted, for it is the basis of the theory which we are controverting.

Those vast wastes are, at the season of the year when little or no rain falls, highly heated by the summer suns. The southwest winds passing over these become rarefied, and thus, being capable of sustaining even more vapor than at a lower temperature, retain the humidity with which they have come from the tropics so heavily charged, till they reach the cooler regions, north and northeast, where they are condensed into rain or snow. Hence in summer the line dividing the zones of rain from those of drought is farther to the northeast; as autumn and winter approach, those highly heated plains gradually cool sufficiently to condense the vapors in the southwest winds, which now give heavy depositions of rain and snow.

A Source of the Bituminous Matter of the Black Shales of Ohio. By Edward Orton, of Columbus, Ohio.

The great black shale of Ohio, that extends from the Huron river on the shore of Lake Erie, to the mouth of the Scioto river in the Ohio valley, with an outcrop varying from ten to twenty miles in breadth, I have already shown to be a composite stratum. In this determination, I have followed Newberry, who showed in his reports upon the geology of northern Ohio that the two black shales of Cuyahoga county, for example, the lower or *Huron* of his nomenclature, and the upper or *Cleveland*, which are there separated by a wedge of greenish blue, *Erie* shale, when followed to the westward, coalesce, through the failure of the intervening member. In his view, the Cleveland shale comes down by overlap upon the Huron, so that the black shale of Huron county consists of the blended products of the two or three formations already named. This stratum I have followed not only with no hint of interruption or division, but with manifold proofs of unbroken continuity, across the state. The name and geological place of the stratum, as assigned in the volumes of the last geological survey of the state, will both require modification according to this view. It can no longer be called the *Huron* shale, for this element constitutes only its lower portion. The boundary between Carboniferous and Devonian time can no longer be drawn at the line of junction of the Erie and Cleveland shales, for that line has disappeared, and the Cleveland shale is only the upper portion of 350 feet of black shales through which thus far no planes of division have been established.

As to the first point, I know no better name for the present, than the old one, viz.: the *Ohio Black Shale*. Professor Shaler, in his reports, upon the geology of Kentucky has reverted to this designation.

As to the place of the stratum in the geological column, I believe that it will be found necessary to refer it all to one age, viz., to the Devonian. The greater part is unquestionably of this age and for the balance, the fossils are too few, through most of the state, and too indecisive in character, to make any proper ground for establishing so important a division. I will, therefore, designate this stratum the Ohio Black Shale and I will, provisionally at least, refer it to the Devonian age. Its approximate area I have already indicated, a belt of ten to twenty miles in breadth, extending across the state from north to south or for nearly two hundred miles. Its thickness ranges along its line of outcrop, from 250 to 350 feet.

There is another stratum of black shale in the Ohio column, of small thickness but of great persistency and extent. It lies about one hundred feet above the summit of the Ohio shale. It was first distinctly recognized and described by the late Professor E. B. Andrews, who called it the *Waverly Black Shale.* In southern Ohio, its boundaries are very distinct and definite. It is borne by the lower Waverly quarry stone, which is the continuation of the Berea Grit, for that portion of the state. The shale comes directly down upon the quarry stone. Its upper boundary is equally abrupt. It has a thickness in this region ranging from fifteen to thirty feet. Throughout central Ohio, the same description will apply, and no change is needed for the northern portion of the state except that the shale here is not always as dark in color as elsewhere, and its upper boundary is not distinct. It makes the roof of the Berea quarries, as it does of the same stratum everywhere to the southward. Meek separated it by implication from the overlying Cuyahoga shale in describing the fossils that it contains, giving to it the name of *Berea Shale.* Newberry describes it clearly but does not separate it from the Cuyahoga shale. It is charged with fossils, several of which seem characteristic.

These three beds of bituminous shale, viz., the Huron and Cleveland of Newberry and the last-named, the Berea shale, have many points of structure and history in common. They are all marine in origin, as the presence of their fossils attests. They were all formed in quiet waters and not upon shore lines, as their fine and undisturbed lamination shows. But their chief point of agreement is in the fact that is implied in one of their most common designations. They are all *bituminous* shales. According to the analyses with which I am acquainted, they carry from eight to twenty-two per cent of organic matter. It is this that gives them their color. It also renders them combustible, to a greater or less degree. There is scarcely a summer in which some of the shale banks of southern Ohio do not take fire from burning brush heaps and the like. When the conditions favor, these fires may continue for weeks or months. One is said to have lasted in the hills of

Pike county for more than two years. Some of the beds burn with a blaze.

It is this same organic matter, obviously, to which they owe their character as oil-producing shales. Oil and gas springs are constantly associated with their outcrops. They have already been turned to account for the artificial production of oil in Ohio and it is scarcely to be doubted that the great stock of carbon which they contain will at some time be utilized by man.

This organic matter has been referred both to animal and vegetable sources for its origin, and both these divisions of the living world have certainly contributed to it, for the remains of both are found in the shales, but the two factors are by no means of equal value.

The clearest and most extended statement that I know in regard to the accumulation of the bituminous matter of these shales is that made by Newberry in volume I, Geology of Ohio, page 156. He speaks of the Huron shale particularly, but what he says will apply to all of the divisions already named. I quote his words: "The nature of the sediments which form the Huron shale, the exceeding fineness of the mineral matter, the large percentage and uniform dissemination of carbon, the peculiar composition of this carbonaceous element, consisting mainly of hydrocarbons, the abundance of marine plants and the absence of terrestrial vegetation, all combine to show that it was deposited in an open sea and not immediately adjacent to the land."

"Under such circumstances we are compelled to attribute the carbonaceous matter to marine vegetation or to marine animals. If it were derived from marine animals, such as we know have contributed largely to the organic constituents of some rocks, the bituminous limestones for example, we should find much more abundant traces of their structures than we now do, since they are, in fact, almost entirely absent. We are, therefore, compelled to consider this accumulation of bituminous matter as the result of the growth of seaweeds in marine basins."

"Most marine plants with which we are acquainted grow upon the shore or in shallow water, and we can hardly imagine so large an area as that occupied by the Huron shale covered with a growth of shallow-water plants without its bearing evidence in some locality of shore lines. It is true, however, that a great growth of aquatic vegetation sometimes takes place remote from the land and where the plants have no attachment to the bottom. Of these areas, we have a type in the Sargasso Sea, through which Columbus ploughed his way, when making the voyage that resulted in the discovery of America. There, as in other similar sheets of seaweed, the vegetation floats upon the surface of the water and maintains a vigorous and luxuriant growth without connection with shore or bottom. Corresponding to this growth must be the decomposition of vegetable tissue on a large scale. The products of such decomposition would fall to the ocean bottom as finely comminuted carbonaceous mud, mingled with stems and fronds detached by violence or decay. Under all such sheets of vegetation, in a sea where a fine mechanical sediment is being deposited, we must necessarily have an accumulation of mud, containing a large percentage of carbonaceous matter; in other words, the elements of a bituminous shale."

"Waiting the demonstrative solution of the problem which patient and exhaustive study will doubtless sometime furnish, I offer as a possible explanation of the peculiar features of the Huron shale, the suggestion that its carbon was derived from vegetation which lined the shores and covered the surface of a quiet and almost land-surrounded sea."

This statement is expanded almost to the proportions of a theory as to the origin of the black shales. In substance, it refers the bituminous matter of the shales to marine vegetation, the decomposition having been carried to such a point as to destroy almost all traces of structure.

A similar view was briefly expressed by the late Professor Andrews, in the Report of Progress of the Ohio Geological Survey for 1869. On page 65, he says, " the conditions under which this formation" (viz., the Ohio Black Shale) " was deposited, involved comparatively quiet waters, charged with a constant supply of fine sediment with which there was at all times commingled organic matter which alone could have furnished the bitumen. The even distribution of the bitumen throughout the entire mass of the sediments would imply that the water abounded with the minute forms of vegetable or animal life. Thus far, search for their forms has been unrewarded. After a failure by myself, I placed samples of the slate in the hands of Professor Wormley, whose skill in microscopic researches is well known and whose instruments are of the most perfect kind. Thus far his search for distinct organisms has been unsuccessful. It is reasonable to suppose that the organisms contained no silica nor lime, and that in their decomposition and bituminization, all organic structure was destroyed."

Within the last year, I have discovered in the black shales of Ohio, several microscopic forms of vegetation which are found in the shales in vast numbers and which obviously constitute an important source, and, as I believe, the chief source, of the bituminous matter that characterizes the formations. A brief account of the facts, I desire to present at this time to the Association.

In the spring of 1881, I had some correspondence with Mr. J. A. Flickinger, ex-county surveyor of Ashtabula county, Ohio, in regard to a deep well, the drilling of which was in progress under his supervision, near the village of Kingsville, in the county above named. A result of the correspondence was that Mr. Flickinger kindly furnished to me the drillings that he had saved from different depths as the work had gone forward. Samples were saved every day or two and always when any change was reported in the materials through which the drill was passing. Fifty or more specimens were furnished, representing a range of between 1200 and 1300 feet.

The record that they gave was plain to read. The boring began in the Erie shale of Newberry and continued in it, with but minor changes of composition for nearly 1000 feet, where the greenish blue layers of the Erie were replaced by the black beds of the Huron. On examining with the microscope the fragments of the latter that were first reached, I found several of the samples, and notably those at 996, 1016 and 1094 feet to contain minute translucent disks, resinous in appearance and unmistakably organic in origin, in great numbers. Beyond this point, I made no progress for a number of months, until upon a microscopic examination of the black shale from Columbus, accidentally undertaken, I found it also charged with forms similar to or perhaps identical with those already recognized from the buried shale of northern Ohio. I then made examination of typical representatives of the several black shales already named throughout the state, the Huron shale from the mouth of Huron river, the Cleveland shale from Cleveland and other points in the vicinity, the Berea shale from the Ohio valley. I also examined the outcrops of the great black shale throughout central and southern Ohio, taking samples from every portion of the series, bottom, middle and top. The result of these various examinations was that the forms already recognized or forms of the same generic character, together with a number of minute but very interesting vegetable and animal fossils, were found distributed in vast numbers, sometimes constituting a notable proportion of the rock itself, throughout the entire extent of the Cleveland-Huron shale and to a smaller extent through the Berea shale. In some localities, every fragment of the shale is crowded with the fossils; in others, the products of their decay and partial decomposition are found readily recognizable, though the fossils themselves may be comparatively rare.

I will describe in a little more detail, the first of the forms to which I particularly refer.

They are, as I have already stated, *disks*, fairly circular in outline. They are translucent and sometimes quite transparent. In color, they are prevailingly amber-like, but they exhibit many shades of brown and yellow. They are readily separable from the rock and can be favorably examined by transmitted light. In size they extend through quite a range. The smallest that I have found in any large number, come from the very base of the Huron shale in northern Ohio and are about $\frac{1}{200}$ of an inch in long diameter. Their thickness is very small comparatively. The largest, coming from near the middle or the upper half of the Ohio Black Shale reach $\frac{1}{50}$ of an inch in diameter. Some, indeed, exceed even this measure and all these become readily visible to the unaided eye. Perhaps the most common form, found in various positions in the shale, is that which has a diameter of about $\frac{1}{100}$ of an inch.

The different sizes are not, as a rule, found in the same beds. Each bed which is well charged with them will contain a very large preponderance of one general size and character. I believe that the facts in regard to them will warrant specific divisions among the forms, and further I see some reason to think that the several forms will prove to some extent diagnostic of the different horizons of the shale.

These bodies burn freely, leaving the pits in the shale from which they disappear distinct and well defined. Though resinous in appearance, they are not soluble in alcohol, ether or turpentine, or other ordinary solvents. They also disappear when the shale containing them is raised to a red heat. A few exceptions have been noted in regard to one of the rarer forms.

Their surfaces are curiously ridged and furrowed as is shown by the bodies themselves or perhaps even more strikingly by the pits from which they have been withdrawn. Many of the latter show impressions and reliefs that resemble the track of a newly-shod horse.

What seems to be a suture or opening, occupying about one-fourth of the circumference is a common feature, and a pit for the attachment of a stem can be recognized in very many. In a few, rare instances, *the stem itself is found with the disks attached?*

These constitute the general and non-professional statements that it seems necessary to make. I submit a few drawings of the forms in question, made by one of my assistants, for the accuracy of which I can vouch. Better still than drawings, however faithful, are the objects themselves. I have a number of pieces of the shale which I shall be glad to distribute to any who care to examine them.

What are these forms and from what sources are they derived? A botanist must answer these questions when they are fully answered, as I believe. My own interpretation was substantially determined by the appearance that microscopic sections of the shale present, viz., yellow translucent circles, in sections parallel to the bedding, and elongated bars or much flattened hoops when cut transversely. These facts correspond so closely with the description of spore-cases in coals as given by Williamson and Huxley that I at first believed the forms to be lycopodiaceous sporangia. They did not, however, show the trigonal form that belongs to the spore-cases of most lycopods and certain facts that I afterwards discovered seemed to militate against this view. That they are either sporangia or spores, there seems no reason to doubt, but the facts so far accumulated would seem to indicate groups somewhat below lycopods in rank as their sources. Upon this point, I am not entitled to an independent judgment. Hoping that this interesting material may be taken up by a competent investigator, I await results.

I have spoken all along as if I were the original discoverer of these fossils of the shale. In the sense that I reached the facts without suggestion or aid from other sources, the discovery is my own, but when I came to correspond with our authorities on paleobotany, I learned from Dr. Dawson, and with very great satisfaction, that he had already had his eye upon these forms, that he had not only recognized but had figured and described, ten years ago, one of the most abundant of them. In an article entitled "*Spore Cases in Coals*," in the April number of the American

Journal of Science for 1871, the distinguished author figures and briefly describes a form that seems, from specimens which he has kindly furnished to me, identical with the one which I at first recognized, that came from 1000 feet below the surface in northern Ohio. This account is brief and I will give it entire.

"The oldest bed of spore-cases known to me is that at Kettle Point, Lake Huron. It is a bed of brown, bituminous shale, burning with much flame and under a lens is seen to be studded with flattened disk-like bodies, scarcely more than a hundredth of an inch in diameter, which under the microscope are found to be spore-cases, slightly papillate externally, and with a point of attachment on one side and a slit more or less elongated and gaping on the other. I have proposed for these bodies the name *Sporangites Huronensis*. When thin slices of the rock are made, its substance is seen to be filled with these bodies which, viewed as transparent objects, appear yellow like amber and show little structure except that the walls can in some instances be distinguished from the internal cavity and the latter may be seen to inclose patches of flocculent or granular matter. In the shale containing them, there are also vast numbers of rounded, translucent granules which may be the escaped spores."

"The bed at Kettle Point is stated in the report of the Geological Survey to be twelve to fourteen feet in thickness, but to what degree either in its thickness or horizontal extent, it retains the characters above described, I do not know. It belongs to the Upper Devonian, being supposed to be a representative of the Genesee shales of New York. It contains stems of *Calamites inornatus*, and of a *Lepidodendron*, obscurely preserved, but apparently of the type of *L. Veltheimianum* and possibly the same with *L. primœvum* of Rogers. The spore-cases are not improbably those of this plant, of the species of *L. gaspianun* which belongs to the same horizon though not found at this locality. The occurrence of this bed is a remarkable evidence of the abundance of lycopodiaceous trees, whose spores must have drifted in immense quantities in the winds to form such a bed. It is to be observed, however, that this is not a bed of coal, but a bituminous shale of brown color, and with pale streak, no doubt accumulated in water and even marine, since it contains *Spirophyton* and shells of *Lingula*. In this it agrees with the Australian Tasmanite which, though composed, in great part, of spore-cases of ferns is, as I am informed by Mr. Selwyn, an aqueous deposit, containing marine shells."

A number of other organic forms are associated with the spores in the black shales. In fact a flora and a fauna of considerable interest are coming to light in these hitherto neglected strata. They have long been known to contain *Lingulas* and *Discinas* and also *Conodonts*, whatever they may be. The bones, plates and teeth of fishes, large and small, have also been found in them, sometimes in great abundance and sometimes though rarely, of surpassing paleontological interest. It is below all of these groups that the forms to which I refer are to be found. Microscopic in size undetermined in character, but invested with both geological and paleontological interest, a considerable harvest awaits the careful student of these hitherto unrecognized forms.

I will not occupy the time of the Association with crude descriptions of such other forms as I have recognized, but leaving this branch of the subject, I will ask you to consider for a few moments, an important office that these minute bodies seem to have rendered and to be still rendering.

The office to which I refer is the production of the great accumulations of petroleum and gas that have been found in the Devonian and sub-carboniferous formations of Pennsylvania, West Virginia and Ohio written the last twenty-five years.

Thanks to the great work carried forward by the second Pennsylvania survey, we are coming to a pretty clear understanding of the geology of petroleum so far as its stratigraphical relations are concerned. This much we know, the great repositories are lenticular masses of sandstone and conglomerate, inclosed within masses of fine-grained shales, sealed up, as we may say, within them, but not quite hermetically. Below these oil sands, heavy deposits of shale, blue, red and black, are known to exist and at still greater depth the Devonian and Silurian limestones are found.

What are the sources of these great accumulations of hydrocarbons that have become in the last quarter of a century an important factor in the commerce of the world? There are but three answers to this question that so far as I know the geologists of our day have to offer. I will state them, but my limits forbid their discussion.

The first answer is that the petroleum and gas with which the oil sands are charged are indigenous to these sand and gravel deposits, formed within them from the decomposition and transformation of land or marine vegetation with which these old beaches have been charged. I scarcely know to whom to credit this theory, but Professor Lesley refers to it frequently with evident approval.

The second theory is that of Dr. T. Sterry Hunt and is to the effect that the animal matter of the great limestones and notably that of the great Devonian sheet, the Corniferous limestone, is an adequate source of these vast reservoirs of oil and gas. Dr. Hunt has made calculation of the amount of oil that could be furnished by such a bitumen-bearing bed as the Bridgeport limestone of Chicago of Niagara age.

The third theory is that of Newberry. At least, the answer to which I now refer has been presented by him in more detail and defended with more painstaking and ability than by any other, so far as I know. His first extended statement appeared in an article in the Agricultural Report of Ohio for 1869, under the title of "The Rock-Oils of Ohio." I will give some of the main points on which Dr. Newberry relies in establishing the last named theory.

He takes the ground that the great source of petroleum and gas in the field already named, is the bituminous matter of the black shales of which I have been treating. The points that he makes are the following: —

1. The hydro-carbonaceous matter required for such a supply is really present in these shales. Analysis shows them to contain from ten to twenty per centum of organic matter of this character, requiring comparatively little transformation to be converted into the bodies whose origin we are seeking. A ton of these black shales is made to yield in our retorts, from ten to twenty gallons of oil, as a matter of practical business.

2. A line of oil and gas springs marks the outcrop of these formations throughout their whole extent, showing unmistakably that they do give rise everywhere and always to these very products. In other words, the shales are undergoing spontaneous distillation by which these products appear wherever the formations themselves are uncovered.

3. The productive wells of the oil fields actually descend to or near to these shale horizons. When limestones hold oil, as the Corniferous of Canada, Dr. Newberry argues that below them still beds of black shale are to be found as the Utica for example. Wells bored below these Devonian black shales in Pennsylvania, even to the great limestone sheet, are comparatively unproductive. The Corniferous has been struck more than once in the deep wells of Pennsylvania and New York without any large returns.

This view has so much to recommend it that it is not surprising that it has won more general acceptance than any other. In the most complete accounts yet given of the geology of an oil producing district, viz., in the late reports of Carll of the Pennsylvania survey, this theory seems to be adopted as furnishing the best available explanation of the facts involved.

The facts to which I have called the attention of the Association at this time are in harmony with this view and may advance our knowledge by one step of the history of these products, but they do not touch the question in a vital or decisive way. Granted that the black shales are the source of petroleum, I have pointed out the particular element of the shales that is engaged in the work. The really vital fact was already held, in the knowledge that analysis has given us of the composition of the shales. It is their ten per centum of hydro-carbonaceous matter that takes the central place in the argument.

If these various *sporangites*, to adopt Dawson's generic designation, prove to be true sporangia, this fact, as I have said, carries us forward by a step at least in the way of accounting for the origin of petroleum and the other hydrocarbons associated with it. Such bodies, lycopodiaceous spores, for example, as Dawson and Hunt and others have shown, partake of the qualities common to most suberine and epidermal matter. Being extremely rich in hydrogen and carbon they approach resins and fats in composition more nearly than they do woody fibre. Hence they repel water — are not easily moistened and are able to resist decay in a remarkable degree. All of these facts bear upon their contribution to and preservation in the beds of coal and shale of which they form a conspicuous part.

Whatever their value shall be found to be in the production of oil and gas, I have shown by the facts here recorded that the bodies in question are universally distributed throughout the Devonian and sub-carboniferous bituminous shales of Ohio and, by fair inference, through all adjacent territory. Let me recapitulate the facts that bear on this question of distribution.

Dr. Dawson first found these *sporangites* on Kettle Point of Lake Huron, but expressly disclaims knowledge of either their vertical or horizontal distribution beyond the twelve or fifteen feet there exposed. They are next found 1000 feet below the surface in northern Ohio. They are rediscovered in the middle or upper division of the Ohio black shale at Columbus. Then by a more systematic search, as opportunity has offered, they are found in every portion of the Cleveland-Huron black shale of Ohio, from bottom to top and from Lake Erie to the Ohio river. They are also distributed throughout all the most characteristic portions of the Berea shale. I regret that I have not had opportunity as yet to examine the extension of these strata elsewhere, but it is not at all hazardous to assert that whoever looks for them in the Hamilton black shales of western New York, in the Devonian black shales of Michigan and Indiana and in the southwest extension of the Ohio black shale along the eastern border of the Cincinnati axis across Kentucky and Tennessee, will find them there in the same associations and conditions that have been here described. Dr. Newberry indeed has informed me that a number of years ago he recognized spores in the black shale of Kentucky.

RECENT INVESTIGATIONS AND PALÆONTOLOGICAL DISCOVERIES IN THE WAPPINGER LIMESTONE OF DUTCHESS AND NEIGHBORING COUNTIES, NEW YORK STATE. By WILLIAM B. DWIGHT, of Poughkeepsie, N. Y.

[ABSTRACT.]

THE Wappinger (or Barnegat) limestones of Dutchess and the neighboring counties, New York state, can be properly studied only in connection with the Poughkeepsie slates with which they are closely interstratified. Recent explorations in both the limestones and the slates, but chiefly in the former, have had rich results. These results are important for the following reasons:

(1) The age of these rocks has been hitherto very uncertain. Emmons classed them as Taconic. Mather, with the approval of Professor Hall, called the slates Utica and Hudson River, and the limestones Calciferous and Trenton. In later years, after a review of the region, Sir William Logan and Professor Hall have published the whole belt as "unfossiliferous Quebec."

(2) The question of the age of the Wappinger limestones and the adjoining strata, has an important bearing on that of the existence of the so-called Taconic system. There have been, in recent years, many investigations by eminent geologists into the true nature of the original typical Taconic rocks. On the whole, these have impaired seriously the theory in regard to them, held by

Emmons, if they have not destroyed it entirely, in general scientific opinion. Prof. J. D. Dana, after detailed personal observation, has recently, in the American Journal of Science, contributed weighty, and, as I believe, conclusive evidence of the continuity and identity of the slates and limestones of Dutchess county and their modifications, with the Taconic rocks of Emmons in western Massachusetts and Vermont. Palæontological evidence as to their age would therefore be decisive as to that of the Taconic strata of the above mentioned states.

This evidence, now ample, shows the so-called original Taconic strata to contain the Hudson river, Trenton, Chazy and Calciferous groups, and probably also the Potsdam sandstone. This result harmonizes with palæontological evidence furnished by A. Wing and other observers in the northern extension of the Taconic in Vermont.

(3) While conclusions concerning the Dutchess county rocks have been heretofore founded almost entirely on stratigraphical grounds, since 1878 abundant palæontological evidence has been found. In 1878, Mr. T. N. Dale found many Hudson river fossils in the slates west of the limestone belt. In 1878 and '79 Professor Dana discovered Trenton fossils at Pleasant valley and Rochdale, near Poughkeepsie, in the limestone. In 1879, Mr. R. P. Whitfield discovered the Chazy *Maclurea magna* in the continuation of the Wappinger limestone at Newburgh. In 1879, the writer, by invitation of Professor Dana, accompanied him and assisted in the discovery of the Trenton fossils at the localities above named. Since that date I have continued a most careful exploration of the limestone, with very rich results. I have been able to develop more fully the fossils in the Trenton, and also for the first time to find abundant evidence, by fossils, of the presence of the Calciferous group in extensive deposits. I have been able to trace out to a large extent the mutual relations of the Hudson river slates, and the Trenton and Calciferous limestones. It appears that they are disposed in at least three anticlinal folds, with many local complications. The general strike is about north 30° east (true), and the average dip about 70° easterly.

Partial metamorphism has generally obliterated and always affected the fossils, but a very close scrutiny has revealed a number of interesting fossiliferous localities. Of these, Rochdale, four miles northeast from Poughkeepsie, is by far the richest. I have found fossiliferous Trenton at different points from Salt Point, ten miles northeast of Poughkeepsie to about eighteen miles to the southeast of that city. Fossiliferous Calciferous appears as far to the north, but none has as yet been found farther south than a point five miles below Poughkeepsie.

The Trenton fossils found by Professor Dana at the time above mentioned were *Orthis tricenaria* and *testudinaria*, *Leptæna sericea*, *Strophomena alternata*, *Orthoceras junceum*, cyathophylloid corals, several crinoid species, and a very minute coral since identified by Professor Dawson as the *Chœtetes* (*stromatopora*) *compacta*, previously reported in Canada. Part of an *endoceras*, and a large species of *receptaculites* were also found. A fossil appearing to be *Orthis occidentalis* had also been found by him at Ancram.

My own explorations in the Trenton of the Wappinger valley have developed, in addition to the above fossils, *Orthis pectinella* and *lynx*, *Rhynconella capax*, *Chœtetes lycoperdon*, var. *ramosa*, *Petraia corniculum* (probably), *Illænus crassicauda*, a head and columns of *Echino-encrinites anatiformis*, columns of *Schizocrinus nodosus*, a new species of discinoid brachiopod which I have named *Orbiculoidea conica*, and many fragments of columns of a large *encrinite* of the Canadian "cleiocrinus" type, two centimetres or less transverse diameter. Some of these are too close to *C. magnificus* and *grandis* to be assigned to any other species, though the latter forms are sometimes much larger than the type specimen. Others present considerable variation from these types.

The most remarkable fossils are those which I have discovered in the Calciferous, chiefly at Rochdale. These comprise, of Calciferous fossils of general occurrence in the United States, *Ophileta complanata*, *sordida* and *levata*, and several species of fucoids; of well known Canadian fossils, *Ophileta compacta* and *Triplesia* (*Camerella*) *calcifera*. *Ophileta compacta* has previously been reported from a single locality in the United States, near Middlebury, Vt., by Mr. A. Wing.

There are also a large *holopea* resembling *H. proserpina*, and smaller ones not identified; *pleurotomariæ* resembling *P. Canadensis*, *quebecensis* and *gregaria* of Canada; a minute *ophileta*, new species; a *Murchisonia* resembling *M. gracilis* of the Trenton group; several other gasteropods not identified; one or two species of quite small and finely striated *orthides;* many undeterminable fragments of *Bathyurus;* also several specimens of *Chœtetes lycoperdon*, var. *ramosa*, well marked. The latter is not, I believe, hitherto reported below the Trenton.

Of *Orthocerata*, I have been surprised to find at least from *twenty-five to thirty species* in this Calciferous rock. Unless *Orthoceras primigenius* may be present, which is doubtful, and excepting *Oncoceras mummiforme* (Whitfield) which appears to be represented, all are new forms in the United States Calciferous. All have close septa, none having wider septa than nine to the inch, among hundreds of specimens collected. Most have from fifteen to thirty-five or more septa to the inch. The species vary in length from under two to twenty-three centimetres, and in breadth from under one to over four centimetres. Curved forms and lateral, mostly marginal siphuncles, predominate. There is also a predominance of large siphuncles of the endoceras type, but generally lacking the internal cone. There is much variety in the shape of the siphuncles and in the taper and shape of the shell. Some of these resemble, and may prove identical with, the Canadian species " Montrealense," " glaucus," and perhaps one or two others, but a large part appear to be new species. Two of these orthoceratites are evidently " endoceras," bringing this genus down to a low horizon.

One of the more common and beautiful species is from one to two inches long, with six to eight annulations, and twenty to forty septa to the inch.

Two species of *lituites* occur here, and a *cyrtoceras* with the marginal siphuncle on the side of the inner curve.

From the above statements it will appear that both the Trenton and Calciferous strata of this region differ greatly in palæontological character from other strata of those ages in the United States, while they resemble the strata of Canada, or of perhaps, some of the localities of the United States immediately bordering on Canada.

It would also appear that the Wappinger limestone, contrary to its received unfossiliferous reputation, is highly "fossiliferous," and that Rochdale, its most interesting locality, is the richest palæontological locality of the Calciferous in the United States, and one of the richest in cephalopodic life of any formation in the country.

THE WINOOSKI MARBLE OF VERMONT. By G. H. PERKINS, of Burlington, Vt.

[ABSTRACT.]

THE object of this paper was to call attention to the remarkable deposits of variegated marble which extend along the western border of Vermont for about thirty-five miles, from near Burlington north beyond St. Albans. Quarries have existed in some of the beds for thirty years; but recently more extensive operations than formerly have more than ever before developed the variety and beauty of the marble. The beds are primordial in age and form a part of the "Red Sandrock" of western Vermont. The marble is a dolomite and consists of magnesia carbonate about forty per cent, lime carbonate thirty per cent, silica ten per cent, and about the same amount of iron and alumina, but its constitution is very variable. It is not generally fossiliferous, but in some layers, *Salterella pulchella* Billings, occurs in patches and other indistinctly marked fossils have been found, but they are very rare. The predominant color is red mingled with more or less white; less common are green and drab tints. Of red almost every imaginable shade occurs and the variety which is produced by the mingling of these different shades with white and green or drab is wonderful. Over thirty well marked varieties are found within a short distance of the large mill recently built on Mallett's Bay about six miles from Burlington. All of these are handsome. In some the color is dark Indian red and from this the varieties pass to a light shell pink and these latter are exquisitely delicate and beautiful.

The supply is practically unlimited and so located on the bay, which is a part of Lake Champlain, that the derrick which lifts the blocks from the quarry can swing them around and deposit them in boats lying alongside by which they may be taken to the Hudson and St. Lawrence. Though harder and hence more difficult to saw and cut than common marble, the Winooski, or as it is now called the Wakefield, compensates for this by its durability, superior polish and general beauty, and it seems destined to prove of great value for wainscoting and other ornamental work in fine buildings. Specimens of some of the chief varieties were exhibited, and others described.

THE GREAT TERMINAL MORAINE ACROSS PENNSYLVANIA. By H. CARVILL LEWIS, of Germantown, Pa.

[ABSTRACT.]

AFTER describing the investigations in New Jersey, and along Long Island and southern Massachusetts, which first demonstrated the existence of a true terminal moraine, and referring to the discovery of similar moraines in Wisconsin and Minnesota, which marked halting places in the retreat of the glacier, the author stated that, having obtained the aid of the Geological survey of Pennsylvania, and, during a portion of his work, the assistance of Prof. G. F. Wright, he had been able to follow and define the southern limit of glaciation for the first time in a continuous line four hundred miles in length across that state, and to find that it was everywhere marked by a remarkable accumulation of glaciated material, which, winding across mountains and valleys, from the lowlands of the Delaware to the great Alleghany plateau, was continuous from end to end, and formed a true terminal moraine to the great ice-sheet that once covered northeastern America.

The method employed in discovering the line of the moraine was to zigzag along its course from the glaciated into the non-glaciated region, and *vice versa*, going each time far enough on the one side to be fully satisfied of the absence of glaciation, and on the other to find undoubted traces of its action.

He then spoke of the marked distinction between the glaciated portion of Pennsylvania and that region south of glacial action. Although the general topography of the two regions is alike, the varied superficial features due to glacial agencies, the far traveled and scratched boulders, the smoothed and striated rock exposures, the unstratified deposit of *till*, the many *kames*, and especially the numerous glacier-scratched fragments and pebbles,—all these deposits are in strong contrast with those south of the moraine, where all the gravels are stratified and the pebbles water-worn, where the rocks are never polished or striated, but, on the other hand, often decomposed to a great depth, and where, except near the seacoast, wide stretches of the more elevated regions are perfectly free from all drift.

Nowhere south of the line of the terminal moraine had the author found any traces of glacial action, all statements by other geologists to the contrary notwithstanding. When typically developed, the terminal moraine is characterized by peculiar contours of its own. A series of *hummocks*, or low conical hills, alternate with short straight ridges, and enclose shallow basin-shaped depressions, which, like inverted *hummocks* in shape, are known as *kettle holes.* Large boulders are scattered over the surface, and the unstratified *till* which composes the deposit is filled with glacier-scratched boulders and fragments of all sizes and shapes. The average width of the moraine is about one mile.

At many places, however, the limit of glaciation is marked merely by an unusual collection of large transported boulders. This is especially the case in front of a high mountain range, which has "combed out" the drift from the ice.

The general course of the moraine across Pennsylvania was defined as follows: appearing first in Northampton county, a mile below Belvidere, at latitude 40° 49′, it rises through the stratified drift as low gravel hills, which, winding up over the slate hills to the west, are soon developed into an accumulation of typical till, holding kettle holes and filled with boulders. Winding in a great curve first westward and then northward, it reaches the base of the Kittatinny mountain three miles east of the Wind Gap.

Ascending to the top of the Kittatinny mountain, 1600 feet high, the moraine crosses over it, being well shown upon the very summit, and entering Monroe county, crosses the great valley between the Kittatinny and the Pocono, enclosing in its course several moraine lakes. Having crossed this valley, and reached the base of the Pocono escarpment, it swings sharply back and around Pocono Knob, immediately afterwards to ascend the steep face of the mountain to the wide plateau on top, 2100 feet above the sea. Crossing this in a fine curve, and heaped up in an immense accumulation, it goes first north and afterwards west, until it reaches the Carbon county line.

Crossing the centre of Kidder township, Carbon county, it reaches the gorge of the Lehigh river, some ten miles north of Mauch Chunk. It crosses the gorge at Hickory run, and, without swerving from its general northwestern course, ascends mountain range after mountain range in Luzerne county, descends to the valley of the east branch of the Susquehanna, and crosses the river at Beach Haven, here forming immense heaps of drift, afterwards to be washed down the river into terraces.

Then, in Columbia county, following along the base of Huntington or Knob mountain for awhile, it finally ascends it, and crossing over the summit, at a height of 1500 feet above the Susquehanna just below, descends the north slope of the mountain to the broad undulating valley to the north. Taking a northerly course, it follows up on the east bank of Fishing creek to the North or Alleghany mountain. The summit of the Alleghanies in Sullivan county is covered with glacial striæ, and contains boulders and other marks of glaciation. The moraine entering Lycoming county, passes westward along the base of the mountain, crossing in its course the Muncy and Loyalsock creeks, and finally, near the village of Loyalsock, turns at right angles and ascends the mountain.

Having reached the summit of the Alleghanies, over 2000 feet above the sea, it crosses the picturesque cañon of Lycoming creek, and passing west through a wild, wooded region nearly as far as Pine creek, it begins a nearly straight northwestward course, through the southwest corner of Tioga county, and the northwest part of Potter. In the high ground of Potter county, the moraine crosses a great continental watershed, from which the waters flow into the gulf of Mexico, Lake Ontario, and Chesapeake bay. The moraine is here finely shown at an elevation of 2580 feet, being higher than elsewhere in the United States.

The line of the moraine now enters the state of New York, in the southwest corner of Allegany county. Passing still northwest and entering Cattaraugus county, it twice crosses the winding course of the Allegheny river, east and west of Olean; then trending to a point five miles north of Salamanca, in latitude 42° 15′, it forms a remarkable apex, whence to the Ohio line its course is southwest. Turning at right angles to its former course, the moraine passes southwest through the southeast corner of Chautauqua county, and, keeping approximately parallel to the course of the Allegheny river, reënters Pennsylvania, in Pine Grove township, Warren county. It crosses the Conewango river seven miles north of Warren, forming immense accumulations in the valley of the river.

Then trending west in Warren county, still at a general elevation of nearly 2000 feet above the sea, the moraine crosses one gorge after another, and forms a line separating, not only the glaciated from the non-glaciated region, but also the cultivated from the uncultivated and densely wooded region.

In Crawford county, the line appears in the southeast corner, and crosses Oil creek between four and five miles northwest of Titusville.

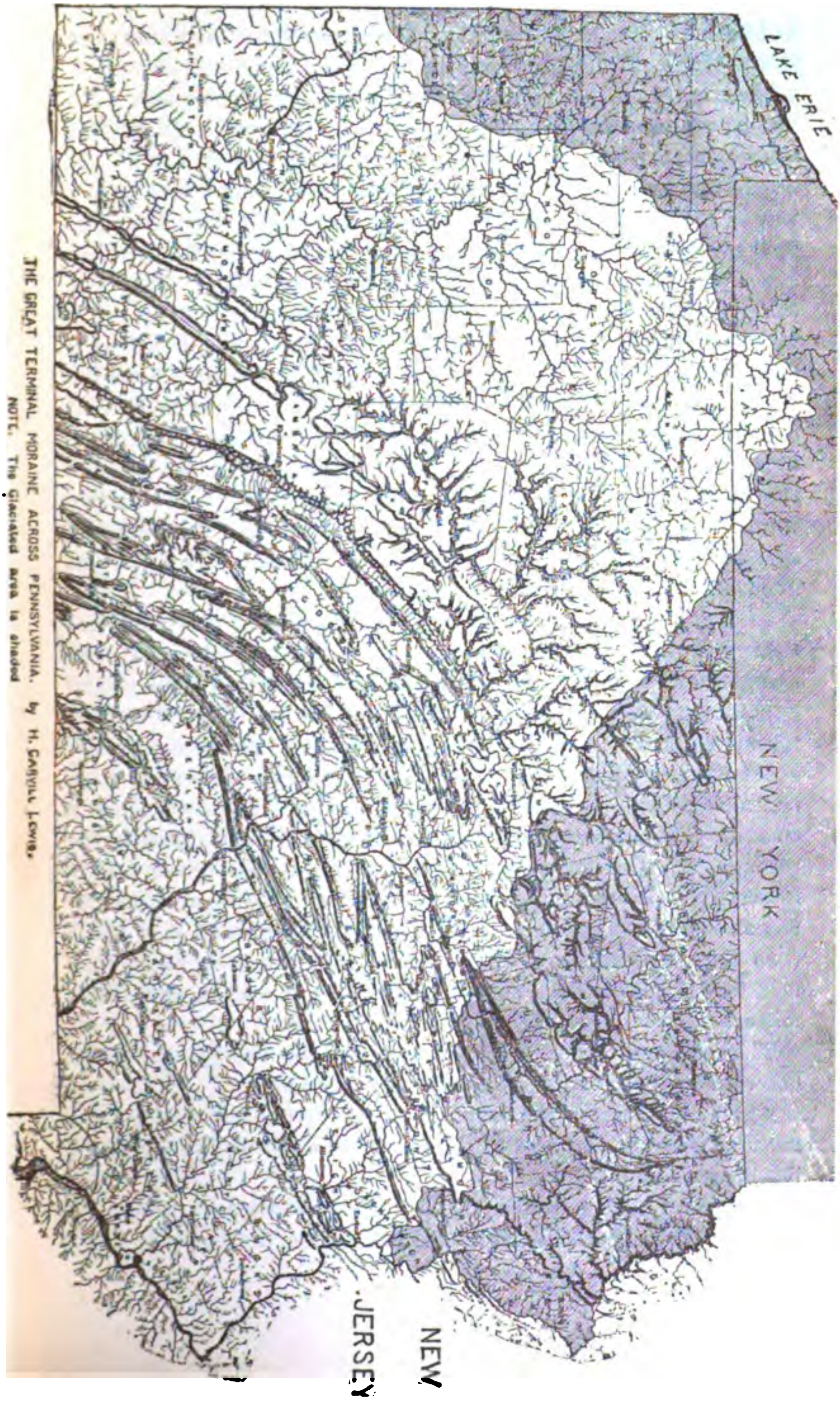
In Venango county it skirts the northwest and west boundary of the county, crossing French creek four miles west of Franklin.

It crosses the three northwest townships of Butler county, and the southeast corner of Lawrence. The Beaver river is crossed by the moraine eight miles south of New Castle.

The moraine traverses the extreme northwest corner of Beaver county, and, in the middle of Darlington township, thirteen miles north of the Ohio river, and at a latitude of 40° 50′, crosses the Ohio state line.

The moraine thus leaves Pennsylvania at precisely the latitude at which it entered the state, and if a straight line were drawn across the state between these two points, the line of the moraine would form with it a nearly right-angled triangle, whose apex was 100 miles distant perpendicularly from its base. The total length of the moraine, as here shown, is about 400 miles. The moraine crosses the Delaware at an elevation of 250 feet, the Allegheny at an elevation of 1425 feet, and the Beaver at an elevation of 800 feet above the sea, or 225 feet above Lake Erie. Upon the high lands it rises higher by 1000 feet or more.

Coming to the details of the moraine, the author spoke of the fine development of the moraine in Northampton county, west of Bangor, where it forms a series of hummocky hills, which, 100 to 200 feet in height, and covered with transported and striated boulders, rise abruptly out of a clayey plain to the west. Glacial striæ upon exposed surfaces near Bangor point southwest, or towards the moraine. After following the moraine to the base of the Kittatinny mountain, it became of great interest to know whether a great lobe of ice descended from New Jersey along the lower side of the mountain, or whether a tongue projected through the Delaware Water Gap, or whether the glacier even so close to its southern limit, came bodily over the top of the mountain, unchecked by it, and unchanged in its course. The last, the most improbable of these hypotheses, and certainly the least expected by the author, proved to be undoubtedly the true one. The author



THE GREAT TERMINAL MORAINE ACROSS PENNSYLVANIA. by H. CARVILL LEWIS.
NOTE. The Glaciated area is shaded

had been able to show that the moraine crossed the mountain near Offset Knob, that large boulders, derived from lower elevations several miles northward, lie perched all along the summit, 1400 feet above the sea, and that, as shown by the numerous striæ on the northern slope of the mountain, running *up-hill*, the glacier moved diagonally up and across the mountain, uninfluenced in any way by the presence of the Water Gap, and finally came to an end in the valley south of the mountain, as marked out by the terminal moraine. Huge boulders of fossiliferous limestone, sometimes thirty feet long, were torn by the ice from their parent strata in Monroe county, on the north side of the mountain, lifted up a thousand feet, carried across the mountain, and dropped finally in the slate valley of Northampton county. The author had found one of these limestone boulders upon the very summit of the mountain, where the jagged sandstone rocks had combed it out of the ice during its passage across. The journeys of these boulders were short, but that of a well-rounded boulder of Adirondack syenite, which the author had found in the same county, was about 200 miles.

In Monroe county, the course of the moraine as it winds from the top of the Kittatinny mountain down to Cherry valley and then up again on to the Pocono, is a complete vindication of the glacial hypothesis. It is in no sense a water level, nor could it have been formed by floating ice. No other cause than that of a great glacier could form a continuous accumulation of glaciated material, which contains no evidences of water action and which follows such a course. The fact that no tongues of ice were protruded either through the Delaware Water Gap or down the broad valley between the Pocono and Kittatinny mountains, indicates the immense size of the glacier. Although more than 1000 feet lower than the mountains and twelve miles in width, the valley last mentioned deflected the southern boundary of the ice but a few miles.

There are no striæ indicating passage of ice through the Water Gap, the supposed striæ being due to water action.

Again, neither on the mountains nor in the valley does the moraine rest against any defined barrier as would be the case were it a shoreline.

Of special importance in this connection are the two facts: (1) that, as shown by the absence of stratification, by the angularity and the striated surfaces of its enclosed stones, and by its topographical position, it has rarely been subjected to the action of water; (2) that, as proved by numerous glacial striæ and by transported boulders, its course is always at right angles to the direction of glacial movement.

The author stated that the moraine was wonderfully shown upon the summit of Pocono mountain, over 2000 feet above the sea, where a great ridge of moraine hills twelve miles long, one mile wide, and 100 or more feet high, composed of unstratified till, and bearing numerous boulders of Adirondack gneisses and granites, lies upon the level, sandy plain of the Pocono plateau and sweeps around from Pocono Knob into Carbon county. Known locally as "Long Ridge," its origin had never before been suspected. It encloses remarkable little "moraine lakes" without inlet or outlet, and is heaped up into just such conical hills as may be seen in the moraine in southern Massachusetts. Nothing can more clearly show the continuity and uniformity of action of the great glacier than the identity of its moraine accumulations at such remote points.

The author here described the *kames* of Cherry Valley, fine examples of which appear south of Stroudsburg, and are interesting relics of sub-glacial water action. They are composed of stratified water-worn gravel, having often an anticlinal structure, and, as a series of conical hills and reticulated ridges, enclosing "kettle holes," form conspicuous objects in the centre of the valley. They appear to have been formed by sub-glacial rivers, which flowing from the moraine *backwards*, under or at the edge of the ice, emptied into the Delaware valley. He thought that a study of the great subglacial drainage, of which kames are the most prominent relics, would throw much light upon certain high-level stratified gravels whose origin has been ascribed to great changes of elevation.

The author then described the more interesting glacial striæ of Monroe county. One of these, on the north side of the Kittatinny mountain, was six feet wide and seventy feet long. Near the moraine the striæ always curved so as to keep at right angles to it.

He spoke of the very slight effect that the glacier had upon the topography of Pennsylvania. It neither leveled down mountains nor scooped out cañons. The glacier passed bodily across the sharp edge of the Kittatinny mountain without having any appreciable effect upon it, the glaciated part of the ridge being as high and as sharp as that part south of the moraine. In describing the course of the moraine across Carbon county, he referred to the numerous lakes due to glacial action as being of two kinds:—moraine lakes, or kettle holes in the moraine holding water, and dammed up lakes, due to the obstruction of streams by the unequal distribution of the till. He spoke of the sharp distinction between the glaciated and the non-glaciated region on the Lehigh river, and referred to the striæ upon the summit of Penobscot Knob, which, at an elevation of 2200 feet above the sea point S. 10° W., this being the general direction of the ice flow. On the other hand, in the valley at White Haven, immediately below, the striæ point southeast, or along the valley. He spoke of the importance of separating the high level from the low level striæ, the former indicating the general movement of the top of the ice, which is uniform over large areas, while the latter conform more or less to the valleys and show mere local movements of the base of the ice. The glaciation of Penobscot Knob showed that the ice was at least 2000 feet thick at a point eight miles back of its extreme edge. Data were given, leading to the conclusion that the extreme edge of the glacier had a thickness varying from 400 to 900 feet.

In describing the course of the moraine across Luzerne county, the author showed that it crossed several mountain chains in succession, by each of which it was locally deflected northward.

He stated that at the point where the terminal moraine crosses Buck mountain, in a line diagonally across the mountain, the moraine was so sharply defined that he was able to stand with one foot upon the glaciated and the other upon the non-glaciated region. He described the fine kames between Scranton and Pittston, on the Lackawanna river, and showed that they were nearly parallel to the glacial striæ.

He then gave some details of the course of the moraine as traced through Columbia county. He stated that it was interesting to find that in *front* of a mountain chain, such as Huntington mountain or the Allegheny mountain, the moraine was poorly developed, as though the mountain had *combed out* the drift from the ice.

He described an instructive portion of the moraine, where, three and one-half miles northwest of Berwick, it seems to abut against a high slate hill, which furnishes, therefore, a *section* of the end of the glacier. It shows that the extreme edge of the ice was about 400 feet thick, and that while the moraine and the scratched pebbles were carried along at the base of the ice, sharp fragments of sandstone were carried on top.

The interesting course of the moraine along the eastern bank of Fishing creek was described, where the glacier stopped abruptly on the downward slope of a hill, stopping simply because its inertia became exhausted. Where the moraine crossed the creek, it formed a great dam, and it was shown that the moraine was often steeper at the back than at its front edge — a fact analogous with the features of modern Swiss moraines.

The evidences of glaciation upon the Allegheny mountain in Sullivan county were given, the striæ pointing south 9° west, and the moraine was followed along the base of the mountain to a point in Lycoming county near Loyalsock, where it climbed up to the great Allegheny plateau, and then, keeping at a high elevation, passed through Tioga and Potter counties into New York.

That this great region of high elevation (over 2500 feet) had a decided influence upon the general course of the moraine was inferred from the local influence already shown by the author to have been exerted by single mountain chains, and he thought it probable that as the lowlands along the Atlantic allowed the ice to flow as far south as Belvidere, so this great mountain region, acting like a wedge, caused the moraine to swing northward into New York; and so, too, the depressions of Lake Erie and the Mississippi valley produced another and more extended southward flow, a portion of which traversed the western part of the state.

Professor Lewis here described the remarkable apex made by the moraine north of Salamanca, N. Y. He showed that it was probable that the Allegheny river flowed under a tongue of the glacier, ten miles broad and two miles long, through a sub-glacial channel at the time of its greatest extension near Olean. He described a great natural dam across the valley of Great Valley creek, near Peth, where the moraine stretches across the valley from side to side, and he spoke of the contrast between the numerous drainage valleys which drained the waters of the melting ice into the Alleghany river, and those valleys which took their rise south of the moraine and were free from all drift.

After giving some details of the western lobe of the ice sheet in Pennsylvania, and referring to its agricultural significance, he spoke of some curious deposits of glaciated material which occurred in a narrow strip of ground immediately in front of the moraine, and which he had named the *fringe*. These deposits consisted of boulders of Canadian granite, and other rocks, which he found perched upon the summits of hills, sometimes as far as five miles in front of the moraine, though never farther. This glacial "fringe," confined to the western part of the state, was found to increase in width from two miles in Warren county, to five miles on the Ohio line, and was at first a puzzling phenomenon. The hypothesis proposed was that, like breakers on the seashore, the top of the ice overreached the lowest strata by the width of the "fringe," and that while the moraine marked the halting place of the bottom of the ice, by which it was formed, the far-transported boulders were carried on more rapidly in the top strata of the ice, and were dropped outside of the moraine to form the "fringe." It was stated that the striæ in the western part of the state all pointed southeast, being at right angles to those in the eastern part of the state, but like them, pointing always towards the moraine.

In conclusion, the author reviewed the more important facts discovered during his exploration of the line of the moraine, dwelling upon the character of the moraine where crossing river valleys, the absence of proof of any tongues of ice down such valleys, the absence of glacial drift south of the moraine, the very slight erosion caused by the passage of the glacier, and especially upon the deflections, large and small, in the line of the moraine, which were inexplicable on any other hypothesis than that the moraine now described was pushed out at the foot of a continuous ice-sheet of immense extent.

THE SILICIFIED STUMPS OF COLORADO. By ROBT. B. WARDER, of North Bend, Ohio.

[ABSTRACT.]

A GROVE of well preserved Sequoia Stumps at Florissant, Colo., about twenty-five kilometers W. N. W. of the summit of Pike's Peak, was visited by the author in September, 1872. Red granite abounds; and small outcrops of a trap-like rock are numerous.

A small and elevated basin was filled with shales and sandstones, which are still horizontal. Some strata abound in impressions of leaves, similar to the pine, oak, beech, and willow. The prevailing appearance resembles specimens from the Oeningen beds of Switzerland, exhibited in the Museum of Comparative Zoölogy at Cambridge. Some reed-like plants and fish were also found.

Twin Creeks (small tributaries of the South Platte river) have cut through this deposit, to the depth of ten meters or more, exposing the stumps.

The annual rings and medullary rays are well marked. Exposed surfaces are bleached; but many specimens are of a rich chestnut brown, or black, with every appearance of recent wood. The growths of a single year vary much in thickness; the extremes noted were about 1 mm., and 20 or 25 mm. The character of the grain varies much in different trees; sometimes it is straight, sometimes like curled maple, sometimes waved with beautiful regularity. No limbs could be distinguished; but there seemed to be gnarled roots, where the fibres were much confused. In some pieces, the fibres are bent nearly to a right angle, without being broken; in others, the jagged edge looks like brittle wood.

A singular coal-black mass, locally known as the "Carpenter Shop," seemed to be a sort of breccia of portions from 1 to 10 cm. in length, all closely united, with the fibre running in all directions. A few of the stumps are more completely altered, with subvitreous lustre, and red or dull yellow color. One of the least altered specimens lost five per cent of its weight on ignition, including water with a trace of organic matter. Chalcedony and drusy quartz crystals also occur. The former, if in botryoidal or mammillary forms, is commonly called "petrified gum."

These trees, including a specimen five or six meters in diameter, would indicate a milder climate than the present, for frosts are now common as early as September. The endogenous fossil wood found near Denver also supports this conclusion. Hot silicious springs may once have flowed from the volcanic rock; a process of silicification, now in operation on the Yellowstone, is described in Hayden's Report for 1871; but in the Colorado stumps the operation seems to have been far slower and more complete.

THE CHANGE OF RELATIVE LEVEL OF THE OCEAN AND THE UPLANDS ON THE EASTERN COAST OF NORTH AMERICA. By GEORGE H. COOK, of New Brunswick, N. J.

[ABSTRACT.]

AT a former meeting of this Association, in this place, in 1857, I read a paper upon this subject. My object in again bringing it forward is to call further attention to it — to the kind of evidence upon which it rests, to the effects of this change and to the means of controlling its injurious consequences.

1. Submerged forests furnish the most common and widespread evidence of this change. William Bartram in his "Travels through North and South Carolina, Georgia, East and West Florida," etc., in 1773, says: "It seems evident, even to demonstration, that those salt marshes adjoining the coast of the main and the reedy and grassy islands and marshes in the rivers, which are now overflowed at every tide, were formerly high swamps of firm land, affording forests of cypress, tupelo, magnolia grandiflora, oak, ash, sweet bay and other timber trees, the same as are now growing on the river swamps, whose surface is two feet or more above the spring tides that flow at this day; and it is plainly to be seen by every planter along the coast of Carolina, Georgia and Florida to the Mississippi when they bank in these grassy tide marshes for cultivation, that they cannot sink their drains above three or four feet below the surface before they come to strata of cypress stumps and other trees as close together as they now grow in the swamps." Lyell, in his second visit to the United States in 1845, writes of the coast of Georgia at the mouth of the Altamaha: "As we approached the sea and the brackish water, the wood bordering the river began first to grow dwarfish, and then, lowering suddenly, to give place entirely to reeds; but still we saw the buried stumps and stools of the cypress and pine continuing to show themselves in every section of the bank, maintaining the upright position in which they originally grew. The occurrence of these in the salt marshes clearly demonstrates that trees once flourished where they would now be immediately killed by the salt water. There must have been a change in the relative level of land and sea, to account for their growth, since, even above the commencement of the brackish water, similar stumps are visible at a lower level than the

present high tide, and covered by layers of sedimentary matter, on which tall cypresses and other trees are now standing."

On the coast of South Carolina, Professor Tuomey, in his Geology of the State, 1846, records similar facts regarding submerged forests, though he does not think their occurrence indicates any subsidence of the land. He acknowledges that Mr. Lyell has examined the coast of South Carolina and Georgia, and has come to the conclusion that these submerged stumps "show a vertical depression of the land to the amount of at least four feet." In North Carolina Dr. Emmons records the occurrence of submerged forests, in Albemarle Sound. In Virginia, along the shores of Chesapeake Bay, timber is found in the place of its growth, but several feet below the level of tide water, as I am informed by my friend, General R. D. Cutts, of the United States Coast and Geodetic Survey.

The coast of New Jersey is marked by the occurrence of timber and stumps below the present tide level in the marshes which border the State for much of the distance from the head of Delaware Bay to Cape May, and from the last named place to the mouth of the Hudson. In some of these marshes the timber is of white cedar, and has grown in the swamps and soft earth—notably so in the cedar swamps of Dennisville, Cape May County, and in the marsh along the Pennsylvania Railroad between Newark and Jersey City. But there are very many places where oak, maple, gum, yellow pine and other trees of upland growth are found. They are not on the parts of the marsh which are exposed to the action of the sea, but they are still covered by the marsh earth, which must have quietly and gradually advanced and inclosed them, and their stumps and roots are still sound and firm in the solid earth in which they grew. Examples can be seen in the meadows on Delaware Bay, below Salem—in many of the ditches in the marsh near the upland, which have been cut by farmers, everywhere—and a very marked example is to be seen in the yellow pine stumps, in the banks of the canal which connects South River at Washington, N. J., with the Raritan. (Reports Geol. Surv. N. J., and Amer. Jour. Sci., 2d Ser., vol. 24, p. 341.)

On Long Island, in New York, submerged forests occur under the same circumstances as in the marshes farther south. In *The Popular Science Monthly*, vol. 10, p. 434, E. Lewis, jr., records the occurrence of timber and stumps in the salt marshes at Islip, at Tiana Bay, Peconic Bay, Montauk Point, and Fort Hamilton.

In Massachusetts, Professor Hitchcock, in the Geology of that State, p. 307, under the head of "Submarine Forests," says "they consist of the remains of ancient forests now submerged a few feet below the sea, though sometimes laid bare at low water. . . . . . In the Harbor of Nantucket, as I am informed by Lieutenant Jonathan Prescott, portions of cedar, maple, oak and beech trees, some of them in an erect position and accompanied by peat of an imperfect character, were found. All the wood except the cedar, which was nearly as sound as ever, was very much decayed. They were buried by four feet of sand, and were about eight feet below low-water mark.

"Another submarine forest exists at Holmes's Hole, on Martha's Vineyard. It is on the west side of the harbor and was described by the pilot as having the appearance of a marsh at low water. Stumps have been found there in considerable quantity; of the cedar at least. Near the southwest extremity of the Vineyard, on the north shore, I was informed that another forest of a similar description may be seen. On the north side of Cape Cod, also, opposite Yarmouth, cedar stumps may be found (as I was informed by the captain of the Falmouth Packet), extending more than three miles into Barnstable Bay. And Henry Wilder, of Lancaster, who first directed my attention to this subject, says that the same thing occurs in the bay of Provincetown, on the side opposite the village. Further inquiries will no doubt bring to light other instances of a similar character; for my opportunities of observation on this subject have been but few."

In the "Geology of New Hampshire," by Professor C. T. Jackson, p. 280, J. L. Hayes, of Portsmouth, mentions that at Rye Beach "among the curious phenomena which may be witnessed, are the remains of a submerged forest. The stumps and roots of trees of a large size are frequently seen during very low water, on the lower margin of the beach. They appear to have been broken off near the roots, which remain in their original positions. The trees were mostly white cedar. . . . Stumps of trees are found in most of the salt marshes around Portsmouth."

In a paper on the Geology of Portland, Me., by Professor Edward Hitchcock and J. L. Hayes (Boston Journal Natural History, vol. 1, p. 338), similar circumstances in regard to submerged forests are mentioned.

In the Acadian Geology of Principal Dawson, page 28, is described a submerged forest at the head of the Bay of Fundy, near Fort Lawrence. He says: "At the distance of 326 paces from the abrupt edge of the marsh, and about 25 feet below the level of the highest tides, which here rise in all about 40 feet, I saw the first of the rooted stumps, which appear in a belt of sand, gravel and stones mixed with mud, which intervenes between the slope of mud already mentioned and the level of low tide. Beyond the stump first seen, and extending to a depth of at least thirty to thirty-five feet below the level of high tide, other stumps were irregularly scattered as in an open wood. The lowest stump seen was 135 paces beyond the first; and between it and the water level there was a space of 170 paces without stumps, but with scattered fragments of roots and trunks, which may have belonged to rooted trees broken up and swept away by the ice.

"On digging under and around some of the stumps, they were found to be rooted in a soil having all the characters of forest soil. In one place it was a reddish, sandy loam, like the ordinary upland of Fort Lawrence; in another place it was a black vegetable soil resting on a white sandy subsoil. Immediately over the soil were the remains of a layer of tough bluish clay, with a few vegetable fibres, apparently rootlets of grasses, which seemed to have been the first layer of marsh mud deposited over the upland soil. All the rootlets of the stumps were entire and covered with their bark, and the appearances were perfectly conclusive as to their being in the place of their growth.

"Of thirty or forty stumps which I examined, the greater number were pine (*Pinus strobus*), but a few were beech (*Fagus ferruginea*), and it is worthy of note that these are trees characteristic rather of dry upland than of lower swampy ground. The pine stumps were quite sound, though somewhat softened and discolored at the surface. The beech, on the other hand, though retaining much of the appearance of sound wood in the interior, was quite charred at the surface, and was throughout so soft and brittle that large trunks and roots could be cut through with a spade or broken with a slight blow. Owing to their softness the beech stumps were worn down almost to the level of the mud, while some of the pines projected more than a foot; even these last were, however, much crushed by the presence of the ice, which, with the tides, must eventually remove them. The largest stump observed was a pine two feet six inches in diameter and showing more than two hundred annual rings of growth. I was informed by respectable and intelligent persons that similar appearances have been observed on the opposite side of the La Planche, and in various other places in the Cumberland Basin. It is only, however, in places where the marsh is being cut away by the current that they can be seen, and the stumps, when laid bare, are soon removed by the ice. Similar beds of stumps and vegetable soil are also occasionally disclosed in digging ditches in the shallower parts of the marshes, and there appears little reason to doubt that the whole of the Cumberland marshes rest on the old upland surfaces."

The west coast of Greenland from about latitude 60° 43′ N. to Disco Bay near latitude 69°, appears from various signs and traditions to have been subsiding for the last four centuries. Ancient buildings on low, rocky islands, and on the shore of the main land, have been gradually submerged, and experience has taught the aboriginal Greenlander never to build his hut near the water's edge. The Moravian settlers have been obliged, more than once, to move inland the poles upon which their large boats were set, and the old poles still remain beneath the water as silent witnesses of the change. (Lyell's Principles of Geology.) The evidence in this case, however, is much more like that to be brought forward under the second kind of proofs of change of level.

2. Artificial structures and works of men furnish other evidence regarding this change of level. At Southampton, on Long Island, the first settlement was made by the English in 1640, and the land was laid out in farms and fields before 1650. One of the fields near the seashore and a little west of the village pond was fenced in at that early day. The fence posts must have been set in the meadow sod immediately behind the beach. All traces of this fence were lost sight of till within a few years, the sand beach having been driven inward so as to bury it entirely. But now after long and violent storms, which waste away the sand on the flat or storm beaches, the old posts and the meadows in which they were set are washed clean, and they are seen on the outside of the beaches, and near low-water mark. The meadow sod is still tough and firm, and the tracks of cattle and horses show as

plainly in it as they did when they were made, which was two hundred or more years ago. In that time the sand beach was moved inward its whole breadth or more, and the surface of that time has gone down several feet. The flat sands made by storm-tides are two or three feet above ordinary high water, and the several intelligent gentlemen, residents of Southampton, whom I met, and who had seen them at different times, judged that they were now covered with from four to seven feet of sand. Should another opportunity present, I am in hopes this change of level can be ascertained accurately.

The first settlement near Barnegat, N. J., was made as early as 1690; farms were cultivated on the fertile soils where they first located. They were only a few feet above the level of the salt meadows, which bordered them, and which are always at the level of ordinary high water. The first settlement, which really occupied the best of the soil, has long since been abandoned. The ground is now overgrown with salt grass, and the corn rows in which the fields were last cultivated can just be distinguished. The change of level here cannot be less than two feet, and it may be much more than that.

On the beach at the seaside about five miles below Barnegat inlet, there were settlements made by whale-fishers as early as 1690, and they had houses built, and kept animals on the beach and the adjoining salt meadows. There was also a timber swamp extending along on the inside of the beach for two miles or more. The settlement was long since abandoned, the sand of the beaches has been moved inward several hundred yards, the timber in the swamp is nearly all killed, and the stumps of the trees are some of them still to be seen standing in the flat storm-tide beach. On digging down to find whether they were in their original places of growth, I found them still in the swamp earth, with the bark and fibrous roots attached, and that the surface of the swamp earth was fully three feet below the level of the sand which embedded them, and as low as if not lower than ordinary high water — quite too low for cedar trees to grow now. Two or three persons who have been long familiar with the beach assure me that, for a long distance there, there is salt meadow sod to be seen projecting out on the strand and near low-water mark, — and on this they have seen the tracks of horses, cattle and sheep distinctly marked. These are only exposed after long and violent storms which waste off the sand and carry it out to sea.[1] And they are again covered with sand when the waves are less powerful and the roll of the ocean brings back the sand and piles it in the flat and steeper-sloped strand of common tides. Tracks would be covered and preserved by the wash of the ocean through glades or low parts of the beach, which would carry forward the sand and deposit it on the surface of the meadow, and the wind finally moves the whole body of the broken beach inward. These circumstances certainly indicate a change of level, and a rise of the sea upon the land within the past two hundred years.

The land on the eastern shore of Delaware bay was taken up, and settlements begun as early as 1650. In the early years succeeding islands of hard and timbered ground in the meadows were cleared and farm buildings erected on them, and the farms were cultivated for many years. Now these islands are covered with water in high tides and only marsh grass is grown upon them. This, too, indicates a vertical rise of the tide-water upon the upland, to the extent of a few feet within two hundred years past.

The consequences of this change in level are to be seen everywhere along the seashore, in its rapid and continuous wearing away by the action of the waves. From Florida to Greenland, as has been mentioned in the former part of this paper, facts recorded show that this is the case. In some instances four hundred or five hundred feet in breadth have been worn from the shore within the last forty years. The wear is so uniform and so consistent too, with the cause, that is, with the change of level, that it does not seem necessary here to cite cases. Another ill effect of the change is that of making the low-lying uplands into tide meadows and marshes. This change is gradual, if they are in wood; the trees die out slowly and no new trees grow — and if it is farm land it becomes less productive from imperfect drainage, the soil is soured, and finally its cultivation is unprofitable. Where the shores are bold these effects are not taken note of at all.

Navigation is improved by this change, the channels must deepen, and the waterways be easier for the passage of vessels — and this to some may be a kind of compensation for the damage which is done to others.

The rapid wear of the shores, and the changes produced by it, have made necessary in many cases the construction of expensive works to resist or divert the increased force of the waves. Sandy Hook has grown longer and narrower gradually for the last hundred years, and jetties have been built out on the sea side of the Hook to stop the wearing away of the land and to afford a lodgment for the sand that is moving with the current. The work appears to be successful. Similar plans were made for preventing the wear about the entrance to Charleston Harbor many years ago. Expensive works are projected to prevent the wear of the shore at Long Branch, and the damages there are so heavy that the protection must soon come. The changes about many of the lighthouses on the low seashores are owing to this cause, and the works for their preservation involve a heavy expense every year. When this change is accepted as a settled truth, and engineers study its consequences and effects, in the places where they are shown, then the proper works for obviating them can be best devised.

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[1] Such a storm occurred in Sept., 1882. and the tracks were found on solid and firm sod, about three feet below mean high-water mark.

It becomes an interesting subject of inquiry as to the cause of this change of level. Facts have not yet been accumulated to a sufficient extent to determine whether this change is local or general throughout the northern hemisphere. At present I am led to look upon it as general. The noted case of subsidence in southern Sweden, which is referred to in all our text books on geology, is of the same kind with those in our own continent.

In Scania, the most southerly part of Sweden, the seaport towns bear evidence that the land is there sinking. Streets originally built above high-water mark are now below it, and even old streets are found at a still lower level, showing that the subsidence has been in progress for a considerable time. A stone, the position of which had been exactly fixed by Linnæus, in 1749, was found in 1836 to be one hundred feet nearer the water's edge than it was eighty-seven years before."—Jukes, p. 332. Lyell, in his "Principles of Geology," gives a long list of cases on the coasts of England, Scotland, France, Holland, and along the Baltic, where the action of the sea is the same as on our coasts, and he gives also numerous cases of submerged forests similar to ours and occurring below the present tide level.

There has been considerable doubt thrown upon the fact of a general subsidence such as is here described, on account of the occurrence of sea-shells and buried timber, both of kinds now living, in earthy deposits which are a few feet above the present sea-level. Such cases do occur. I have observed them in New Jersey, and they are described by others as occurring in other places along the American sea-coast. But, however recent they may be, they belong to a period which preceded the present one of depression. The wood which is found is none of it so fresh as to be fit for timber or lumber as that of the present depression is, and the occurrence of the shells is more in alluvial sand or mud beds than any that is forming now, and the shells themselves, though of the species still living in the adjacent waters, appear a little different from the recent ones. The present depression may not have been going on more than 500 or 1000 years, and the indications are that there has been another period of depression in which the surface of the upland was ten feet or more nearer the sea-level than it now is. A single instance will illustrate this statement. The Maurice River is a tidal stream which empties into Delaware Bay. About fifteen miles above its mouth the river has cut into its eastern bank so as to expose an ancient oyster-bed, with its mass of undisturbed shells, still in pairs and all in their natural position. This bed is fully eight feet above ordinary high-water mark, and yet there is every indication that it has never undergone any disturbance except in the gradual elevation which brought it above the tide level, and is now lowering it again to its original place beneath the waters.

A careful study of the numerous cases like this will satisfactorily prove that there have been other periods of alternate depression and elevation in comparatively recent times, the phenomena of which are so nearly alike that they are very commonly confounded with each other. And when they are clearly distinguished, it will probably be found that the rise or the depression is one common to our whole coast and probably to the whole northern hemisphere. An accumulation of ice in the northern polar regions would be sufficient to move the centre of gravity of the earth in that direction, and thus to cause an increased amount of water in that hemisphere, and of course disturbance of the relative level of the ocean and uplands, and the apparent subsidence of the land.

ON THE CLASSIFICATION AND ORIGIN OF JOINT STRUCTURES. By W. O. CROSBY, of Boston, Mass.

[ABSTRACT.]

THIS paper is, in a large measure, an outgrowth of the articles by Mr. Gilbert and Prof. LeConte in the American Journal of Science for January, March and July, of the current year. I fully agree with Mr. Gilbert, that parallel jointing must be quite distinct in its origin, as it usually is in its characteristics, from both shrinkage cracks and slaty cleavage. Nevertheless, I cannot follow him in so restricting the definition of joint structure as to exclude shrinkage cracks, chiefly because such a classification appears impracticable. A very large proportion of the planes of division in both stratified and eruptive rocks are due to shrinkage, as is clearly recognized by all in the columnar jointing of basalt, and it is often impossible to distinguish the shrinkage cracks from those produced by other causes. Hence, if Mr. Gilbert's classification were adopted, it would be necessary to coin a new term to be used, in the very numerous cases where the origin of the planes of division can be determined. It seems better, therefore, to class shrinkage cracks as joints, and then to distinguish, where we can, between joints having unlike origins.

Broadening the definition of joint structure in the way here proposed, we are obliged to recognize at least three distinct classes of joints. These are, first, the short, non-parallel and non-intersecting joints dividing the rocks into irregular polygonal blocks and having their origin, as all geologists agree, in the contraction of the rocks, and hence properly called contraction or shrinkage joints; second, the joints near, and parallel with, the surface in granitic rocks, which have been satisfactorily explained by Prof. Shaler, as due to the expansion of the surface under the influence of the sun's heat, and which may, therefore, be appropriately named expansion joints. The third and most important class embraces the straight, parallel and intersecting joints. These have been attributed to shrinkage, compression, magnetism and an unexplained shearing force. The writer concurs with Mr. Gilbert in rejecting all these hypotheses, and advances the idea that earthquakes are the principal cause of parallel jointing.

An earthquake is a series of spherical elastic waves, the normal velocity of which, in solid crystalline rocks, as shown by the progress of slight tremors and sound, is 10,000 to 12,000 feet per second. But it has also been demonstrated by observation that the actual average velocity of important shocks is only about one-eighth as great, or, 1,200 to 1,600 feet per second. The generally accepted explanation of this enormous difference is that the high waves of the severe shock break the rocks, which are at best but imperfectly elastic, at every step, and are greatly retarded in consequence, while the low waves, passing successfully without rupture, run with the normal velocity of the medium. All parts of the earth's crust are affected by earthquakes, and since all important vibrations break the rocks which they traverse, the conclusion follows irresistibly that all geological formations (except, perhaps, the most recent, which are precisely those freest from joint structure) must be much broken by this agency. The question arises, then, where are these earthquake fractures? For obvious reasons, they cannot be regarded as cleavage planes; and the only alternative is to call them joints. Joints possess the required character of universality; but the earthquake fractures are clearly not contraction or expansion joints; and so we are driven to the conclusion that earthquakes are a principal cause of ordinary or parallel joints.

Earthquake fractures are necessarily parallel to the wave surface, or normal to the line of propagation. Therefore, they must be parallel to each other and usually appproximately vertical; agreeing exactly in these respects with parallel joints. Earthquake foci are usually situated along certain definite lines in the earth's crust, either lines of volcanic activity or of upheaval, plication and faulting. Hence it is reasonable to expect considerable constancy in the directions of earthquake movements and of earthquake fractures in limited districts, thus affording another close agreement with parallel jointing. Fractures produced by earthquakes moving in different directions must cross each other without interference, precisely as do the joints in different sets. Apparently, there is no important characteristic of parallel jointing which is not satisfactorily explained by a reference to earthquakes as a cause.

Elevation and plication must also be accompanied by fractures now and then; and these, it is easy to see, will be parallel, and will intersect without interference, when the strains are varied in direction. But earthquakes account for joints in rocks of all classes and conditions, in the unconsolidated and undisturbed Post-tertiary clays as well as in the ancient, crystalline and contorted gneisses, while the normal accompaniment of compression and plication is cleavage, and not jointing; although neither structure can be developed by these agencies without disturbing the stratification. Earthquakes are contrasted with all the other agencies referred to by their almost instantaneous action; and this peculiarity enables us to explain the remarkably regular jointing of some very coarse and irregular conglomerates; for these rocks break very irregularly, unless the force is applied with great suddenness.

I have elsewhere[1] pointed out that the irregular and ragged outlines usually exhibited by masses of coarsely crystalline eruptive rocks require us to believe that the joint structure does not extend to great depths, but is essentially a superficial feature of the earth's crust. And I have also shown that this view is perfectly consistent with the hypothesis that shrinkage is the chief cause of jointing.

My purpose now is to prove that the argument for the superficial nature of joint structure is strengthened, rather than weakened, when we substitute earthquakes for shrinkage as the principal cause of jointing. It is a well established fact that earthquake-foci are, in the main, comparatively superficial, and also that the rocks composing the earth's crust are much less solid and coherent, that is, much less elastic, near the surface than at great depths. But the number and extent of the fractures produced by an earthquake are inversely proportional to the elasticity of the rocks which it traverses; and just as certainly as the elasticity increases downward, the fractures (joints) must increase upward.

Hence, in conclusion, we may safely say that every theory of the origin of joint structure now entertained by geologists (and in a certain sense they are all true, some explaining one kind of jointing and some another) is consistent with the view that jointing is a superficial structure, the deep-seated rocks being essentially unjointed.

[1] Geological Magazine for Sept., 1881.

THE UNDULATIONS OF THE ROCK-MASSES ACROSS CENTRAL NEW YORK STATE.

By HENRY S. WILLIAMS, of Ithaca, N. Y.

[ABSTRACT.]

EXAMINATION of the dip of the rocks exposed in the counties of Chemung, Tioga, Tompkins, Schuyler, Seneca, Cayuga and Ontario has revealed a series of nearly parallel folds trending about twenty to thirty degrees north of east, decreasing in strength from the Pennsylvania line toward the northwest.

A north and south line from Ithaca, N. Y., to Ulster, Pa., shows the following folds:

About five miles south of Cayuga lake is the axis of a syncline, quickly rising into an anticline which crosses the Lehigh valley railroad just south of Newfield Station.

A second syncline crosses the same road between West Danby and Spencer stations, and the U. I. and E. R. R. between Swartwood and Van Ettenville. This seems to be the continuation of the Cowanesque syncline of the Pennsylvania survey, which is seen farther east, at Marathon in Cortland county, N. Y.

Near Van Ettenville there are evidences of a slight syncline in the top of the main anticline. The third syncline crosses the Lehigh valley road between Bingham's and East Waverly. The southern slope of this syncline, with northwesterly dip, appears at Chemung, Chemung Narrows, Waverly and the northern suburbs of Owego.

This is doubtless the continuation of Sherwood's Crooked creek syncline of the Pennsylvania survey.

From Ithaca going northward, the first syncline crosses Cayuga lake near Ludlowville, and Seneca lake at North Hector.

Slight folds are seen along both Seneca and Cayuga lakes farther north, but too slight to determine their axes with precision. The most northern anticline detected is near the foot of Canandaigua lake on the west side.

North of the Pennsylvania line the greatest dip is rarely more than three degrees.

The dips to the northwest are shorter and steeper than those in the opposite direction.

The folds are about twelve miles apart.

OCCURRENCE OF MAGNETIC ORE DEPOSITS IN VICTORIA COUNTY, ONTARIO. By W. HAMILTON MERRITT, of Toronto, Canada.

[ABSTRACT.]

IN going north from Lindsay, the primary rocks are met with, between Fetty's Bridge and Kinmount.

They here consist of granite, gneiss, syenite and crystalline limestone, with occasional bands of dioritic rocks; the latter are not so strongly developed here as farther east in the Madoc region.

There is a small amount of Labradorite rock or norite, which, however, has been observed at only one place.

Some thirteen deposits of magnetic ore have been located in this Victoria region, the most important of which were visited by me last year; there are numerous other untested occurrences. The ore bodies occur, as is usual with magnetic iron ore deposits, in beds more or less lenticular and of size varying from mere patches tc beds of from 40 to 50 ft. in width, and unproved length and depth, enclosed in and running with the country rock.

It is difficult in some cases to discover defined walls, as a layer of rock often comes in between two deposits of ore, or between the main body and a smaller one; and again in other cases there is a complete gradation from the one to the other. Crystalline limestone almost invariably in this district is associated with the magnetic ore deposits, which either occur in it or near its junction, with granitic or hornblendic rocks. Therefore in searching for iron ores in this region, it is well to keep in the vicinity of the bands of crystalline limestone. The ore body at the Paxton mine, though occurring in a syenitic gneiss, is associated with a thin band of crystalline limestone.

Hornblende is very intimately connected with the magnetic ore, and, especially at the above mentioned Paxton mine, one gives way to the other sometimes insensibly and again very abruptly throughout the extent of the workings.

Besides calcite and hornblende, other minerals such as augite, felspar, mica, iron pyrites, quartz and also specimens of olivine and serpentine were found occurring with the ores.

The magnetic ores of this district contain as a rule merely traces of phosphorus, but most of them require calcining to drive off the sulphur, though some, such as the Paxton ore, are almost entirely free from pyrites. Titanium is as a rule not present in a greater quantity than mere traces, though some deposits such as at Pine Lake carry a notable quantity. The metallic iron in the ore as it comes from the mine averages between 48 and 49 per cent and the selected has about 60 per cent iron.

The beds here, as in other well known localities, occur in defined belts, as for example a belt can be followed from lot 20 in the first concession of Snowdon to lot 30 in the fifth concession and probably farther.

The magnetic ore deposits of the Victoria region considered apart from any others, from their strike and dip with the rocks of the country, their similar crystalline character, their frequent insensible gradation of rock to the ore, and *vice versa*, and the similarity of their intercalated minerals, it would appear inevitable that they had been deposited with the rocks that contained them; either, according to Dr. T. Sterry Hunt, while during the eozoic times chemical activities, dependent upon greater subterranean temperature, different atmospheric conditions and abundance of thermal waters were at work, and therefore we might suppose local deposits of a varied character were going on; or, the neptunean view as ordinarily expounded, with its subsequent metamorphosis.

In the Geological Survey Report for Canada for 1873–74, Dr. Harrington shows how by heat the conversion of a bog ore was effected to a magnetite, having all the characteristics indicated by the name. As deposits of the lake bog ore are taking place at this present day in a manner which would seem to answer all requirements (with subsequent metamorphosis) for the formation of these magnetic ore beds, it seems natural that this theory finds many supporters.

The first developed deposit in this region, known as the Victoria or Old Snowdon mine, occurs in a wide band of crystalline limestone to the east of Kinmount. This ore bed is perhaps the most interesting in Victoria County, owing to its apparent strike, where opened, at an angle of nearly 45° to the general strike of the rocks of the country. It would seem, therefore, in contrast to all other deposits discovered in this region, to have been subsequently deposited in a fissure. The irregularity in the strike of this deposit may, however, when the country can be more thoroughly examined, be found to have been caused by a fault running the length of the valley, at the edge of which the deposit in question is being worked, for there is evidence of much disturbance where the rocks have been exposed.

The Victoria Railroad runs through this district and as large quantities of timber suitable for the manufacture of charcoal are in the vicinity, there is not a more favorably situated locality for charcoal iron smelting in America, and there will undoubtedly be important developments in this direction.

COMPARATIVE VIEW OF THE SUCCESSIVE PALÆOZOIC FLORAS OF CANADA. By J. W. DAWSON, of Montreal, Canada.

[ABSTRACT.]

IN Canada, we possess a very complete series of fossil plants extending from the Upper Silurian age to the Permian, and the object of the paper is to sketch out the characteristics of the several floras and sub-floras which have been studied..

I. *Carboniferous Flora.*

This presents three sub-floras in descending order: (1) That of the *Permo-Carboniferous* or Lower Permian, more especially seen in eastern Nova Scotia. It contains several common Carboniferous species, as *Dadoxylon materiarium*, *Pecopteris arborescens* and *Calamites suckovii*, associated with species peculiar to itself, as *Walchia robusta*, *W. gracilis*, *Calamites gigas* and *Cordaites simplex.* (2) That of the *Coal Formation*, which is the great headquarters of the Carboniferous flora, and is especially rich in *Sigillaria* and its allies, and in ferns. One hundred and thirty-five species of plants have been catalogued from this formation. There are nineteen species of *Sigillaria*, about the same number of *Lepidodendron* and allied genera, fifty ferns and thirteen *calamites* and allied forms. (3) The *Millstone Grit* sub-flora. Here we have a limited number of species, precursors of those of the coal measures, and described in a special report of the author published by the Geological Survey of Canada. *Dadoxylon Acadianum* is a very characteristic conifer of this period. (4) The *Lower Carboniferous* sub-flora. The number of species here is small and many of them peculiar, this formation being separated by the marine limestones from the Millstone Grit, and on the other hand being unconformable to the Erian or Devonian below. *Dadoxylon antiquius* is a characteristic pine of the period, and *Lepidodendron corrugatum* and *Aneimites Acadica* are very common.

II. *Erian Flora.*

Here we have three sub-floras, all markedly distinct from those of the Carboniferous. (1) The *Upper Erian* sub-flora is characterized more especially by ferns of the genera *Archæopteris* and *Cyclopteris*. *A. Jacksoni*, *A. gaspiensis* and *C. obtusa* are very abundant forms. The flora is similar to that of the Catskill of New York and the Kiltorcan beds in Ireland. (2) The Middle Erian sub-flora, corresponding to that of the Hamilton and Chemung groups in New York, is very rich in species. It contains several coniferous trees of the genus *Dadoxylon* and a great number of beautiful and delicate fern fronds, including sphenopterids and *Hymenophyllites*, as well as such ferns as *Megalopteris*, *Cyclopteris* and *Archæopteris*. *Lepidodendron Gaspianum* is very characteristic, and there are species of *Psilophyton*. *Sigillaria* scarcely appears, but there is an abundance of *Cordaites* (*C. Robbii*). The remarkable aquatic plants of the genus *Ptilophyton* occur here, and there are beds filled with Macrospores or "*Sporangites*." (3) The Lower Erian sub-flora, which is specially characterized by the genera *Prototaxites*, *Arthrostigma* and *Psilophyton*. These plants are often very abundant, but belong to few species.

III. *The Upper Silurian Flora.*

This is limited in Canada to *Prototaxites*, *Psilophyton* and the rounded fruits of the genus *Ætheotesta*, equivalent to *Pachytheca* of Hooker. These occur in rocks attributed to the Lower Helderberg group, and no undoubted land plant has been found in any older formation.

The above mentioned sub-floras can be readily recognized by a competent palæobotanist; and, as the author has shown in reports published by the Geological Survey of Canada,[1] can be correlated with those of the United States and Europe; but any more minute subdivisions are of merely local value.

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[1] Report on Erian or Devonian and Upper Silurian Plants of Canada, 1871. Report on Plants of the Lower Carboniferous and Millstone Grit, 1873. Report on Erian or Devonian Plants of Canada, Part II, 1882.

ON THE ORIGIN OF JOINT CRACKS. By H. F. WALLING, of Washington, D. C.

[ABSTRACT.]

MR. G. K. GILBERT, of the U. S. Geological Survey,[1] has observed in recent and nearly undisturbed clay beds of the former "Lake Bonneville" in Utah, a jointed structure in two directions nearly at right angles with each other. In this case, as he remarks, it is difficult to conceive that powerful compressive forces have been exerted in directions normal to those of the joint planes, thereby producing the jointed structure in a manner similar to that by which slaty cleavage is supposed to have been caused.

Nor does Mr. Gilbert accept the explanation offered by President Le Conte,[2] who, having observed shrinkage cracks in some recent alluvial deposits of California, attributes jointed structure to a similar process of contraction. Shrinkage of strata held by adhesion to their supporting beds, produces a prismatic structure in which the planes of separation are seldom continuous beyond the individual prisms. In jointed structure, on the contrary, the planes of separation are continuous, those of different systems intersecting each other but exhibiting no mutual relations. Regarding the reference to magnetic or to shearing forces for the production of jointed structure as unworthy of consideration, Mr. Gilbert considers the problem an open one.

It is clear that the force which has produced parallel joint cracks must have a definite direction over the wide areas where the parallelism prevails. It seems probable, moreover, that it must have acted with a suddenness, differing from the steady, long continued strains which slowly bend the strata into curved folds. Such a force is supposed by the writer to come into action when sufficiently powerful earthquake waves attend the production of faults.

Assuming that the materials of which the earth's interior are composed are in a sufficiently plastic condition, at least near the surface, to maintain a general equilibrium of pressures, it is evident that when this equilibrium is disturbed by the transfer of materials from continents to ocean floors, the inequalities of pressure will tend to move the more plastic substratum which supports the overloaded ocean beds to positions under the lightened continents. The sinking of ocean beds and rising of continents will bend and sometimes fracture the more rigid crust above. Fractures or faults produced in this way will be longitudinal or in the general direction of the axis of denudation, and since the corrugation or folding of strata is normal to the subterranean return flow of plastic material which produces it,[3] by gradually diminishing the width of the belts supporting the upper strata, the axes of denudation and corrugation will be in the same general direction.

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[1] Am. Jour. Sci., Vol. XXIII, June, 1882, and Vol. XXIV, July, 1882.

[2] Am. Jour. Sci., Vol. XXIII, March, 1882.

The sudden sinking, at the time of fracture, on one side of the fault and rising on the other, originates an earthquake, which is a wave movement outward from the point, or in this case *line*, of disturbance. On and near the surface the wave movement becomes not only a succession of rarefactions and condensations, but a progressive, up and down movement like that of ocean waves. The actual amount of bending in cases of exceptionally powerful earthquakes has sometimes been so great as to cause the ground to visibly open and shut in fissures, sometimes even engulfing and closing in upon men and animals.

Here then we have a sudden wave force, competent to rupture brittle and inextensible materials like rocky strata and clay beds in lines or planes parallel to its origin. Moreover, the sinking on one side and the elevation on the other would be likely, while the originating cause continues in action, to augment from time, especially in the portions of the fault midway between its ends. This would give rise to lateral or transverse bendings and fractures with accompanying transverse waves. In this way two sets of joint cracks nearly at right angles to each other might occur.

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[3] See Proceedings Am. Assoc. for the Adv. of Science, St. Louis meeting, 1878, p. 179.

ON THE ASSOCIATION OF CRYSTALS OF QUARTZ AND CALCITE IN PARALLEL POSITION. By R. B. HARE, of Guelph, Ontario, Canada.

[ABSTRACT.]

THE association of crystals of quartz and calcite in parallel position will be illustrated by specimens taken from the Grube "Reicher Trost," Reichenstein, Silesia, Germany. The specimens show the almost perfect similarity of the Reichensteiner and Schneeberger occurrence and defend Eck of Berlin, against von Rath and Frenzel.

TITLES OF OTHER PAPERS READ IN SECTION E.

THE EOZOIC ROCKS OF CENTRAL AND SOUTHERN EUROPE. By T. Sterry Hunt of Montreal, Canada.

THE SERPENTINES OF ITALY. By T. Sterry Hunt of Montreal, Canada.

NOTE UPON THE GENUS PLUMALITES. By James Hall of Albany, N. Y.

ON THE RELATIONS OF DICTYOPHYTON, PHRAGMODICTYUM AND SIMILAR FORMS WITH UPHANTAENIA. By James Hall of Albany, N. Y.

THE FELSITE-TUFA OF COLORADO. By Alexis A. Julien of New York, N. Y.

THE DUNYTE BEDS OF NORTH CAROLINA.[1] By Alexis A. Julien of New York, N. Y.

THE GENESIS OF THE CRYSTALLINE IRON ORES OF NORTH CAROLINA AND NORTHERN MICHIGAN.[2] By Alexis A. Julien of New York, N. Y.

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[1] Published in full in the Proceedings of the Boston Society of Natural History, 1883.

[2] Published in the Proceedings of the Philadelphia Academy of Natural Sciences, 1883.

THE COMPARATIVE STRATIGRAPHY OF THE CRYSTALLINE ROCKS OF NORTH CAROLINA AND CANADA. By Alexis A. Julien of New York, N. Y.

ARCTIC EXPLORATIONS IN NORTH AMERICA. By John Rae of London, England.

THE TOPOGRAPHY AND GEOLOGY OF THE GREAT SALT LAKE VALLEY. By William Bross of Chicago, Ill.

NOTE ON THE FAUNA OF THE CATSKILL RED SANDSTONE. By E. W. Claypole of Yellow Springs, Ohio.

ON AMPHICOELIA CEDARVILLENSIS FROM THE NIAGARA GROUP OF CEDARVILLE, OHIO. By E. W. Claypole of Yellow Springs, Ohio.

NOTE ON THE EXTERIOR MARKINGS OF BARK OF LEPIDODENDRON CHEMUNGENSE. By E. W. Claypole of Yellow Springs, Ohio.

A ROCKING STONE IN NEW YORK CITY. By Chas. Hallett Graham of New York, N. Y.

FRESHWATER LIGNITIC SERIES OF BEDS IN THE CRETACEOUS FORMATION OF FRANCE. By D. W. Kowalevsky of Moscow, Russia.

ON THE SURFACE LIMIT OF THICKNESS OF THE CONTINENTAL GLACIER IN NEW JERSEY AND ADJACENT STATES, WITH NOTES ON GLACIAL PHENOMENA IN THE CATSKILLS. By John C. Smock of New Brunswick, N. J.

SOME MOOTED POINTS IN AMERICAN GEOLOGY. By J. S. Newberry of New York, N. Y.

GENESIS OF NORTH AMERICAN FLORA. By J. S. Newberry of New York, N. Y.

SUGGESTIONS AS TO THE HISTORY OF THE LOWER COAL MEASURES OF OHIO. By Edward Orton of Columbus, Ohio.

THE CAVES OF STAFFA. By F. Cope Whitehouse of New York, N. Y.

SECTION F.

BIOLOGY.

ADDRESS

BY

WILLIAM H. DALL,

VICE PRESIDENT, SECTION F, BIOLOGY.

MEMBERS OF THE SECTION OF BIOLOGY:—

IN compliance with the custom of the Association, your chairman might have made use of this occasion to review the progress for the past year in that department of Biology which is his preferred field of research. Such a review would have had interest chiefly for specialists in Malacology, now, alas! only too few in number, and it has occurred to me that the opportunity might be better utilized in another way. I propose, therefore, to take a brief general review of what has been accomplished in America toward a knowledge of the biology of its mollusks; and, in so doing, to point out certain directions in which the earnest student may find a field ripe for the harvest, and where at present laborers are either wanting altogether or unfortunately few in number. In this way, perhaps I may be able to gain some recruits for this regiment of the scientific army, from among those young and ardent students who have their work, for the most part, still to do, and who have not yet made a choice among the many peaceful and attractive paths which Nature opens to those who would know her and her ways.

The study of mollusks was at first æsthetic rather than scientific, and to this day its popularity is chiefly fostered by the satisfaction of the sense of beauty derived from a contemplation of the exquisite shells produced by some of the members of this branch of the animal kingdom.

An almost purely artificial classification for shell-bearing mollusks was invented by the father of Natural History, Linnæus, which could be mastered by the least scientific mind in a few easy lessons. So convenient did this prove for collectors and others who wished merely to index as easily as possible their cabinets of shells, polished and duly marshalled in pairs, that, when Cuvier and the immortal Lamarck reformed the classification and for the first time created a science of Malacology, opposition was decided and long continued. The early students, seeking to know the relations of animals which were huddled in a few heterogeneous genera by Linné, were the subject of no little opprobrium from the conchologists of that day, and were very generally looked upon as dangerous radicals and unsafe guides. The intercourse between the students of various countries was incomparably less in amount and much more expensive than at present, and various naturalists pursued similar studies unknown to each other, whence arose great confusion in nomenclature which has not yet ceased to trouble the student.

In this condition was the science when Thomas Say, the pioneer conchologist of America, began to publish his studies on North American mollusks. The first scientific article on the shells of this country, published here, was the article on Conchology by Say in the American edition of Nicholson's Encyclopædia printed in 1816.

The determination of species is naturally the first requisite in the investigation of a local fauna, and the work of our earlier naturalists was largely descriptive from the necessities of the case and the state of science at the time. Much had been previously done by foreign naturalists but in an unsystematic way, and for a good many years American students were occupied in the description and iconography of our native mollusks as an inevitable preliminary to the study of geographical distribution and other more philosophical branches of the subject. It is unnecessary here to trace, even did space permit of it, the gradual progress in this direction, to which naturalists in all parts of the country contributed.

At present the molluscan fauna of the land and fresh water shells of North America is remarkably well known, only the older parts of Europe presenting a parallel to it. Something still remains to be done in the southern states, especially in the more mountainous portions, but as a whole we can hardly expect that the addition of new species will more than equal in number the reduction in our specific lists, which a greater familiarity with already named forms may render necessary.

With marine forms the case is somewhat different. The fauna of Canada and of the eastern coast of the United States north of the Chesapeake has been worked out with great thoroughness by the energy of resident naturalists, among whom it may not be invidious to mention Say, Lesueur, Gould, Conrad, Mighels, C. B. Adams, Stimpson, Couthouy, Dawson, Whiteaves, Smith and Verrill, as especially eminent. On the western coast where a much richer fauna prevails, the labors of Carpenter, Cooper, Stearns, Gabb, Hemphill and others have given us a pretty complete knowledge of the species from California to Sitka. For the region still farther north, in addition to the labors of Middendorf, large material, in part unpublished,, has accumulated in my own hands chiefly through investigations made in person and with the assistance of my companions in explorations in Alaska and on the Arctic coast. With the exception of the Alaskan coast the mollusk-fauna of Arctic America is well known, and new species hardly to be hoped for by the most energetic explorer of the litorale.

The exploration of the southern coast, especially that of the Gulf of California, is much less thorough and indeed from Georgia round the peninsula of Florida to Galveston, there is hardly any locality which would not repay a careful search. Nearly every such attempt results in new discoveries. The question is somewhat complicated by the inchoate state of our knowledge of the West Indies, whence multitudes of marine shells have been described, but in a scattered way and often without (or with an erroneous) statement of habitat. Our knowledge here has not progressed far enough to permit of tenable deductions in regard to geographical distribution and a wide field is open for research.

The study of geographical distribution is one of the most fascinating, far-reaching and important branches of biology. By its help problems of world-wide significance find solution; but it requires a thorough knowledge of the forms involved, a uniformity in the use of nomenclature, and some acquaintance with the palæontology of the later tertiary strata. Without these, sound conclusions cannot be arrived at; and hypotheses, like inverted pyramids, are in perpetual danger of complete overthrow.

In this connection I may refer to a field in which no one is systematically at work and which seems to me to promise rich reward to patient and thorough investigation. Within a comparatively short period many lake basins and fresh water deposits have been explored for mollusks recent and fossil. Among these may be mentioned sundry Dalmatian lakes, Lake Baikal, Lake Tanganyika, the lakes of Nicaragua, the Colorado basin and the Laramie beds of our western territories. I have long wondered at the curious repetitions of form and ornamentation which occur in certain *Unionidæ* or fresh water mussels from Burmah, Pegu, Lake Nicaragua, our western Tertiary beds, and last of all are now reported from Tanganyika. *Camptoceras*, originally described from a living species in India, is now reported fossil in England. *Melanopsis*, now not living nearer than Spain, has lately been discovered by White fossil in the Laramie beds; and, as in New Zealand, associated with a slipper-limpet which appears to belong to the genus *Latia*. *Pyrgulifera* Meek, from the Western Tertiaries, offers no characters by which it may be distinguished from Smith's Tanganyika *Paramelania* more recently described. Several forms described by Neumayr and others, from ancient European lake basins, seem nearly identical with American fluviatile genera. In spite of the opinion of the eminent French editors of the Journal de Conchyliologie (biassed a little I fear by the enormous distance intervening), I can see no valid characters by which the shells of a whole group of living mollusks (*Baikalia*) from Lake Baikal can be distinguished from our fossil *Tryoniæ* of the Colorado basin.

These facts, if confirmed as I think they would be by more thorough comparison and research, must have a deep significance. Who will devote himself to the solution of the problem?

Another line of investigation, closely connected with the geographical distribution of species, has recently been taken up with rich results. I refer to the exploration of the bed of the deep sea. Its relations are American rather from the fact that its initiation began under the auspices of the Coast Survey of the United States, and that its latest methods are largely due to the inventiveness of American naval and Fish Commission officers, than from the results of the work itself, which are world-wide in their application, and to which various nations, especially the English, have contributed in no stinted measure.

It was thought when deep sea dredging was in its infancy that the unexplored depths, especially in tropical waters, might shelter lingering representatives of distant geological epochs, and that ammonites and trilobites might put in an appearance when search was made in proper quarters. Indeed, some naturalists went so far as to assert that the fauna of the deep sea was essentially that of the cretaceous period which had existed there uninterruptedly from past ages. These ideas have been swept away by actual research and while we cannot be said to know more than a small proportion of the abyssal fauna, we know enough of it to be sure that it is essentially of a recent character. The survival of some forms which once were supposed to exist only in a fossil state does not materially alter the modern character of the assemblage as a whole.

The knowledge of the conditions which exist over a great part of the sea-bed, and of the topographical features of the bottom, has however greatly modified the accepted views of geographical distribution. We now know that the conditions are such that there is no evident reason why a deep-sea species might not extend its range from the Antarctic Sea to the North Polar regions, and in fact very nearly such a range has been made out for a few forms. The free access of Antarctic cold waters to the northern ocean areas has even led to the suggestion that the Antarctic is the great centre of distribution from which species of all shores may have originated. However true this may be of some branches of the animal kingdom, our knowledge of the mollusca hardly warrants so general a deduction. A certain portion of our fauna, however, may be thus derived.

We know that at a relatively not very distant geological period the conditions of life on the globe were much more uniform than now. Both marine fossils and vegetable remains in the boreal and Arctic regions show that a much milder climate once prevailed there, while if we assume a relative disparity as great as now exists between arctic and tropical conditions at that time, we should have a heat in equatorial regions fatal to most life. Hence it is certain that a greater uniformity in conditions and in faunal characters once prevailed, and we cannot but believe that our present fauna is the descendant with modification of that ancient one. Certain factors have so influenced the climate of the globe that that uniformity has been lost. Yet it would seem probable that the inhabitants of different coasts mainly originated not far from where we now find them, though important exceptions to this rule doubtless exist. The region which has in later times exhibited the most striking contrast of conditions of climate and sea level to the former general uniformity is the Arctic. It is geologically inferrible that this change has been, not slow and constant in one direction, but more or less fluctuating. The Arctic mollusks lend probability to this view for they are almost without exception characterized by wide specific variability; and the genera to which they belong are known as particularly variable even in those species which are not Arctic in their habitat. As the change in environment acted on the ancient fauna, one effect would be to exterminate such species as least readily accommodated themselves to changed conditions and to preserve a fauna of rapid and facile adaptability; perhaps mingled with a few forms particularly indifferent to influences of environment. These last must however in a large fauna always be exceptional.

This is the actual character of the Arctic mollusk-fauna and such features are only less remarkable in that of the Antarctic, because the latter region is almost destitute of coasts and in deep water such fluctuations of climate and sea level as have been referred to would be less readily transmitted and would lose most of their effect.

Other reasons why we may suppose that the present coast faunæ originated in the vicinity of their habitat are afforded by the results of a study I was enabled to make of the relations between the litoral and the immediately adjacent abyssal fauna in the West Indies from the collections of the Coast Survey steamer Blake, made under the direction of Prof. A. Agassiz.

These showed that more than ten per cent of the deep water species were also common to the litorale while a third of the genera were common to both shore and adjacent sea-bed. Only about one and a half per cent were distinctively arctic forms, such as we might expect to be in a large majority were the fauna derived from the North or South Polar Seas exclusively. In the same way we may explain the occurrence of *Voluta* and *Nacella* in the Aleutian Islands, rather as a survival from the Crag period when *Voluta Lamberti* flourished in Britain, than as offshoots from the Patagonian fauna which have travelled northward and left no progeny by the way.

These somewhat crude and perhaps premature generalizations indicate the nature of some of the problems of which a study of abyssal life may afford a solution.

Studies of the embryological stages of a few of our American mollusks have been made in this country and creditable contributions to this branch of the subject have appeared almost every year for several years. Of the total number of American mollusks, those of which we have even an approximate embryological knowledge may be counted on the fingers, and the field therefore is vast. Special training and indefatigable patience, as well as facilities not at the command of all, are absolutely requisite for this most delicate form of investigation and we can hardly hope for rapid progress. Still the results of such enquiries, when properly authenticated and confirmed, are of such exceptional value as to be well worthy the devotion they require.

The anatomical features of the adult mollusks except in a few groups have been much neglected and are of the first importance. Binney and Leidy have done much among the land and fresh water species, and our Pacific Nudibranchs have been the subject of an important monograph by Bergh, while sundry species of marine mollusks have been more or less completely studied by various authors. It is nevertheless true that the soft parts of our commonest species are mostly entirely unknown or but superficially studied, so that even the characteristics of the adult anatomy of our common oyster are differently described by different naturalists and have been monographed by none. This is a reproach that it is in our power to remove, as the facilities required are comparatively limited and inexpensive and few students are so situated that mollusks of one kind or another are not within easy reach. It is true that Americans are not alone in remissness in this direction but that is no sufficient excuse.

The instinctive or mental phenomena which may be exhibited by living mollusks have not been subjected to investigation. It is true that a few observers have incidentally described actions which on mature consideration might be held to exhibit mental processes of a certain kind. The speaker has lately put on record certain observations which seem to indicate the possession, by some species of the genus *Helix*, of the power to recognize a call or sound and to distinguish it from other calls or sounds of a similar character; and, since that publication appeared, he has received several less exact communications which would seem on the whole to confirm this conclusion.

We can hardly doubt, however (whether the particular instances referred to afford proof of it or not), that in a group containing so many animals of a high degree of organization, such as the cuttles and squids, for instance, mental processes of a tolerably complex nature must be carried on in many cases. That this subject has received no attention is a matter of regret and it would seem that a mere mention of it would be sufficient to set some observer at work. Given an aquarium or a snailery, both easily and cheaply constructed and stocked, and patience, time and a little ingenuity will supply all the needed conditions. It would not only seem to be a kind of research peculiarly adapted to attract and amuse an invalid, but one which, if properly conducted, would afford results of the highest scientific interest and value.

There is still another branch of the subject, of which the methods applied to other forms of animal life have occupied a very large portion of the thought and activity of the scientific world for more than twenty years. The results have revolutionized science and justly immortalized the remarkable naturalist who led the way. I refer to the modification of organic life by physical causes and the perpetuation of modifications by natural and sexual selection.

The fact does not seem to have been dwelt upon that the action of natural and especially of sexual selection is much more pronounced, and its effects necessarily must be far more important among organisms either in themselves of a high physical and mental rank reacting on each other (as birds and mammals), or with those upon which the actions of highly organized beings are effective in bringing about a selection of characters as in the case of plants visited by bees and wasps.

It must be granted that natural selection is universal in its action, and self-evident in its truth. It does not therefore follow, however, that it is equally efficacious in perpetuating peculiarities among all kinds of organisms. Indeed on consideration it will appear clearly that it cannot be, and that the more simple and plastic the organism, the less likely it is that natural selection will succeed in confining its variations within any given lines. Take, for instance, a pond full of *Amœba*, millions in number. The probability that any given proliferation, or set of proliferations of the uniform protoplasm of any individual or number of individuals, can be so beneficial as to have any perceptible effect on subsequent generations, is mathematically infinitesimal. And this which is so evident in the case of structureless *Amœbæ* is more or less true throughout all the lower forms of life. It is only when advanced to a comparatively high stage of differentiation that organisms can offer, as it were, a handle for natural selection to take hold of.

There is probably no change or special characteristic in any organism of sufficiently high rank for which a plausible explanation cannot be assumed by the aid of natural selection. Herein lies the danger of this hypothesis. The offered explanation always *may* be true, but students have not always remembered that it also always *may* be false, and that therefore the hypothesis only becomes scientific when it is supported by proof. The *onus probandi* is not lessened by the production of a plausible hypothesis. The proof is often forthcoming in the case of higher organisms, and found with comparative ease. The difficulty increases with each step lower in the scale and practically, as I have shown, becomes insuperable among the lowest organisms. But it is among the lowest organisms that we must look for the first steps toward the grand divisions in which organic life is marshalled. It is here that the key to the great problem of systematic diversity in life is to be found, if at all, and it must be confessed, even at the cost of some disappointment, that natural selection alone does not meet the needs of the case.

American naturalists have from the first recognized more or less clearly the limitations of Natural Selection. By none has it been more clearly stated that natural selection originates nothing, and merely discriminates between already existing differences. By no other geographical group of students have such important expositions been made of the effect of physical forces on the organism and its parts, the relations of anatomical structure to strains and impacts, and of what may be termed the *rhythm* of development as shown by periodic acceleration and retardation in the life of the individual, of the group and of the race. The labors of Cope, Hyatt, Ryder, Morse and others among us, are no less fruitful than suggestive in these directions.

Some unnecessary anxiety seems to have been expressed by many in regard to the origin of variation. It is not doubtful that variation is as much a necessity as natural selection. In all the unnumbered organisms which have existed since the appearance of life on the globe there can have been no two individuals whose circumstances, as regards their internal contents and qualities or their environment, have been absolutely identical. Many approached nearly to it, but absolute identity has not existed. It is therefore not a matter of surprise that variations exist, but rather that there is anything like uniformity among organic beings. The thing that is really extraordinary is that organic life should have so run in ruts, as one might say, that we can, in the majority of cases, refer without doubt a given organism to one of a few great fundamental groups which can be counted on the fingers.

I feel a reasonable confidence that the solution of the mystery lies in the direct physical action of the environment, by ways and methods few of which are yet understood, many of which may long remain mysterious but which in the course of time will undoubtedly be made clear by the research, the energy, the perseverance and the will of that most extraordinary of all organic mysteries, the human mind.

In this connection it seems a duty to refer to that remarkable work, the Principles of Biology, by Herbert Spencer, in which the actions and reactions of organism and environment are so clearly set forth, so many lines of investigation indicated and such pregnant suggestions offered to the philosophical inquirer, that we may fairly regard it as foreshadowing a major part, not only of conclusions definitely arrived at since its publication, but of those which are still hovering in the limbo of probability.

Since the mollusks occupy, in a certain sense, a middle ground between the higher groups, where natural selection has its freest play, and those lower categories, where its operations are veiled or inefficient, investigation of the laws of variation and of those influences which tend to stability of characters, is among them especially desirable.

We find, as by hypothesis we should find, that the influences of selection are most evident in those mollusks which by their terrestrial habitat are brought into the closest contact with enemies of relatively high intelligence such as birds and other vertebrates. In these the selected characteristics are chiefly those of color. The gray and dull appearance of species inhabiting arid regions is well known. It is not impossible that the primary cause of this may be due to a less fluent secretion of the animal products which are the chief components of the pellucid substance and glistening epidermis of shells native to moister regions. It would naturally occur in the dry air of the desert that fluid products unprotected by an impervious epidermis would be less readily retained within the soft tissues of snails. In moister regions a tendency is especially noticeable among operculated Pulmonates, to striped markings of darker and lighter color, such as would tend to conceal them among the lights and shades of grass and herbage which they frequent; and one is tempted to refer the perpetuation and wide prevalence of this style of coloration to causes similar to those by which some have accounted for the vertical stripes of the tiger.

But to what is the origin of the microscopic sculpture of the same shells' most delicate and intricate tracery, constant in the species, to be attributed? It does not seem possible that it can be in any way useful.

Again, in tropical species the reflected lip of the aperture is often painted with exquisite rose, orange or yellow hues, entirely invisible under ordinary circumstances when the mollusks are alive. As they are reciprocally hermaphrodite, sexual selection can have no effect in fixing tints of beauty here. In fact, throughout the group, the most puzzling question, and one which presents itself continually, is that which demands the origin of complex combinations of color and form to which it is impossible to assign any utility. There is an Asiatic species of *Bulimus* of a lovely lemon color which, about the beginning of its last turn before forming the reflected lip which marks maturity, deposits a narrow black or bluish band across the whorl. There is no ridge, no interruption of the smoothness of the shell, no internal process corresponding to the band, nor any other touch of such color elsewhere.

An artist would say at once, the lemon color is heightened and the beauty of the shell greatly enhanced by its contrast with the dark band. But our mollusk is no æsthete and his poor eyes are believed to barely help him to distinguish light and shade, certainly not form or color. Yet we cannot doubt that there is a good and sufficient cause for this and numberless similar features among land shells.

Among marine forms the struggle for existence, after the embryonic stages are past, is much less violent. This arises partly from the much more uniform conditions of life in the sea, partly from the general abundance of food contained in the sea water, and partly from the less intelligence of enemies, which are chiefly fishes and predaceous mollusks.

Since here development of superficial characters in any direction is hardly governed or limited by any sort of selection, we have most remarkable variety exhibited in externals with marked uniformity in structural features. Here, again, we are met with the question, What purpose is served by all this infinite variety of detail which is often, without a microscope, invisible to the observer, and how much more so to the creature itself and its companions of every kind? The exquisite painting of many species of *Conus* is visible only after death, being covered during life with a dense epidermis. The exceedingly delicate ramifications of many Murices, the shagreening of many Trochoids, the richly painted apertures of *Phyllonotus* and the lovely domes of the Cowries,—all these are beyond the perception of their owners or fellow creatures, are utterly useless to them as far as we know, and yet are stable characters within the species.

In certain features Hyatt has traced the influence of gravitation. Food has, without question, great importance especially in determining certain tints of color. I have shown how spirality might have developed from the effect of physical causes seconded by natural selection. In Alaska I noticed a species of *Litorina* which, when living on isolated rocks subjected to the full power of the surf, exhibited modification. The spire became shorter and nearly flat. The last whorl and aperture became enlarged and patulous; strong revolving ridges became rows of rounded nodules and the axis of the shell broad and thick. From this I concluded that those specimens which tended to vary in the directions mentioned possessed advantages over the ordinary form in resisting the action of the waves and therefore retaining their hold upon the rock and life. Once washed into the shifting sands between the rocks, they had small chance of survival. I have often thought of this as one of the most obvious instances I have observed of the action of selection among marine mollusca.

The Carrier-trochus family (*Phoridæ*) are remarkable examples at once of the unexplained acquisition of a valuable habit, its perpetuation by natural selection and in some cases its continued retention when no longer useful.

Certain of these shells pick up and attach to their upper surfaces bits of dead coral, stones and fragments of shell until they are entirely covered. Viewed from above as they must be for the most part by fishes and carnivorous mollusks nothing but dead and refuse matter is visible. It is evident that this must be a great protection both from enemies who would pass them by and from accidental concussions. It may be due to the latter incidental protection that the shells are or have become very thin and indeed barely more than a layer of cement sufficient to hold this extraneous matter in place. It is difficult to imagine how such a habit can have originated, for it is not the practice of gastropods in general and it must have been pretty generally adopted before it could have been of sufficient benefit to the genus at large to have been confirmed as a habit by selection. When this had once taken place its usefulness is obvious. However, it is still more strange that so valuable a habit should, as it has done in many species, fall partially into disuse, so that while the disadvantage of carrying round a burden of projecting and obstructive fragments is retained and indeed sometimes exaggerated by placing the fragments on the extreme periphery of the carrier's shell, the animal should fail to sufficiently cover itself so as to make its load a real protection. This is the case in several species which moreover exhibit a decided discrimination in the materials they collect, so that one species picks up only small dead bivalves, another fragments of coral, and, in one or two cases, only minute pebbles or grains of sand which are of no protection whatever and present the appearance of a mere attempt at decoration. The character and disposition of the load have become so uniform in many cases as almost to take rank as specific characters.

The few instances I have mentioned are merely typical of myriads. They sufficiently indicate the world of mystery which surrounds the life and progress of the members of an entire subkingdom of animals. General readers, perhaps some students, often gain an impression from the steady stream of scientific literature and the enthusiastic claims of some disciples of nature, that we are rapidly illuminating the inmost recesses of the world of knowledge,—that in fact there is little left to explore.

But those whose high privilege it has been to commune with Nature in her sacred haunts, to waste in studious mood the midnight oil and grasp by efforts slow, severe, prolonged, some new truth from the mazy labyrinth,—these will feel with Newton that their labors in the past, and all that any single man may hope to add, are to the contents of great Nature's book but as the gleanings of some little child gathering shells along an ocean strand.

Yet, notwithstanding this, the student rightly feels that every new truth is gathered for all time and is its own reward; and that, in spite of human error, prejudice and weakness, imperfect comprehension of the glories of the harvest and faulty applications of experience, progress is constant. Before the strong breath of persevering and repeated investigation, flake by flake, the chaff is winnowed away. That which remains, fit for the granary of knowledge, is imperishable. Let us then accept the lesson, in graceful verse recorded by a poet naturalist, taught by the ever expanding camerated coil of pearly Nautilus, contemporary of the ages past as well as of the living present:—

"Year after year beheld the silent toil
That spread his lustrous coil;
Still as the spiral grew
He left his past year's dwelling for the new,
Stole with soft step its shining archway through,
Built up its idle door;
Stretched in his new found home and knew the old no more.

Thanks for the heavenly message brought by thee,
Child of the wandering sea,
Cast from her lap forlorn;
From thy dead lips a clearer note is borne
Than ever Triton blew from wreathèd horn;
While on mine ear it rings
Through the dark caves of thought, I hear a voice that sings:

Build thee more stately mansions, O my soul
As the swift seasons roll;
Leave thy low vaulted past!
Let each new temple, nobler than the last,
Shut thee from heaven with a dome more vast,
Till thou at length art free,
Leaving thine outgrown shell by Life's unresting sea."

VARIATIONS IN NATURE.

A CONTRIBUTION TO THE DOCTRINE OF EVOLUTION, AND THE THEORY OF NATURAL SELECTION. By THOMAS MEEHAN, of Germantown, Pa.

MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—

You have honored me by a special invitation to address you on this occasion,[1] and you have suggested that I take for my theme some topic connected with plant life. I know that the papers I have given you during the past ten or fifteen years have led you to this kind consideration; and I therefore feel that I cannot do better than present to you now some of the impressions which my work has made on my own mind, in relation to some of the great questions of the day.

Perhaps the greatest of all questions is still the origin of species —the derivation of all the various forms we see—in brief, the doctrine of Evolution; and even where there is no doubt that one form has been evolved from some differing parent form, it has been by no means made so clear under what theory this growth of form is to be explained, that there can be no reasonable difference of opinion thereon. To my mind the doctrine of the origin of species as presented by Mr. Darwin cannot be controverted; and the theory of natural selection which he elaborated to sustain this doctrine requires only to be pruned of some abnormal growths to make it impregnable. This has been my task during the years referred to. Whenever I have seen weaknesses, I have adduced facts to oppose them. My task to-day is, as I hope, to show that the doctrine of evolution, and especially the theory of natural selection, is all the stronger for the work I have done.

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[1] At the Cincinnati meeting the standing committee invited Mr. Meehan to occupy the chair of (the then) Section B, vacant through the inability of Dr. Geo. Engelmann, the elected Vice President, to be present. Mr. Meehan declined, fearing that his imperfect hearing would interfere with the proper performance of the duties of the chair. The Section then invited Mr. Meehan to deliver a special address at the next meeting of the Association.—ED.

I am particularly glad that the opportunity has been afforded me to undertake this pleasant task at this time, when the whole world is in mourning for Charles Darwin, whose chief claim to our love lies in the earnestness with which he pursued this very theme. I shall have to point out now as heretofore what I regard as weak points in his arguments, not to refute his main propositions, but in the belief that they will be stronger without them. I think you will regard it as one of the most fitting tributes to his memory if I succeed in showing that his propositions are stronger than he himself knew.

Darwin has shown that under domestication and under cultivation variations have occurred among animals and plants which we should certainly regard as species if we found them in a wild condition and their derivation were unknown. He shows that there are variations in nature, and by the operation of a law of selection the intermediate die, leaving forms which seem distinct to us because the intermediate have been lost. These forms continue to vary, with selections as before, till in time we have the great diversity we see. To this there is added another principle which he terms inheritance, and which counteracts variation in some degree, holding and retaining a given form to some extent, but never totally overawing the tendency to vary.

I know of nothing in my own observations, or in the observations of others to contradict this view. Facts confirming them are now of almost every-day occurrence, and we find them in the commonest plants. Near my home in southeastern Pennsylvania, we find the common Virginia creeper, *Ampelopsis quinquefolia*, with five leaflets. In Texas, Mr. Buckley finds it with seven, in northern Pennsylvania it is found sometimes with three. Along the Canada line it is mostly with five, but sometimes with seven. In southeastern Pennsylvania the leaflets are usually broadly ovate, slightly serrate, dark green, and the flowers and fruit are borne on rather stout pedicels; in Colorado the leaflets are rather wedge shaped, deeply laciniate, of a somewhat glaucous green, and the pedicels slender. In southeastern Pennsylvania the secondary veins are delicate, curved, and diverging from each other as they extend towards the edge of the leaflet; along the shores of Lakes Erie and Ontario, especially on Goat Island, near Niagara, the veins are very prominent, straight, almost parallel, and give the appearance at first sight of horse-chestnut leaves. We do not regard these outlying forms as species, we do not even consider them as varieties. But why? Merely because we find in what I will call the central form, a tendency to all the characters referred to. If this central one were to disappear, I think botanists would have no difficulty in regarding the outlying forms as well marked varieties, if not good species. It may be said that these are properly the results of conditions and not variations; that they are the results of accidental circumstances, and are not perpetuated under hereditary laws; but it is one of the discoveries of modern times that these local variations, as they would once have been styled, are just as capable of being perpetuated as the most undoubted species. There is no difference whatever in this respect. Given a fair chance in the struggle for life for the seed to grow and the plant to mature seed, and the race at once has all it requires to go on and perpetuate itself.

It is just here that the advocacy of the theory of natural selection often shows a weakness which I distinguish from the theory itself. Mr. Darwin himself seemed to believe that variation in nature was a slow process, and he continually refers to the gradual modifications going on through long ages.[2] He contends that "variability is not an inherent and necessary contingency" of existence[3], "but comes from the law of correlation of growth; to the direct action of the conditions of life; to use and disuse; to the intercrossing of species;"[4] and he is especially emphatic in stating his belief that variations are ever in the line of profit to the individual. His leading idea is that those individuals which receive the greater sum-totals of profitable variations are the best able to maintain themselves in the struggle for life; in other words every act of life, variation included, proceeds on the basis of individual good. We have been made familiar with this view in much that has been said on the cross-fertilization of flowers. It has been said that flowers have been made beautiful solely on the basis of utility to the plants which bore them. It led to the visits of insects, which then led to cross-fertilization; and cross-fertilization is believed to be a means whereby the progeny is better able to battle for existence. Utility is with him the basis of all individual action; the impelling force in all change.

[2] Darwin's Origin of Species, Chap. IV, paragraph 5, X, paragraph 4, V, summary.

[3] Darwin's Origin of Species, Chap. V.

[4] Darwin's Origin of Species, Chap. V.

I feel sure, however, that a careful view of variations in nature will show that there is not the slightest physiological advantage in many a form which has had a wide distribution, over other forms which have but a localized existence. In the forms of *Ampelopsis* I have referred to, and which would become species if the central form were lost, any one form is just as able to do battle as another. Whether it succeeds in establishing itself or not depends wholly on other circumstances than matters connected with the character of its leaves or inflorescence, so far as we have been able to observe them. Or, let us take a race of plants of simple structure and of great variety. The family of cacti will serve for illustration. Supposing all these to have sprung from one primordial form, and granting that its spines are for protection, what possible gain has any one species obtained among the vast variety of form of plant or spines which enables it on that account to get along better than its neighbor? Supposing one species had a fascicle of twenty spines, how would it be the gainer by developing a new species with but ten, or another one with forty? The only argument I ever heard in such cases is that they must be individually beneficial or they would not exist. This is not the kind of reasoning that will satisfy us to-day, especially as a different view of the objects of nature will furnish us with a better answer.

What are the objects of nature? What will this world and all its creatures be like a hundred centuries from now? We see by the past as compared with the present that the one great aim is development. Nothing now is as it was then ; and yet how closely connected is the present with the past! Then, as now, there was a struggle for life ; but the gain to the conquerors was far less to them than to us. In one word it is posterity at which nature aims. It is not only those who die in the struggle who abandon life that others may live. It is only a few more years, as seen in the light of ages, before those for whom the others seemed to die follow after. Self-sacrifice and not self-interest is nature's demand on us all. We are here as Nature's invited guests ; to do her work ; to assist in this work of developing the future. She makes our stay as pleasant to us as possible ; we should not do her work willingly unless she did, but she ruthlessly removes us the moment we are no longer of use to her in her plans of development. All nature is at work ; but all this work would be vanity if it were merely for individual good, and utility ended with individual life. Look at some poor mother toiling for her children, perhaps in poverty, and with but the barest necessaries of life to eat,—without rest or sleep that they may be fed and clothed; watching over them day by day in sickness and suffering, till her own health gives away, and she becomes a human wreck. She takes pleasure of course in this sacrifice; her lot would be truly unbearable if she did not. But wherein is the individual benefit? No! her struggle is not for life. It is self-sacrifice. She is aiding Nature in her great law of development. Her work is for posterity.

Just as you see it among human beings, you find it among all other animals. The most selfish of all will lay down its life for its young; its work like ours is for the future of its race, rather than for any good which may accrue to its own existence.

We find precisely the same laws operating in the vegetable as in the animal world. We discover nothing in the behavior of plants to indicate that they are actuated by individual good, further than may be necessary to enable them to fall in with Nature's great aim of preparing for the future. Millions of seeds are produced for every one that grows; millions grow for every one that lives long enough to flower; millions of flowers open for every one that yields seeds, and millions on millions of grains of pollen are produced for every one grain that is of service in fertilization. Under the individual utility theory all this is utter waste, and that plant life should be employed for, say, almost the whole of its existence, in sheer waste of effort solely that one moment might be possibly useful, is beyond all comprehension. It is customary to explain this by the guess that nature indulges in all this waste in order that there shall be no chance of failure. But even the thousandth part of all this waste would be sufficient for this permanent policy of insurance. Take for instance the sweet chestnut, *Castanea vesca*, count the number of stamens in a single flower of a male catkin, and multiply by the number of flowers the catkin bears; then get an idea of the number of male catkins on a single tree. Remember that each of the anthers contains hundreds of pollen grains, and conceive if you can the myriad millions of pollen grains on a single tree. Now look at your few bushels of chestnuts, and remember that a single pollen grain served for the production of each nut, and believe if you can that nature induced that tree to produce a million pollen grains in order that one should surely do its work! Independently of this consideration we now know that these showy male flowers fall early, and, there is reason to believe, before the female flowers open, and that pollinization is effected by a second crop of male flowers which appear simultaneously and on the same spike with the female. Moreover, we find, by what are known as cleistogene flowers, that nature can effect fertilization without any waste of pollen were she so minded. In *Viola* there is about a grain of pollen to each ovary in the cleistogamous flower. Another point has been suggested,— that this wonderful amount of vegetable exertion is to secure cross-fertilization, and this seems to be sustained by the small amount produced by cleistogene flowers; but allowing for all this it still does not account for the enormous waste. But we find these surplus seeds, surplus plants, surplus pollen, are useful, not to the parents which bore them, not in any way to themselves, but as sacrifices to posterity. They serve as food. They die that something else may live. They all work in with Nature's grand aim of developing something for the future. At the present time the eyes of science are turned to the past. We compare the dim view with that which is about us, and we perceive that all things have worked together for the good of the whole. We see that nothing has lived in vain. We know that in the general economy of nature there is no waste anywhere. Had the past turned to where we stand it would have seen nothing. People then would have had a better right to believe than we, that individual utility is at the bottom of all life; that "No. 1 is the first law of nature." It is for us to see that self-sacrifice was really their lot, and what was the future for them will still be the law of the future as the world rolls on.

To my mind, then, no doctrine of evolution, no theory of natural selection, will be perfect that looks to individual or immediate utility alone as its foundation-stone. Self-sacrifice, the good of the future at the expense of the individual or of the race, if need be, must hold the primal rank.

Let us now turn to look at nature. I am satisfied that not even the wonderfully acute powers of observation of the great Darwin led him to perceive the wonderful variations there are in vegetation everywhere. It is a common saying that things are "as much alike as two peas;" but if you look with a microscope you will not find two peas alike. No two leaves on a tree can be found exactly alike. In outline, serration, pubescence, venation, some one thing or another you will find unlike the one you compare with.

Individual plants are not like other plants. You will be surprised if you compare closely to find how much they differ. It is best to take for examination those genera which have but a single species, so that no objections from cross-fertilization may arise. Take the ox-eye daisy, *Chrysanthemum Leucanthemum*, *Linaria vulgaris*, or *Epigœa repens.*[5] I name these because they are common, and because I have carefully examined them, and I have never found two plants from different seeds, but differed in some respect, either in foliage or inflorescence. You will find differences in cotyledons as well as in the primary leaves which succeed them. Examine a series of first leaves of *Robinia pseud-acacia* the common locust; some are elongated, some orbicular, some panduriform, some oblate, and these varying again in other characters. I have examined, for these facts, plants obtained from seed of the same tree. I also exhibit a series of *Pyrus*, or *Cydonia Japonica*, which I do know were raised from seed produced on one bush. There are some two hundred seedlings, and not only are no two alike, but there are amazing differences of form in the foliage, from narrow saliciform to almost round. Our esteemed fellow member and secretary of this meeting, Mr. William Saunders, showed me some seedling gooseberries of remarkable character. The seed was all from one berry, but the plants varied; some with very bristly fruit, some with smooth fruit, and I have myself seen ovate and globular fruited plants also raised from seed of one berry. I also exhibit two branches from two plants of *Rosa cinnamomea*, both plants raised from seed from one seed vessel, in which you see one branch almost wholly free of thorns and bristles; the other densely, nay ferociously, armed with long bristles and thorns. I could indefinitely continue these illustrations; you may at any time find numbers for yourselves. You will have no difficulty in believing that perpetual variation is a condition of existence. These variations have no special physiological value; no one will say that the possessor of one form has any special advantage over another in the struggle for life. Unless they are the incipient stages of that development which is to result in species and genera in the future, they have absolutely no meaning at all.

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[5] Proceedings Academy of Natural Sciences of Philadelphia, pp. 155–6.

Some who have noticed this fact of universal variation attach little importance to it. They believe it is but what they term a limited variation, extending only to a certain contracted line, and then returning; that such variations are at best but temporary and have no place under hereditary law. I confess that I once held this opinion. In a certain sense it is correct. If a leaf, oval, normally as we would say, changes until it becomes round, it cannot afterwards become rounder. A plant with a small flower develops one with a large flower. As flowers are morphologically changed leaves, a flower cannot become larger than the original leaf-type will permit. In this sense there must be a limit to variation, but in no other sense that I can find.

In the many combinations which can be made with a few elements, there is continuous change.

In regard to heredity I know from many years' experience, and contrary to generally received opinion, that any variation that appears in nature is capable of being perpetuated under the laws of heredity in just the same degree as those plants which we commonly regard as species. Let a white variety of a colored flower appear, and its progeny will continue white-flowered. I have had several generations of purple-leaved berberries and weeping peaches, and weeping ashes produce weeping seedlings. Purple peaches, golden spiræas, variegated plants of all kinds reproduce their kind with only just so much variation as we find in connection with all things.

I may remark further that sex, while to some extent an agent under the laws of variation, acts with greater force as an agent under heredity. Under the laws of variation A may depart somewhat from B on one side; and on the other side C departs from B; but an insect cross-fertilizing A with the pollen of C brings the progeny again towards B, and thus cross-fertilization acts as a check on rapid divergence. I have noticed this action of bees in the case of variations in *Linaria vulgaris*.[6] On the other hand there may be three variations, A, B, C; B being an advance on A, but B is fertilized by C; C having no opportunity to perpetate itself dies out; but B succeeding in this has gained a step through the sexual agency of C. To my mind the operation of the law of sex in connection with heredity, checking too great a tendency to wander off and mix with the world, producing as it were a sort of family relationship which it takes several generations under the law of variation to break up, is a far more legitimate theory of the use of cross-fertilization than anything which has been brought out under the more selfish hypothesis of utility to the individual or to its immediate descendants.

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[6] Proceedings of Academy of Natural Sciences of Philadelphia, 1872, p. 235.

Now let us formulate our own theory of evolution and the origin of species by natural selection.

Nature primarily knows only the individual. This individual is made to reproduce itself after a short term of life, but with some general resemblance; yet every individual varies from its parent, some in one direction, some in another, the object of this variation being to reach some harmonious result far away in the future — a future which seems to us almost an eternity of years. The march of this variation is held in temporary check by heredity, aided especially by the influence of sex. Individuals become a large community. The wide world is around them. Those on the outside of the circle battle with the rest of the world; those in the interior battle also with themselves. For illustration we will call the community a pine forest. Millions of seeds are produced on the trees of those of the interior, not a hundred in a million will grow; of these hundred not ten may come to be young trees; and of these ten not more than one may get the chance to reproduce itself. Suppose that one happens to be what the human mind should regard as a striking variation; cross-fertilization steps in and draws it back to the original. No matter how great might be the physiological value of the variation to that tree in any ordinary struggle for life, antagonizing circumstances render the point gained of no possible use to it. But if the variation find itself on the outside of this circle, with no struggle with its own kind, with plenty of chance to mature its seed, an abundance of room for the young plants to develop, little opportunity for cross-fertilization to mar the distinctive variation it has gained, and still gain a chance for the progeny to reproduce in the same way, heredity will give you a distinct series, which will lead to arguments whether they are hybrids, varieties or species. If, however, acres of the interior of the forest get destroyed, and nothing but the outline of the circle is left, if there should arise a sort of Pacific Ocean over what was once the land, and only, say Japan, remain on the west, and the Atlantic United States on the east, you would have only the modern variations left to tell the ancient story. It may not be the centre which meets this destructive fate. Some forms, as for instance *Onoclea sensibilis*, the well known fern, may avoid destruction, and get enwrapped in the laws of heredity for a hundred thousand years, while some or all of its progeny may have been swept away. There is no reason why grandparents among species may not survive long after every child or grandchild has ceased to be. The advance, the resting spot for the family, the further advance and the changes, the weeding out, and the survival of the fittest to carry on the great work of destiny,— how simple thus becomes the doctrine of evolution and the theory of natural selection!

We have taken the facts for our own conclusions as we have gone along; let us finally examine one more illustration. Here we have a series of cones made up from trees growing within a circuit of, say, a day's ride around Hammonton, N. J. They are all from trees of *Pinus rigida*. From one tree we have cones four inches long and almost round; others four inches long and not two inches wide; the one would be called almost linear, the other almost globose. Others are flat at the base and will stand upright; the others are rounded and roll over like marbles. One is so small as to be not more than an inch in length, and yet with perfect seeds. Some have very narrow scales, others have very wide ones. Some open slightly, but never much, others open so much that they reflex. Then there are some which to the best of my belief never open. Then we find several characters going along together. For instance, it is the narrow scaled forms which recurve; while the broader the scale the less tendency there is in the cone to open and scatter the seed. We see that all these cones belong to *Pinus rigida*, because between each cone there is but a shade of difference. Now I have already shown you that seeds of any one of these cones would produce trees bearing cones with a general resemblance to the parent cone. Supposing all these intermediates were to be crowded out, and only the two extremes left, there is not a botanist but would regard them as two distinct species. Now what I have given you as a suppositious case is an actual truth. We have at one end *Pinus rigida*, at the other *Pinus serotina* of the older botanists. *Pinus serotina* is no longer considered a good species. Why? Not because there was any error in the original description, but simply because we have found the missing links. You cannot conceive of any physiological value to the individual or to the race to spring from any of these changes from the original form; you will not for one moment believe that in any one of these forms there is the slightest advantage to be gained over the other in what would be generally understood as the struggle for life. On the contrary, you will find one which has the cone closed, and in this particular lost the power to readily distribute the seeds; lost the great advantage of numbers in the tremendous battle; is the one form which has so well succeeded in establishing itself as to be the only one which has gained a distinctively botanical name.

I think you will come to agree with me that "the race is not always to the swift, nor the battle to the strong" is just as true in the development of species as in morals; that the whole round of organic existence is not one of self-seeking advantage over one's neighbor, but one of intrepid self-sacrifice; and that the doctrine of evolution and the theory of natural selection are rendered clearer and stronger by the perception of these facts.

I might rest here, but science cannot afford to ignore the objections sometimes made that evolution and especially the theory of natural selection is opposed to doctrines which are held sacred by so large a portion of the human race. I would rather that we could ignore them. Science should be allowed to work out its own salvation, if there be aught in it worth saving. It would die of itself when it had nothing to live on; but objections being made it is a gain to science to show that they have no force.

The essential doctrine of Christianity is self-sacrifice. It was the great burden of its founder's teaching, and was illustrated by the sacrifice of his own life. True happiness to him was to do the will of Him who sent him.

How exactly does this not accord with the laws of nature as I have unfolded them? Variation, as we have seen, is established, not that things which vary derive any special advantage by the variation, but that all varied things may work the better together for the good of some future object, to them unknown. Their work is of no advantage to them, but the task is made pleasant, so that the work may still go on.

And the reaper, death, gathers in his harvest of species, just as he gathers in his harvest of individuals, without regard to youth or old age, to the strong or to the weak, oftentimes confounding our reason by the mere assertion of his will, and leaving us no alternative but to believe that natural selection, true as it is and must be, is still in the hands of some directing power which we have not yet fathomed, but which is making all things work together for some great future object, of which, as yet, not the faintest glimmer has been perceived by the wisest man.

There is no conflict between the doctrine of evolution with its theory of natural selection and the essential doctrines of Christianity. Rather, as I believe, will the day arrive when those who advocate these views will be regarded as Christianity's best friends.

PAPERS READ.

Remarks Concerning the Flora of North America. By Asa Gray, of Cambridge, Mass.

In the remarks which I have to offer to this Section, you will understand the word *Flora* to be written with a capital initial. I am to speak of the attempts made in my own day, and still making, to provide our botanists with a compendious systematic account of the phænogamous vegetation of the whole country which the American Association calls its own.

I shall make no effort to avoid the personal turn which my narrative is likely to take. In fact, it will be seen that I have partly a personal object in drawing up this statement.

Only two Floras of North America have ever been published as completed works, that of Michaux and that of Pursh. A third was begun (by Dr. Torrey, assisted by a young man who is no longer young), by the publication in the summer of 1838 of a first fasciculus; the first volume of 700 pages was issued two years afterward; and 500 pages of the second volume appeared in 1841 and in the early part of 1843. The time for continuing it in the original form has long ago passed by. Its completion in the form in which I have undertaken it anew, is precarious. *Precarious* in the original sense of the word, for it is certainly to be prayed for: precarious, too, in the current sense of the word as being uncertain; yet not so, according to an accepted definition, viz.: "uncertain, because depending upon the will of another;" for it is not our will but our power that is in question; and it is only by the combined powers and efforts of all of us interested in Botany that the desired end can possibly be attained.

It were well to consider for a moment how and why it is that a task which has twice been — it would seem — easily accomplished has now become so difficult.

The earliest North American Flora, that of the elder Michaux, appeared in the year 1803. It was based entirely upon Michaux's own collections and observations, does not contain any plants which he had not himself gathered or seen, is not, therefore, an exhaustive summary of the botany of the country as then known, and so was the more readily prepared. Michaux came to this country in 1785, returned to France in 1796, left it again in Baudin's expedition to Australia in 1800, and died of fever in Madagascar in 1802. The Flora purports to be edited by his son, F. A. Michaux, who signed the classical Latin preface. The finish of the specific characters, and especially the excellent detailed characters of the new genera, reveal the hand of a master; and tradition has it that these were drawn up by Louis Claude Richard, who was probably the ablest botanist of his time. This tradition is confirmed by the fact that Richard's herbarium (bequeathed to his son, and now belonging to Count Franqueville), contains an almost complete set of the plants described, and I found that the specimens of Michaux supplied to Willdenow's herbarium at Berlin were ticketed and sent by Richard. Not only the younger Richard but Kunth also habitually cited the new genera of the work as of Richard, and some others have followed this example. Singularly enough, however, there is no reference whatever to Richard in any part of the Flora, nor in the elaborate preface. The most venerable botanist now living told me that there was a tradition at Paris that Richard performed a similar work for Persoon's *Synopsis Plantarum*, and that he declined all mention of his name in the synopsis and in the Flora, because the two works — contrary to the French school — were arranged upon the Linnæan Artificial System. He had his way, and the tradition may be preserved in history; but his name cannot be cited for the genera *Elytraria*, *Micranthemum*, *Elodea*, *Stipulicida*, *Dichromena*, *Oryzopsis*, *Erianthus*, and the like. For, by the record these are of *Michaux*, *Flora Boreali-Americana*, and not of Richard.

Michaux's explorations extended from Hudson's Bay, which he reached by way of the Saguenay, to Florida, as far, at least, as St. Augustine and Pensacola; he was the first botanical explorer of the higher Alleghany Mountains, and, crossing these mountains in Tennessee, he reached the Mississippi in Illinois, and was as far south as Natchez. His original itinerary, which I once consulted, is preserved by the American Philosophical Society, at Philadelphia, to which it was presented by his son. It ought to be printed. That little journal shows that it was not Michaux's fault that the first Flora of North America was restricted to the district east of the Mississippi River. He had a scheme for crossing the continent to the Pacific. He warmly solicited the government at Washington to undertake such an exploration, and offered to accompany it as naturalist. This may have been the germ or the fertilizing idea of the expedition of Lewis and Clark, which was sent out a few years afterward by Jefferson, to whom, if I rightly remember, Michaux addressed his enterprising proposal.

Leaving out the Cryptogams of lower rank than the Ferns, we find that the Flora of Michaux, published at the beginning of this century, say eighty years ago, contains 1530 species, in 528 genera. No very formidable number; as to species (speaking without a count) little over half as many as are described in my Manual of the Botany of the Northern States, which covers less than half of Michaux's area.

Eleven years afterward, namely, in the year 1814 (the preface is dated December, 1813), appeared the second Flora of North America, namely the *Flora Americæ Septentrionalis* by Frederick Pursh. This was not confined to the author's own collections, but aimed at completeness, or to give "a systematic arrangement and description of the plants of North America, containing, besides what have been described by preceding authors, many new and rare species, collected during twelve years' travels and residence in that country."

It appears that Pursh was born at Tobolsk, in Siberia, of what parentage we do not know. He himself tells us, in his preface, that he was educated in Dresden, and he came to this country — to Baltimore and Philadelphia—at the close of the last century, when he must have been only twenty-five years old. He was able to make the acquaintance not only of Muhlenberg, who survived until 1815, and of Wm. Bartram, who died in 1823, but also of the veteran Humphrey Marshall, who died in 1805. His early and principal patron was Dr. Benjamin Smith Barton, who supplied the means for most of the travels which he was able to undertake, and who, as Pursh states, "for some time previous had been collecting materials for an American Flora." Pursh's personal explorations were not extensive. From 1802 till 1805 he was in charge of the gardens of Wm. Hamilton, near Philadelphia. In the spring of the latter year, as he says, he "set out for the mountains and western territories of the Southern States, beginning at Maryland and extending to the Carolinas (in which tract the interesting high mountains of Virginia and Carolina took my particular attention), returning late in the autumn through the lower countries along the sea-coast to Philadelphia." But, in tracing his steps by his collections[1] and by other indications, it appears that he did not reach the western borders of Virginia nor cross its southern boundary into the mountains of North Carolina. The Peaks of Otter and Salt-pond Mountain (now Mountain Lake) were the highest elevations which he attained. Pursh's preface continues: "The following season, 1806, I went in like manner over the Northern States, beginning with the mountains of Pennsylvania and extending to those of New Hampshire (in which tract I traversed the extensive and highly interesting country of the Lesser and Great Lakes), and returning as before by the sea-coast." The diary of this expedition, found among Dr. Barton's papers and collections in possession of the American Philosophical Society, has recently been printed by the late Mr. Thomas Potts James. It shows that the journey was not as extended or as thorough as would be supposed; that it was from Philadelphia directly north to the Pokono Mountains, thence to Onandaga, and to Oswego,—the only point on the Great Lakes reached,—thence back to Utica, down the Mohawk Valley to Saratoga, and north to the upper part of Lake Champlain and to the lesser Green Mountains in the vicinity of Rutland, but not beyond. Discouraged by the lateness of the season, and disheartened—as he had all along been — by the failure and insufficiency of remittances from his patron, Prush turned back from Rutland on the 22d of September, reached New York on the 1st of October, and Philadelphia on the 5th. The next year (1807) Pursh took charge of the Botanic Garden which Dr. Hosack had formed at New York and afterward sold to the State, which soon made it over to Columbia College.[2] In 1810, he made a voyage to the West Indies for the recovery of his health. Returning in the autumn of 1811, he landed at Wiscasset, in Maine, "had an opportunity of visiting Professor Peck of Cambridge College, near Boston," and of seeing the alpine plants which Peck had collected on the White Mountains.[3] At the end of the latter year or early in 1812 he went to England with his collections and notes; and at the close of 1813, under the auspices of Lambert, he produced his Flora, consulting, the while, the herbaria of Clayton, Pallas, Plukenet, Catesby, Morison, Sherard, Walter, and that of Banks. Evidently such consultations and the whole study must have been rapid. The despatch is wonderful. One can hardly understand the ground of the statement made by Lambert to my former colleague, Dr. Torrey, that he was obliged to shut Pursh up in his house in order to keep him at his work.

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[1] In herb. Barton and herb. Lambert.

[2] Expecting, no doubt, that it would be kept up. But "the Elgin Botanic Garden" was soon discontinued. It occupied the block of ground now covered by the buildings of the College, and the surrounding tract—now so valuable—from which the College derives an ample revenue. *Noblesse oblige*, and it may be expected that the College—so enriched — will, before long, provide itself with a botanical professorship, and see to the careful preservation and maintenance of the precious Torrey Herbarium, which it possesses along with other subsidiary herbaria.

I know not how Pursh was occupied for the next four years, nor when he came to Canada. But he died here at Montreal, in 1820, at the early age of forty-six. More is probably known of him here. If I rightly remember, his grave has been identified, and a stone placed upon it inscribed to his memory.[4] A tradition has come down to us — and it is partly confirmed by a statement which Lambert used to make, in reference to the vast quantity of beer he had to furnish during the preparation of the Flora—that, in his latter days, our predecessor was given to drink, and that his days were thereby shortened.

In Pursh's Flora we begin to have plants from the Great Plains, the Rocky Mountains, and the Pacific Coast, although the collections were very scanty. The most important one which fell into Pursh's hands was that of about 150 specimens, gathered by Lewis and Clark on their homeward journey from the mouth of Columbia River. A larger collection, more leisurely made on the outward journey, was lost. Menzies in Vancouver's voyage had botanized on the Pacific coast, both in California and much farther north. Some of his plants were seen by Pursh in the Banksian Herbarium, and taken up. I may here say that in the winter of 1838–39 I had the pleasure of making the acquaintance of the venerable Menzies, then about ninety-five years old.

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[3] It is at Wiscasset, therefore, that Pursh's "*Plantago cucullata* Lam. . . . . in wet rocky situations, Canada and Province of Maine," is to be sought. Mr. Pringle has recently found the related *P. Cornuti* (which may be the plant meant), in Lower Canada, not far from the other side of Maine.

It must have been in Professor Peck's herbarium (no longer extant), that Pursh saw what he took to be *Alchemilla alpina*, which he remarks "*v. s.*" and refers to from memory only, probably mistakenly. For it has not since been detected either in Vermont or New Hampshire, or anywhere in North America; and Pursh's Journal makes it certain that he did not reach any alpine region in the Green Mountains.

[4] In the Canadian Naturalist, Principal Dawson gives a brief account of the transference of the remains of Pursh from a grave-yard below Montreal, in which they were interred, to the beautiful Mount Royal Cemetery, where they rest in a lot purchased for the purpose and under a neat and durable granite monument, provided by the naturalists of Montreal and their friends. A small company of botanists, led by Dr. Dawson, visited the spot shortly after the reading of this paper. We learned that Pursh had botanized largely in Canada, in view of a Canadian Flora, and that his collections were consumed by a fire at Quebec shortly before his death, to his extreme discouragement.

In the Supplement, Pursh was able to include a considerable number of species, collected by Bradbury on the Upper Missouri, in what was then called Upper Louisiana,—much to the discontent of Nuttall, who was in that region at the same time, and who, indeed, partly and imperfectly anticipated Pursh in certain cases, through the publication by the Frasers of a catalogue of some of the plants collected by Nuttall.

To come now to the extent of Pursh's Flora, published nearly sixty-nine years ago. It contains 740 genera of Phænogamous and Filicoid plants, and 3076 species,—just about double the number of species contained in Michaux's Flora of eleven years before.

I must omit all mention of more restricted works, even such as Nuttall's Genera of North American Plants, which came only four years after Pursh's Flora; also the Flora Boreali-Americana of Sir Wm. Hooker, which began in 1829, but was restricted to British America. I cannot say how early it was that my revered master, Dr. Torrey, conceived the idea of the Flora which he at length undertook. But he once told me that he had invited Nuttall to join him in the production of such a work, and that Nuttall declined. This must have been as early as the year 1832, that is, half a century ago. My correspondence with Dr. Torrey began in the summer of 1830, when I was a young medical student, and three or four years afterward I joined him at New York and became, for a short time, his assistant, for all the rest of his life his botanical colleague. He was very much occupied with his duties as professor, chiefly of chemistry; he had not yet abandoned the idea of completing his Flora of the Northern and Middle States, the first volume of which was finished in 1824, while yet free from all professional cares. Although working in the direction of the larger undertaking, the *Flora of North America* did not assume definite shape before the year 1835. I believe that some of the first actually-prepared manuscript for it was written by myself in that or the following year. I was then and

me expecting to accompany the South Pacific Explor-
ion, as originally organized under the command of
Ap. Catesby Jones, but which was subject to long de-
y vicissitudes ; during which, having plentiful leisure,
prentice hand upon some of the earlier natural orders.
xpedition, as modified, was ready to sail, under the
Capt. Wilkes, I had accepted Dr. Torrey's proposal
l be his associate in the work upon which I had made
nning as a volunteer. Two parts, or half of the first
pages), of this Flora, were printed and issued in July
, 1838.

ought at first, in all simplicity, that the whole task
e at something like this rate. But, apart from other
ns, it soon became clear that there had been no proper
n of the foundation-species of the earlier botanists,
is downward ; and that our Flora could not go on sat-
ithout this. Dr. Torrey had, indeed, some years before,
y visit to Hooker at Glasgow, to London, and to Paris ;
ng of a few notes upon some particular plants in the
Hooker, Lambert, and Michaux, and the acquisition,
r, of a good set of the Arctic plants of the British
as about all that had been done. I proposed to at-
hing more ; so, taking advantage of a favorable oppor-
led for Liverpool in November, 1838, and devoted a
the ensuing year to the examination of the principal
ich I need not here specify, in Scotland (where the
ne of Sir Wm. Hooker still remained), England,
tzerland and Germany, namely, those which contained
ens upon which most of the then-published North
pecies had been directly or indirectly founded, es-
se of Linnæus and Gronovius, of Walter, of Aiton's
ensis, Michaux, Willdenow, Pursh, and the later ones
lle and Hooker.[5]

return the work made good progress ; the remaining
first volume was brought out in the spring of the year
y the spring of 1843 the 500 pages of the second vol-
occupied by the vast order Compositæ, had been
t meanwhile I had in my turn to assume professorial

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connection, "Notices of European Herbaria particularly those most in-
North American Botanist," in this Journal, vol. xi, January, 1840.

duties and incident engagements,—with the result that, although the study of North American plants was at no time pretermitted, either by Dr. Torrey while he lived, or by myself, we were unable to continue the publication during my associate's lifetime; and it was only recently, in the spring of 1878, that I succeeded in bringing out, in a changed form, another instalment of the work, completing the *Gamopetalæ*.

In the interval I had made two year-long visits to Europe for botanical investigation, the first partly relating to the botany of the South Pacific, the second wholly in view of the North American flora. And since this last publication still another visit—the fourth and we may suppose the last—of the same character and the same duration, has been successfully accomplished.

The serious question, in which we are all concerned, arises, whether this work can be carried through to a completion, and the older parts (wholly out of print and out of date), reëlaborated —I will not say by my hands—but in my time, or soon enough to render the whole a reasonably full and homogeneous representation of the North American flora, as known in this latter part of the nineteenth century. And it brings us to consider why the undertaking, to which so much time has been devoted, should be so slow of accomplishment.

If this slowness is a constant wonder and disappointment to most people interested in the matter, I can only add that it is hardly less so to myself. It is a constant surprise—if one may so say—that the work does not get on faster.

Of course the undertaking has become more and more formidable with the enlargement of geographical boundaries and of the number of species discovered. As to the increase in the number of species to be treated, we have by no means yet reached the end. The area, that of our continent down to the Mexican line, we trust is definitely fixed, at least for our day. And, since we cannot be rid of the peninsula and keys of Florida, which entail upon us a considerable number of tropical species, mostly belonging to the West Indies—the southern boundary is now as natural a one as we can have.

The area which Pursh's Flora covered was, we may say, the United States east of the Mississippi, with Canada to Labrador, to which was added a couple of hundred of species known to him outside these limits northwestward.

Torrey and Gray's Flora took the initiative in annexing Texas, ten years before its political incorporation into the Union; although the only plants we then possessed from it were certain portions of Drummond's collections. California was also annexed at the same time, on account of Douglas's collections, and those of Nuttall, who had just returned from his visit to the western coast, which he reached by a tedious journey across the continent over ground in good part new to the botanist. Douglas had already made remarkably full collections along a more northern line. The British arctic explorers, both by sea and land, had well developed the botany of the boreal regions, and Sir Wm. Hooker was bringing out the result in his Flora of British America. Of course our knowledge of the whole interior and western region was small indeed, compared with the present; and the botany of a vast region from the western part of Texas to the Californian coast was absolutely unknown, and so remained until after the publication of the Flora was suspended.

As to the number of species which Torrey and Gray had to deal with, I can only say that a rapid count gives us for the first volume about 2200 Polypetalæ; that there are 109 species in the small orders which in the second volume precede the *Compositæ;* and that there are of the *Compositæ* 1054. So one may fairly conclude that if the work had been pushed on to completion, say in the year 1850, the 3076 species of Pursh's Flora in the year 1814 might have been just about doubled. Probably more rather than less; for if we reckon from the number of the *Compositæ*, and on the estimate that they constitute one-eighth of the phænogamous plants of North America, instead of 6150, there would have been 8430 species known in the year specified.

It most concerns us to know the number of species which, after the lapse of thirty years more—years in which exploration has been active, and has left no considerable part of our great area wholly unvisited—the now revived Flora has to deal with. We can make an estimate which cannot be far wrong. In the year 1878, my colleague, Mr. Watson, finished and published his Bibliographical Index to the Polypetalæ of North America, covering, that is, the same ground as the first volume of Torrey and Gray's Flora, completed in 1840. In it the 2200 species of the latter date are increased to 3038. The "*Gamopetalæ* after *Compositæ*" in the Synoptical Flora, brought out in the same year, contains 1656 species. The two together must make up half of our phænogamous botany, that is, adding the increase of the last four years, about 5000 species. And so Mr. Watson adopts the estimate of 10,000 species for our known Phænogams and Ferns. My expression is that the species of *Compositæ* have increased at a rate which, unless they exceed the eighth part of our Phænogams, will warrant a still higher estimate. The number of introduced species of various orders, which will have to be enumerated and most of them described, is, unhappily, fast increasing,[6] and new indigenous species are almost daily coming to us from some part or other of our wide territory; so that the 10,000 species of this estimate may before long rise to eleven or twelve thousand. Only the experienced botanist can form a just idea of what is involved in the accurate discrimination and proper coördination of 10–12000 species, and in the putting of the results into the language and form which may make our knowledge available to learners or to succeeding botanists.

Moreover, there is of late an *embarras des richesses* which is becoming serious as respects labor and time. The continued and ever increasing influx of materials to Cambridge, beneficial as it ever is, is accountable for this retardation of progress in a greater degree than almost any one would suppose. The herbarium, upon whose materials this work is mainly done, and which has been, like the Temple, full forty and six years in building, has received the contributions of two generations of botanists, and the Torrey herbarium goes back one generation further. Still the number of American specimens annually coming to it is greater than in most former years. Apart from the mere selection and care of these, consider how in other ways it affects the rate of progress of the Flora. The incoming of additional specimens may at a glance settle doubts as to the validity of a species; but new specimens are as apt to raise questions as to settle them; more commonly they raise the question as to the limitation and right definition of the species concerned, not rarely, also, that of their validity. When one has only single specimens of related species, the case may seem clear and the definition easy. The acquisition of a few more, from a different region or other conditions, almost always calls for some re-consideration, not rare for re-construction. People generally suppose that species, and even genera, are like coin from the mint, or bank notes from the printing press, each with its fixed marks and signature, which he that runs may read, or the practised eye infallibly determine. But in fact species are judgments—judgments of variable value, and often very fallible judgments, as we botanists well know. And genera are more obvious judgments, and more and more liable to be affected by new discoveries. Judgments formed to-day—perhaps with full confidence, perhaps with misgiving—may to-morrow, with the discovery of new materials or the detection of some before unobserved point of structure, have to be weighed and decided anew. You see how all this bears upon the question of time and labor in the preparation of the Flora of a great country. If even in old Europe the work has to be done over and over, how much more so in America, where new plants are almost daily coming to hand. It is true that these fall into their ranks, or are adjustable into their proper or probable places, but not without pains-taking and tedious examination.

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[6] I say "unhappily," for they adulterate the natural character of our flora, and raise difficult questions as to how much of introduction and settlement should give to these denizens the rights of adopted citizens.

Of our Flora, it may indeed be said, that "if 'twere done when 'twere done, then 'twere well it were done quickly." But I may have made it clear that, in the actual state of the case, it is likely to be done slowly. At least you will understand why thus far it has been done slowly. As to the future, if it depended wholly upon me, the completion would obviously be hopeless. I need not say that our dependence for the actual elaboration must largely be upon associates, upon the few who have the training and the vast patience, and the access to herbaria and libraries, requisite for this kind of work, but above all upon my associate in the herbarium at Cambridge, to whom, being present with us, I will not further allude.

Of course we rely, very much indeed, upon the continued coöperation of all the cultivators of botany in the country; and it is gratifying to know that their number is increasing, new ones not less zealous than the old, and better equipped, are taking the places of those that have passed away, and some of them extending their explorations over the remotest parts of the land, and into districts where there is most to be discovered. All can help on the work, and all are doing so, by the communication of specimens and of observations. Those within the range of the published manuals and floras get on—or should get on—with only occasional help from us. They should send us notes and specimens to any amount; but they should not ask us to stop to examine and name their plants, except in special cases, which we are always ready enough to take up. Those who collect in regions as yet destitute of such advantages may claim more aid, and we take great pains to render it; partly on our own account, that we may assort their contributions into their proper places, partly for the encouragement of such correspondents, who otherwise would not know what they have obtained, and who naturally like to know when they have made interesting discoveries.

But the scattered and piecemeal study of plants is neither very satisfactory nor safe. And it involves great loss of time, besides interrupting that continuity and concentration of attention which the proper study of any group of plants demands. As respects the orders of plants which are yet to be elaborated for the Flora, and as to plants which require critical study or minute examinations, necessarily consuming much time, it is better to defer their complete determination until the groups to which they severally belong are regularly taken in hand.

The coöperation of all our botanical associates is solicited in this regard, as a matter of common interest and advantage. For we are all equally concerned in forwarding the progress of the Flora of North America; and we may confidently expect from our botanical associates their sympathy, their forbearance, and their continued aid.

ON THE POSITION OF THE GAMOPETALÆ. By LESTER F. WARD, of Washington, D. C.

[ABSTRACT.]

IN 1789, Laurent de Jussieu, in his "Genera Plantarum," established the three "Divisions" of Dicotyledons, assigning the Apetalous Division the lowest place, the Monopetalous Division the second, or intermediate, place, and the Polypetalous Division the highest place in the series.

In 1848, Adrien de Jussieu, in his "Cours Eléméntaire de

Botanique," reversed the order of the second and third of these Divisions and gave his reasons therefor.

Notwithstanding this, nearly all subsequent authors have steadily adhered to the order established by Laurent de Jussieu, which is supposed to have been that of his uncle Bernard, the real founder of the "natural system."

This arrangement has, however, been severely criticised by Al. Braun and many others.

The present paper is designed to supplement an article "On the Natural Succession of the Dicotyledons" communicated by the writer to the "American Naturalist" for November, 1878, and to bring forward some evidence from statistics and from palæontology, as that did from organogeny, of the comparative *recency* of those plants having tubular, bell-shaped, or otherwise consolidated corollas, and thus to claim for that Division of the Dicotyledons — the *Monopetalæ*, or *Gamopetalæ*, as they are now more properly called — the highest place in the series, as did the younger Jussieu.

This is shown, first, by the great extent to which these plants are herbaceous as compared with those having the petals free (*Polypetalæ*) or with those destitute of petals (*Apetalæ*, or *Monochlamydeæ*).

From a comparison of seventeen floras, including those of the Eastern United States, of California, of Great Britain, of Italy, of the West Indies, etc., it is found that of the total Dicotyledons the Apetalous Division furnishes on an average 13.3 per cent, the Polypetalous Division 43.3 per cent, and the Gamopetalous Division 43.4 per cent; but when the shrubs, trees, and woody vines of each of these Divisions are alone considered, we find that the *Apetalæ* furnish about 24 per cent, the *Polypetalæ* over 43 per cent, and the *Gamopetalæ* only about 33 per cent. In some of the floras compared the percentage of this last Division is as low as 20.

A perhaps still more striking way of showing this is to see what proportion the *woody plants* of each Division bear to the total number in that Division; *e. g.*, of the *Apetalæ*, which in the flora of the Eastern United States form only 13 per cent of the total Dicotyledons, 42 per cent are woody, while of the *Gamopetalæ*, which form 47 per cent of the Dicotyledons, only 16 per cent are woody.

If trees only are taken into the account the contrast becomes still stronger. In the last named flora, which is the largest of those compared, the *Apetalæ* constitute 38, the *Polypetalæ* 45, and the *Gamopetalæ* 17 per cent of the dicotyledonous trees.

These facts seem to show that the *Gamopetalæ* constitute a comparatively late type of vegetable growth which has not yet had time, as it were, to perfect itself and assume the frutescent and arborescent forms which mark the highest development, or phytological maturity.

In the second place, and in order to anticipate the possible objection that this might equally prove degeneracy, such as is seen in the Monocotyledons and in the Cryptogams, which, though known to be older types of vegetation, are, nevertheless, at the present time chiefly herbaceous, although once probably for the most part arborescent, the following palæontological evidence was adduced:

It was shown

1. That the first appearance of plants that are clearly referable to Gamopetalous genera is much later than that of either Apetalous or Polypetalous plants.

2. That the most abundant of those genera are such as in the living flora have very deeply lobed corollas, as in *Viburnum*, thus approaching the dialypetalous condition and strongly suggesting that at the time of their appearance they might not have been truly gamopetalous at all.

3. That the relative paucity of gamopetalous species is much more marked in fossil than in living floras.

4. That the lower the horizon, the greater the disproportion in this respect.

5. That in all collections the number of *specimens* of fossil is less than of living *Gamopetalæ* in proportion to the number of species; *i. e.*, there were then not only less numerous forms, but those that existed were less abundant.

These several propositions were supported by copious citations and by tubular exhibits compiled from Schimper's "Paléontologie Végétale," Heer's "Flora tertiaria Helvetiæ," Lesquereux's Cretaceous and Tertiary Floras of the United States, and other works, as well as by data obtained from the collections of fossil plants at the United States National Museum.

A REVISION OF THE GENUS CLEMATIS OF THE UNITED STATES.

By JOSEPH F. JAMES, of Cincinnati, Ohio.

[ABSTRACT.[1]]

THE paper contains descriptions of all the species of Clematis native to the United States. Following the descriptions are remarks relative to them and the following changes are suggested in the nomenclature. *Clematis ovata* Pursh is reduced to a synonyme of *C. ochroleuca* Aiton; *C. Fremontii* Wats, is reduced to a variety of *C. ochroleuca* Ait.; *C. coccinea* Engelm. is referred back as a variety of *C. Viorna* L.; *C. Pitcheri* Torr. and Gr. is considered a variety of *C. Viorna* L.; *C. Catesbyana* Pursh and *C. pennsylvanica* Donn are considered synonymes of *C. virginiana* L.; *C. holosericea* Pursh is considered var. *bracteata* D. C. of *C. virginiana* L. Following the descriptions is given the geographical distribution of each of the species, and after this remarks on the probable place of origin of some of the species. The paper concludes with a synonymy of the species.

ACHENIAL HAIRS AND FIBRES OF COMPOSITÆ. By G. MACLOSKIE, of Princeton, N. J.

[ABSTRACT.[2]]

THE surface of the achenes, the character of their hairs, and their internal structure, are used to indicate the affinities of the suborders, according to the following tabular scheme.

I. Vernonieæ; as in Asteroideæ.

II. Eupatorieæ; "

III. Asteroideæ; achenes bearing duplex hairs, which are usually bifurcate at tips, acute, and often unequal. They have no elaters. Hairs sometimes few or obsolete; but even in such cases rudiments of duplex hairs may occur. Some are as in Inuloideæ.

IV. Inuloideæ; duplex hairs, usually obtuse and equal. No elaters.

V. Helenioideæ; as in Asteroideæ. Crystalloids occur in the endocarp.

VI. Anthemideæ; achenes usually glabrous: but having pericarp-cells with spiral filaments (Achillæa has internal glands in pericarp-cells.)

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[1] Printed in full in Jour. Cincinnati Soc. Nat. Hist., Vol. vi, July, 1883.

[2] Published in full, with illustrations, in the AMERICAN NATURALIST for Jan., 1883.

VII. Senecionidæ; duplex hairs, having divisions equal, with elaters on filaments which escape when moistened.

VIII. Calenduleæ; probably as in Senecionidæ (in some cases with multicellular hairs interposed).

IX. Arctotideæ; as in Asteroideæ.

X. Cynaroideæ; some (*Carlina*, *Xeranthemum*) as in Asteroideæ; some (*Cnicus*, etc.) as in *Cichorieæ*. Some have simple hairs of perianth encroaching on achene (*Arctium*, *Centaurea*, *Echinops*).

XII. Mutisieæ; as in Asteroideæ.

XIII. Cichorieæ; achenes glabrous, with denticulate epidermal cells. Endocarp having filaments enclosing crystalloids. These by pressing the seed-vessel of dandelion cause it to dehisce and protrude the seed.

These observations suggest a somewhat different line of affinities from that usually adopted. The Asteroid group should include most of the suborders. Anthemideæ are misplaced: and the tribal unity of Cynaroideæ is questionable, one section of its genera agreeing as to their achenes with Cichorieæ.

SOME OBSERVATIONS ON THE ACTION OF FROST UPON LEAF-CELLS.

By C. E. BESSEY, of Ames, Iowa.

[ABSTRACT.]

THE severe frosts of May 22 and 23, 1882, in Central Iowa, gave me the opportunity of making some important observations on the action of frost upon the cells of the young leaves of certain trees. The leaves of the Green Ash (*Fraxinus viridis*) were just developing, and were in that tender state peculiar to such young structures. Many of the leaflets were entirely killed, while perhaps the great majority were simply blackened, that is, they were killed near the apex, while toward the base they were still alive.

The examination of thin sections of these browned leaves soon after the frost, say within forty-eight or sixty hours, showed that the cells were differently affected by the low temperature. Instead of all the cells being equally injured, some were found to be dead, and in these the protoplasm was brown and shrunken; in others the contents showed more or less of injury by a greater or less coagulation of the still green protoplasm; others, again, appeared to be scarcely injured.

The disappearance of the chlorophyll granules, as distinct bodies, was one of the most common phenomena in the less injured cells. In many cells, which apparently were still alive, the colored protoplasm appeared to have retracted itself from the outer cell walls, toward the inner part of the cell, that is, the part of the cell lying toward the interior of the leaf. This was more particularly noticeable in the palisade cells of the leaf section.

Some of these leaves were affected by the Ash-leaf Rust, the so-called *Æcidium fraxini* of Schweinitz, forming small orange-yellow spots in the parenchyma or the fibro-vascular bundles of the ribs. The leaf-tissues are always thickened in these spots so that the thickness of the leaf in them is double or treble that in the normal portions. Under the microscope the tissues of the spots are seen to be penetrated by ramifying hyphæ of the parasite, which also extend for some distance into the surrounding, still green, tissues.

After the frost, I noticed within a few hours, that the cells immediately surrounding the orange spots were somewhat translucent, giving early evidence of their having been more injured than the adjacent outer tissues. Upon waiting a day or two, I observed that these translucent cells turned black and began to decay. Sections made both in the earlier and later stages showed that in this surrounding belt of cells the protoplasm had been much more severely injured than in those lying farther away from the spots.

The observations here recorded appear to indicate that in many plant cells there is the power of resisting (however slightly) the injurious effects of low temperature, and that in this respect there is a difference between cells which occupy, so far as we may see, similar positions, and which are equally exposed to the action of the frost. Moreover, it appears to be demonstrated that the lowering of the vitality of cells, as by the presence of parasitic hyphæ, lowers, also, their power of resisting low temperature.

The importance of these observations consists in furnishing *experimental* proof of the power of living plant cells to resist destruction by low temperature; hitherto the doctrine has been held on theoretical grounds mainly.

THE MOTIONS OF ROOTS AND RADICLES OF INDIAN CORN AND OF BEANS. By W. J. BEAL, of Lansing, Mich.

C. DARWIN showed that a piece of gummed paper on one side of the tip of a root in damp air caused it to bend out of its course. At the Cincinnati meeting I showed that roots in damp air often made coils without anything being placed on the tips. In germinating some 700 kernels of yellow dent corn in the soil, not one root made a coil. The roots seldom made very abrupt turns in any direction.

In a cool, damp cellar in August, kernels of Indian corn were planted with one end uppermost and the surface of sand pressed down smooth. They were placed in sand about three-fourths of an inch below the surface. The plumules all turned up and came out of the soil. Primary and secondary roots came out of the sand and often run along on the surface for a foot or more. The roots often entered the sand again after coming out. When planted with the *apex* of the kernel uppermost, secondary roots came out most numerous; often two or three to each plant. When planted with the radicle end uppermost the primary root often came out of the ground. Of 150 kernels with the radicle end down three sent the primary root out of the sand.

Of 181 kernels where the radicle was planted uppermost, eighty-nine sent primary roots out of the sand. Whether out of the sand or in it, the primary roots for a foot or more always kept ahead of the secondary roots. The roots apparently grew just as well out of the sand as when beneath the surface. Those above the soil soon became red. When planted in sandy soil, in open garden, with radicle down, secondary roots often came to the surface, where they perished. When planted with the tips or radicles uppermost, about ten per cent sent the primary root out of the soil.

Lima beans planted with the hilum down all came up well. After some time many seeds of Lima beans with the hilum uppermost sent some portion of the germinating plant out of the ground. Five out of eighty seeds soon perished when planted in the cellar with the hilum uppermost.

In the open garden, nine out of twenty-five sent the roots and lower end of plumule out of the soil, when the seeds were planted with hilum uppermost. These seedlings soon perished.

OBSERVATIONS ON THE FERTILIZATION OF YUCCA AND ON STRUCTURAL AND ANATOMICAL PECULIARITIES IN PRONUBA AND PRODOXUS.

By C. V. RILEY, of Washington, D. C.

[ABSTRACT.]

THIS paper records some recent experiments and observations which establish fully and conclusively the fact that Pronuba is necessary to the fertilization of the capsular Yuccas. It describes for the first time how the pollen is gathered and collected by the female Pronuba. The act is as deliberate and wonderful as that of pollination. Going to the top of a stamen she stretches her tentacles to the utmost on the opposite side of the anther, presses the head down upon the pollen and scrapes it together by a horizontal motion of her maxillæ. The head is then raised and the front legs are used to shape the grains into a pellet, the tentacles coiling and uncoiling meanwhile. She thus goes from one to another until she has a sufficiency.

My observations confirm the accuracy of Dr. Geo. Engelmann's conclusion as to the impotence of the stigmatic apices in some of the Yuccas and show how the apparently contradictory experience of Mr. Meehan can be reconciled on variation in this respect in the species of the same genus. The exceptional self-fertilization in *Yucca aloifolia* — the only species in which it is recorded — is shown to be due to the fact that in the fruit of this species, there is no style, the stigma being sessile and the nectar abundant, filling and even bulging out of the shallow opening or tube. The flowers are always pendulous and the pollen falling from anthers, under favorable circumstances, readily lodges on the nectar.

The irregularity in the shape of the fruit of the Yuccas — considered a characteristic by botanists — is proved by experiments to be due to the punctures of Pronuba.

The egg of Pronuba which averages 1.5 mm. long, having a swollen apical end and a long and variable pedicel is passed into the ovarian cavity. The puncture is made usually just below the middle of the pistil on the deeper depression which marks the true dissepiment, or through the thinnest part of the wall. The horny part of the ovipositor reaches the longitudinal cavity at the external base of the ovule near the funiculus, without, as a rule, penetrating or touching the ovule itself, and the delicate and extensile oviduct then conveys the egg for some distance (the length of six or eight ovules) along the cavity, the terminal portion of the oviduct being

furnished with retrorse hairs which help to hold it in place during the act.

The paper concludes with some studies of the internal anatomy of Pronuba and Prodoxus.

THE HIBERNATION OF ALETIA XYLINA (SAY), IN THE UNITED STATES A SETTLED FACT. By C. V. RILEY, of Washington, D. C.

[ABSTRACT.]

I HAVE already shown in previous remarks before the Association that there were various theories held by competent men — both entomologists and planters — as to the hibernation of this Aletia (the common cotton worm of the south); some believing that it hibernated in the chrysalis state, some that it survived in the moth state, while still others contended that it did not hibernate at all in the United States. I have always contended that the moth survived within the limits of the United States, and in this paper the fact of its hibernation, principally under the shelter of rank wire-grass, is established from observations and experiments made during the past winter and spring. The moth has been taken at Archer, Fla., during every winter month until the early part of March when it began to disappear, but not until eggs were found deposited. The first brood of worms was found of all sizes during the latter part of the same month on rattoon cotton, while chrysalides and fresh moths were obtained during the early part of April.

The fact thus established has this important practical bearing:

Whereas, upon the theory of annual invasion from some exotic country there was no incentive to winter or spring work looking to the destruction of the moth, there is now every incentive to such action as will destroy it either by attracting it during mild winter weather by sweets, or by burning the grasses in which it shelters. It should also be a warning to cotton growers to abandon the slovenly method of cultivation which leaves the old cotton stalks standing either until the next crop is planted or long after that event, for many planters have the habit of planting the seed in a furrow between the old rows of stalks. The most careful recent researches all tend to confirm the belief that Gossypium is the only plant upon which the worm feeds within the cotton belt; so that in the light of the facts presented there is all the greater incentive to that mode of culture which will prevent the growth of rattoon cotton, since it is questionable whether the moth would survive long enough to perpetuate itself upon newly sown cotton except for the intervention of the rattoon cotton.

EMULSIONS OF PETROLEUM AND THEIR VALUE AS INSECTICIDES. By C. V. RILEY, of Washington, D. C.

[ABSTRACT.]

THE value of petroleum for the destruction of insects has long been recognized and I have for years been endeavoring to solve the question of its safe and ready use for this purpose without injury to plants. The paper contains the results of extended experiments carried on under my direction, by several of my assistants and particularly by Prof. W. S. Barnard, Mr. Jos. Voyle of Gainesville, Fla., Mr. Clifford Richardson, assistant chemist of the Department of Agriculture, and Mr. H. G. Hubbard, who has for over a year been devoting his time to practical tests in orange groves at Crescent City, Fla.

Passing over the ordinary methods of oil emulsions by phosphates, lactophosphates and hypophosphites of lime and various mucilaginous substances, experience shows that, for the ordinary practical purposes of the farmer and fruit-grower, soap and milk are among the most available substances for the production of petroleum emulsions.

Ordinary bar soap scraped and rubbed into paste at the rate of 20 parts soap, 10 parts water, 30 parts kerosene and 1 part of fir balsam, will make, when diluted with water, an emulsion stable enough for all practical purposes, as the slight cream which in time rises to the surface, or the flakiness that often follows is easily dissipated by a little shaking. Soap emulsions are, however, less satisfactory and efficient than those made with milk. Emulsions with milk may be made of varying strength, but one of the most satisfactory proportions is 2 parts of refined kerosene to 1 part of sour milk. This must be thoroughly churned (not merely shaken) until a butter is formed which is thoroughly stable and will keep indefinitely in closed vessels and may be diluted *ad libitum* with water when needed for use. The time required to bring the butter varies with the temperature and both soap and milk emulsions are facilitated by heating the ingredients. Ordinary condensed milk may also be used by thoroughly stirring and beating it in an equal or varying quantity of kerosene.

The diluted emulsion when prepared for use should be finely sprayed upon the insects to be killed, its strength varying for different insects or plants and its effect enhanced when brought forcibly in contact with the insects.

Of mucilaginous substances, that obtained from the root of *Zamia integrifolia*, a plant quite common in parts of Florida, and from the stems of which the Florida arrowroot is obtained, has proved useful as an emulsifier.

These petroleum emulsions have been used with success by Dr. J. C. Neal, of Archer, Fla., against the Cotton Worm, without injury to the plant, but their chief value depends on their efficacy against the different scale-insects which affect Citrus plants. Experience so far shows that such plants do not suffer from its judicious use, but that it must be applied with much more care to most deciduous fruit trees in order not to injure them.

A NEW SEXUAL CHARACTER IN THE PUPÆ OF SOME LEPIDOPTERA.

By J. A. LINTNER, of Albany, N. Y.

[ABSTRACT.]

THE sexual character of insects have always been of much interest to entomologists, even when, as in most insects, they are well-marked, and much more so when they are quite obscure and only to be discovered by study and comparison.

Many of the features seem to be directly dependent upon sex, while others apparently serve no special purpose. A number of these are mentioned in the paper.

It would not be expected that sexual features should be as numerous and as marked in the earlier stages of insects as in the final stage. We find them rarely in eggs. Sex clearly exists in

the larval stage, although it is but seldom shown in external characters. The caterpillar of *Thyreus Abbotii* is generally believed to show distinct sexual forms.

I have heretofore pointed out sexual characters in the anal segment of some *Sphingidæ.* In this paper attention is called to a very interesting feature indicating the sexes of the pupæ of the *Cossinæ*, and of such *Ægeridæ* as have been examined. These species, living as larvæ and pupæ in the interior of plants and trees, are provided with a pupal armature of two transverse rows of teeth or spines upon most of the abdominal segments, which serve, at their time of final transformation, to release them from their burrows and partially to extrude them from the trunk.

In the male, the tenth segment (not counting the head as one), or the antepenultimate, *is furnished with two rows of teeth, while the female uniformly has but one* (as have the two following segments in each sex).

Another interesting feature of this armature is, that it discloses excellent characters for the determination of species.

ON AN EGG-PARASITE OF THE CURRANT SAW-FLY, NEMATUS VENTRICOSUS. By J. A. LINTNER, of Albany, N. Y.

[ABSTRACT.]

IN the twelfth Report of Dr. Fitch on the Insects of New York, he refers to the discovery of this interesting and efficient parasite made by me at Utica, N. Y., in the year 1866. Two years later, at Schoharie, N. Y., I again collected the insect, and by confining individuals in a glass supplied with newly deposited eggs of the currant-fly was able to observe the manner of its oviposition, placing two or three of its eggs within one of the currant-fly.

The specimens then obtained were destroyed before they could be satisfactorily studied and determined. From that time until the present year (1882) no more examples could be found although meanwhile faithfully sought for.

In June last, it was rediscovered in my garden at Albany, N. Y., and from another garden in the same city, a large number of leaves were collected bearing the parasitized eggs of the currant-fly,—

at once recognized by their brown shades in their early stage, and by the pupal form later.

Packages of the egg-bearing leaves (containing from two hundred to three hundred eggs each) were at once made up, and sent to entomological friends in several of the States and Canada, with the request that they be pinned upon currant bushes containing eggs of the currant-fly, in the hope that the valuable parasite might thus be extensively and more rapidly distributed than if left to its natural distribution. Several parasites of other insects have already been distributed in this manner, and it is hoped that the present effort will prove to be successful, and aid in checking the ravages of this well-known and great pest of our gardens.

The specimens of the parasite obtained by me — exceedingly minute insects, barely exceeding the hundredth of an inch in size and belonging to the family of Chalcididæ—prove to have been described by Professor Riley recently, in 1878, as *Trichogramma pretiosa.* It had been obtained by him from eggs of the cotton-worm moth, *Aletia argillacea*, collected in the southern states. Another species of the genus, *T. minuta*, parasitizes the eggs of one of our butterflies, *Limenitis disippus.*

OBSERVATIONS ON THE ELM-LEAF BEETLE, GALERUCA XANTHOMELÆNA. By G. MACLOSKIE, of Princeton, N. J.

[ABSTRACT.]

ITS ravages at Princeton 1880–1882. The adult winters in cellars and attics of houses: comes out early in May: ovipositing: form and development of ova. The larva described and its habits. The pupa, its form and habit of lying concealed in crevices of bark and about the base of trees. Coming-out of the beetles: their forms and habits: $\frac{1}{4}$ inch long, with orange elytra having two longitudinal black bars. Sucker-bristles on tarsus: heart seen beating through scutellum: mode of folding wings (1) longitudinally along main ribs (2) obliquely at the tip (3) obliquely at internal angle near base. The folding effected by the tension of the elastic chitine.

Powers of flight moderate (slow progress in extending to new localities).

Second course of eggs on second crop of elm-leaves. Probably three broods in the season.

Damage to the trees. Efforts to counteract this, (1) by destroying beetles in their winter refuge, (2) by kerosene on the bark of the trees (it kills leaves) when the larvæ are travelling, (3) by collecting and destroying the pupæ while changing, (4) by entrapping the beetles with old carpets, etc., (5) by sprinkling with London purple (1 lb. in 100 gallons of water). This mode is recommended by C. V. Riley.

No advantage from wrapping the stems in tarred cloth nor from leaving lighted lamps near the trees at night.

ON THE GALL-MITES, PHYTOPTUS. By WM. A. BUCKHOUT, of State College, Pa.

[ABSTRACT.]

IN the spring, galls caused by Phytopti may be found upon the leaves of the white maple as soon as they have expanded, and a careful examination shows the galls in process of formation while the leaves are still folded within the bud. The mites are upon the outer, *i. e.*, the under sides of the leaves, which when in the bud are very hairy. The irritation caused by the mites produces an abnormal multiplication of vegetable cells and the gradual arching upward of that part of the leaf, thus making a gall which differs from the galls produced by insects in that it has an opening below, which is frequently lined with hairs. Though generally on the upper surface of the leaf they are sometimes on the lower. These result from stray mites that have worked their way around to the upper side. The gall is always on the side opposite to that upon which the mite begins its attack. Each gall contains at first but one mite. The galls enlarge with the leaves and white globular eggs appear. A gall eventually becomes tenanted by a large number of mites. These migrate to all parts of the tree. Some produce (generally on the under side of the leaf) the growth of hypertrophied hairs called Erineum. The mites live and breed in this growth as they do in the galls. Later they seek winter quarters, and may be found in the crevices of the bark in the maple, and between the scales of the buds in the pear. The hairiness of the bud scales of many trees makes detection of such small objects as gall-mites very difficult.

The gall-mites when fully grown are of a narrow cylindrical form and average ¼ to ⅙ mm. long. All variations in size can be accounted for by difference in the age of different individuals, and by the ease with which they change form. There are two parts to the body, cephalothorax and abdomen; the latter is minutely striated, the former smooth, and bears the legs and mouth appendages.

The mouth parts are a pair of blunt pointed mandibles held closely together, between which is a blunt body, perhaps the maxillæ, generally retracted, but occasionally protruded.

Eyes are wanting. The legs are four, placed far forward, close together, and alike in size and shape. Each leg is five-jointed, the second joint the longest. There are one or two curving bristles at certain joints. The terminal joint is made up of a finger-shaped process to which is opposed a feather-like body of extreme tenuity having a central rachis and five barbs.

The abdomen comprises seven-eighths of the body, is closely striated, and very uniform except at the free extremity which is smooth and sucker-like. There are seven pairs of bristles attached to the body; one pair at the base of the second pair of legs, one pair on the back anteriorly, two pairs on the side of the abdomen anteriorly (but at slightly different planes), one pair on the side of the abdomen posteriorly, one pair long, and one pair short, anal bristles; each bristle grows from a small tubercle.

There is a large clear space in the anterior part of the body cavity of an adult and behind it are two great masses of eggs, lying close to the abdominal walls, leaving a more or less clear central channel. In its hermaphrodite character the resemblance of Phytoptus to the Tardigrades is very marked, although the testes have not been made out. In external form there is resemblance to Demodex and Pentastoma.

The gall mites walk with considerable ease using the legs as an ordinary quadruped does.

The gall mites are adult, and have no further change.

In all the galls and abnormal growths examined by me, the mites were essentially the same, and no difference worthy of being ranked specific can be found.

The blister spots of the pear and the malformed flower clusters of the black ash, while not technically galls, are abnormal growths caused by peculiarities resident in the plants themselves, and not in the mites.

The cases in which gall mites were found upon berries of the grape where they bred in the minute roughened and cracked spots in the pedicels, no galls, erineum or malformation being produced anywhere in the vicinity, make plain the fact that galls are not the necessary sequence of the presence of these mites, but that so-called gall-mites are widely scattered over the cellular tissue of plants in which they may produce malformations of various kinds, or none at all, according to circumstances.

The following is a list of the trees and plants on which mite-galls and other malformations caused by Phytopti have been found by me.

1. White Maple (*Acer dasycarpum*)
    a. galls, irregularly rounded, inner wall roughened by protuberances, medium sized (3–5 mm.)
    b. erineum, crimson, under side of leaf.
2. Red Maple (*Acer rubrum*)
    a. galls, reddish, rounded, small (1–2 mm.)
    b. erineum, crimson, under side of leaf.
3. *Sugar Maple* (*Acer saccharinum*)
    galls elongated (3–5 mm. long), pointed, interior roughened.
4. Black Cherry (*Prunus serotina*)
    galls, large (5–8 mm. long), obovate, tapering to a narrow stem, generally curved, and splitting lengthwise.
5. Wild Yellow Plum (*Prunus Americana*)
    galls as in *P. serotina* but smaller, more slender.
6. Cultivated Pear (*Pyrus communis*)
    "Blister spots," at length blackened.
7. Linden (*Tilia heterophylla*)
    galls, elongated, pointed (3–5 mm.) interior densely covered by long, curved unicellular hairs.

8. Black Ash (*Fraxinus sambucifolia*)
flower clusters, malformed, producing a cauliflower-like mass.

9. Willow (*Salix*———sp.)
galls, small, numerous, clustered, reddish.

10. American Beech (*Fagus ferruginea*)
erineum, yellow, upper side of leaf (but one specimen examined).

11. Poison Ivy (*Rhus toxicodendron*)
galls, small, numerous, clustered.

12. Button Bush (*Cephalanthus occidentalis*)
galls, small, numerous, clustered along the veins.

13. Sour Gum (*Nyssa multiflora*)
galls, small, numerous, clustered, becoming corky.

14. Yellow Birch (*Betula lutea*)
erineum, reddish green, upper side of leaf (but few specimens found).

15. Red Birch (*Betula nigra*)
galls, small, numerous, rounded, interior smooth or slightly granular.

16. Speckled Alder (*Alnus incana*)
galls, small, rounded, reddish.

17. American Elm (*Ulmus Americana*)
galls, medium sized, elongated, cylindrical, curved, interior filled with long, curved, unicellular hairs.

18. Cultivated Grape (*Vitis*———sp.)
slight protuberances of the pedicels of the berries shelter colonies of gall-mites, which wander over the whole vine.

19. Hornbean (*Ostrya Virginica*)
galls, small, numerous, rounded.

20. European Alder (*Alnus glutinosa*)
galls, small, rounded, reddish.

A few leaves were received from Finland, through Mr. Franklin B. Hough.

ON THE MOUTH OF THE LARVA OF CHRYSOPA. By WM. SAUNDERS, of London, Ont., Canada.

[ABSTRACT.]

THE author refers to the form and tubular character of the jaws, the manner in which the chrysopa larva seizes and holds its victims, the aphides, and its method of feeding as seen under the microscope.

At the base of each of the mandibles the integuments are dilated into a sac-like form capable of expansion and compression at will, a portion of the thorax is similarly constructed and it is by the repeated dilating and compressing of these sacs that the fluid contents of the body of an aphis is transferred through the tubular mandibles to the stomach of the chrysopa larva.

THE CLASSIFICATION OF THE UNGULATA. By EDWARD D. COPE, of Philadelphia, Pa.

[ABSTRACT.]

THE name Ungulata was used in this paper for the placental mammalia with enamelled teeth, which are furnished with hoofs. Thus defined, it includes the existing groups Artiodactyla, Perissodactyla, Proboscidia and Hyracoidea; and the Amblypoda, Toxodontia and Condylarthra of extinct types. The material of the latter groups now known enables us to understand some of the primary characters of the orders and to perceive the reason of the order of their appearance in time.

Especial attention was devoted to the arrangement of the bones of the carpus and tarsus. Two types of carpus can be recognized. In the first, the trapezoides supports the scaphoid exclusively; in the second, it supports the lunar also. In the first, the magnum supports the lunar, in the second it supports a part of the cuneiform also. To the former type belong the Condylarthra, the Proboscidia, the Amblypoda and the Hyracoidea; to the latter the Perissodactyla and the Artiodactyla.

In the tarsus, three divisions are also easily distinguished. In the first the astragalus articulates only with the navicular, and the cuboid with the calcaneum only. This includes the Condylarthra and the Hyracoidea. In the second, the astragalus unites with the navicular only, and the cuboid with the navicular and the calcaneum. This includes only the Proboscidians. In the third division, the astragalus unites with both cuboid and navicular, and the cuboid with the astragalus and cuboid. Here come the Amblypoda, the Perrissodactyla, and the Artiodactyla. Collating the relations of both carpus and tarsus, we have then four divisions, each characterized by peculiarities as above indicated. The nomenclature of these divisions it was proposed should be as follows:

Order I. TAXEOPODA; suborders *Hyracoidea* and *Condylarthra*.
Order II. PROBOSCIDIA.
Order III. AMBLYPODA; suborders *Taligrada*, *Pantodonta* and *Dinocerata*.
Order IV. DIPLARTHRA; suborders *Perissodactyla* and *Artiodactyla*.

The Amblypoda is the only one of these orders which has no recent representatives. The Condylarthrous division of the Taxeopoda is the oldest group.

The order of evolution has been as follows: in the Taxeopoda the relations of the carpal and tarsal bones are in separate continuous series. In the Proboscidia and Amblypoda, the carpal series is linear, and the tarsal series alternating, but in different ways. In the Diplarthra the carpal and tarsal bones alternate with each other in the longitudinal direction, or form oblique series. It is evident that the alternating series form the most compact and effective structure, resisting strains much more strongly than that of the order Taxeopoda, or the linear arrangement. In the Proboscidia, we have the weaker structure of the forefoot, and a peculiar but stronger one of the posterior foot. In the Amblypoda we have partly the same structure of the forefoot, while the posterior foot has the interlocking character of the hinder foot of the Diplarthra.

Three points are here to be observed. I. That the weaker structure of the carpus and tarsus appears first in time. II. That the stronger structure appeared first in the posterior limbs. III. That the interlocking structure has greatly multiplied, while the linear has dwindled and mostly disappeared. Here is a direct connection between mechanical excellence and survival.

The appearance of the alternating type first in the posterior foot, is a fact parallel to the reduction of the toes in the hind foot earlier in time than in the fore foot. This I have suggested has been due to the greater force of the impacts and strains of the hind foot than the fore foot, since it is the limb which propels the body, while the anterior limb principally supports. The reduction of both fingers and toes has commenced on the inner side, and the second row of carpal bones has coincidentally with this event, been rotated inwards, with the obvious effect of giving a more alternating position to the digits. The change in the tarsus has been effected by the inward extension of the cuboid, which also gives greater support to the external, and subsequently to the penultimate toe.

THE FAUNA OF THE PUERCO EOCENE. By EDWARD D. COPE, of Philadelphia, Pa.

[ABSTRACT.]

THE paper on the fauna of the Puerco Eocene epoch included a general account of fifty species, mostly mammalia, of the oldest tertiary formation known. The author said he discovered the formation in 1874, in N. W. New Mexico, and that in 1876 it had been observed by Endlich and Holmes in southern Colorado. Its deposits reach a depth of 1000 feet, and consist of marls divided by beds of sandstone.

Of Reptilia, six species only had been found, one tortoise, one crocodile, and three species of Champsosaurus. The latter genus forms a peculiar suborder probably of Lacertilia (lizards), and is characteristic of the underlying Laramie or Cretaceous No. 6 of America, and has been obtained from the lowest Eocene of Reims, France. The species were aquatic, according to Lemoine.

The Mammalia present many peculiarities. The Rodentia, Carnivora, and all the existing orders of Ungulata are wanting, and their place is taken by Marsupialia, Bunotheria and the ungulate suborders Condylarthra and Taligrada.

Five species of Marsupials have been found, which are partly related to the Plagiaulax of the Jurassic, to the Pliocene Thylacoleo, and existing kangaroos of Australia. The most remarkable are the two species of Ptilodus, a genus which has also been found in France with the Champosaurus. *Catopsalis pollux* was larger than any existing kangaroo.

Of the supposed Bunotheria, some may be Marsupialia. They include two species of lemuroids, seven species of Creodonta (flesh-eaters) two of Tillodonta, one of Tæniodonta and two of Insectivora. The lemuroids (*Adapidæ*) belong to genera of the Wasatch epoch, while of the creodont genera, only one (*Didymictis*) belongs to that epoch, and three are peculiar. One of these is Sarcothraustes, which represents the flat-clawed group of the Wasatch and included a large powerful species (*S. antiquus*) as large as a black bear. Deltatherium and Triïsodon are very opossum-like Creodonta. The Tillodonta belong to the genus Psittacotherium, which is provided with four powerful cutting rodent-like incisors in the lower jaw. It was a nutcracker or bark stripper. Of the insectivora, Conoryctes with two species is the peculiar genus. It has well developed canines, and probably lived on hard substances.

The suborder Condylarthra is found only in this formation and in the Wasatch, and is represented in the Puerco by two families, the Phenacodontidæ and Meniscotheriidæ and twenty-five species. The former family has simply tubercular, the latter crested teeth. The suborder is more nearly allied to the Hyracoidea (conies) of Asia and Africa, than to any other, and with that and probably some other groups should be combined in a special order, the Taxeopoda, which differs alike from its nearest allies, the Amblypoda and the Proboscidia. Besides Meniscotherium and Phenacodus, there have been referred to this order six genera with eleven species, which are only known from jaws and teeth. Some of these have some of the cusps of the inferior molars compressed, so as to serve the sectorial function of a flesh-eater. This is especially the case with *Haploconus*. Periptychus was at first supposed to be a creodont. It has the teeth remarkably sculptured. *Pantolambda bathmodon*, the size of a sheep, is the largest species, and is an aberrant Amblypod.

In review, it appears that of the fifty known species of the Puerco epoch, all are peculiar to the formation. Three belong to a genus of the Laramie epoch. Ten belong to six genera which are also found in the Wasatch epoch. One (*Hyopsodus*) ranges to the Bridger. Of the sub-orders only two (*Marsupalia*, *Insectivora*) still exist, and three, including eighteen species, are not found above the Eocene. Two other Eocene sub-orders are represented by a very few species in the lowest Miocene.

ON A RECENT SPECIES OF HETEROPORA FROM THE STRAIT OF JUAN DE FUCA. By J. F. WHITEAVES, of Ottawa, Ontario.

THE genus *Heteropora* was constituted by De Blainville in 1830, for the reception of certain erect and more or less branching Polyzoa, whose calcareous skeleton is composed essentially of slender tubes of comparatively large size, each of which is surrounded by a single row of smaller ones. The larger tubes, which are usually circular in section, are universally admitted to represent the polyp-cells proper, — but the functions of the smaller ones, which Mr. Busk calls the interstitial canals and which are often angular in section, are by no means clearly understood. It has been ascertained, too, that the smaller tubes at least are provided with cross partitions, that both kinds are furnished with radiating spines, and that the walls of both are perforated by numerous openings. These cross partitions and radiating spines have been held to be the homologues of the tabulæ and septa of the tabulate corals and the perforations in the walls of the tubes as analogous to the mural pores of the Favositidæ.

Lindstrom, in 1876, maintained that the Palæozoic fossils known to geologists under the names *Chætetes*, *Stenopora and Monticulipora* have almost exactly the same kind of internal structure as *Heteropora*, and that the former genera therefore should be removed from the class to which the true anthozoan corals belong and placed in that of the Polyzoa, a conclusion which had been arrived at ten years before by Dr. Rominger.

Many species of *Heteropora* have been described from the Mesozoic and Tertiary rocks of Europe and the United States, but no living representatives of the genus had been discovered until 1879. In that year Mr. Waters described and figured a recent species from Japan, under the name *H. pelliculata*, in the Journal of the Royal Microscopical Society, and a little later in the same year Mr. Busk published a diagnosis, with illustrations, of a living polyzoon from New Zealand, which he called *H. Neozelanica*, in the Journal of the Linnean Society. Mr. Waters and Dr. H. A. Nicholson, however, are both of the opinion that the *H. Neozelanica* is identical with the previously described *H. pelliculata*.

On the coast west of Sooke, Vancouver Island, in the Strait of Juan de Fuca, Mr. James Richardson, late of the Geological Survey of Canada, found a single specimen of a recent calcareous polyzoon, in 1874, which, in the writer's judgment, cannot be distinguished by any tangible character from the Japanese and New Zealand species of *Heteropora* described by Messrs. Waters and Busk. No thin sections of this specimen have been made to show the minute structure of the interior, but the whole of the outer surface has been carefully examined under the microscope and camera drawings have been made of some of the appearances thus presented. The punctured calcareous pellicle which Mr. Waters represents in his figures as closing the mouths of the interstitial canals in *H. pelliculata*, and to which the specific name refers, can be well seen in the Canadian specimen. On other portions of the latter the apertures of the larger tubes project distinctly beyond the general level of the surface, a feature which is not indicated on any of Messrs. Waters' or Busk's illustrations, but this slight variation from their types can scarcely be held as indicating a specific divergence or difference from them.

ON THE HABITS OF CRYPTOBRANCHUS. By BURT G. WILDER, of Ithaca, N. Y.

[ABSTRACT.]

LIVING examples of Cryptobranchus have rarely been brought to this country, and the following observations may be worth recording even if they merely confirm those of Dutch and German writers, whose works I have not yet been able to consult.

This specimen is about 75 cm. (two and one-half feet) long, and was obtained for Cornell University through Prof. H. A. Ward, who brought it by hand from Japan.

It is very sluggish, remaining quiet for hours in the water, excepting for the periodical respiratory movements presently to be described. Nevertheless, it can display considerable activity, and on one occasion escaped from a common wash-tub which was about 30 cm. (one foot) deep. Out of water it appears uncomfortable and crawls first in one direction and then in another, with frequent stoppages. It evidently seeks the shade, but gives no sign of discriminating objects, walking against dogs and cats and people as against wood or stone. The trunk is never lifted from the ground, and the compressed tail rests upon one side, but the head and neck are sometimes raised.

The respiratory actions in deep water I have not observed accurately; in shallow water just covering it, the nostrils are raised above the surface at frequent intervals, a hissing sound is heard, and after the nostrils are again carried below the surface, a few bubbles of air escape therefrom, and there are muscular movements about the neck. During an hour, in freshly changed water, these respiratory motions occurred at intervals varying from half a minute to twelve minutes, but usually the time was from two to four minutes.

No notice is taken of raw or cooked beef or fish, either floating at the surface, lying at the bottom or suspended just above the water. But if bits of food are dropped close to the mouth, or allowed to slide over the top of the head or held at the lips they are readily snapped up and swallowed if not too large. After a time, the head was moved slowly toward meat held about 1 cm. from the lips; I could not determine whether sight or smell were the sense concerned. Neither have I ascertained the functions of the tubercles.

This specimen has now eaten the following: beef heart, raw and boiled; blue-fish, raw and boiled; hard boiled white of egg; canned roast beef; raw lambs' liver, heart, diaphragm, lung and thymus; baked maccaroni.[1]

THE PLACENTAL DEVELOPMENT IN MAMMALS: A UNITY OF ANATOMICAL AND PHYSIOLOGICAL MODALITY IN ALL VERTEBRATES.

By HENRY O. MARCY, of Boston, Mass.

[ABSTRACT.]

RECENT investigations by a number of distinguished scholars have given this subject a new and exceptional interest. Deductions have been formulated of fundamental importance not only to the physician and the pathologist, but also to the comparative anatomist and scientist. The text-books repeat almost unchanged the teachings of twenty years ago. The value of comparative anatomy in clearing up intricate questions pertaining to the structures of the human species has been generally underestimated; especially is this true in reference to the human placenta. The older masters of the Italian schools (Phillipo Ingrassius, Fabricius, Malphigi) studied with great care many phases of placental development in mammals. The modern investigations of Owen, Huxley, Turner and especially of Prof. Ercolani of Bologna are of the first importance.

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[1] According to Prof. D. S. Jordan, the prior generic name of the great Japanese salamander is *Megalobatrachus*. *Menopoma* should be replaced by *Cryptobranchus* and *Menobranchus* by *Necturus*.

The subject of this abstract died May 25, 1883. Beside the skin and skeleton, preparations of the brain, heart and other viscera are preserved in the museum of Cornell University.

The simplest form of placental development is found in the diffused placenta of the solipeds.

In the non-gravid uterus of the mare, "the mucous membrane is smooth and velvety, of a rose color inclining to yellowish, and here and there it is doubled over into large and soft folds. In the gravid uterus, on the contrary, the folds are wanting and over the whole surface of the mucous membrane are scattered vermiform projections winding closely against each other, of a very intense deep red color tending to violet, and the surface has a swollen appearance."[1]

Vertical sections of the uterus, in an advanced stage of gestation, show in the place of the mucous membrane, which in the non-pregnant state is scarcely distinguishable, a uniform layer from one to two millimetres thick, which upon superficial examination appears like a transformation of the uterine mucous membrane. Into this the villi of the chorion enter and are somewhat uniformly distributed over the whole uterine surface. "This so-called tumefied mucous membrane has its origin from the sub-epithelial connective tissue and consists of an infinite number of simple glandular follicles, and may very properly, as designated by Ercolani, be considered as a new glandular secretive organ. The villi of the chorion are enclosed by these introflexions and follicular depressions, instead of entering as was earlier supposed the uterine utricular glands. These glandular follicles are supplied with an internal epithelium and are developed only during gestation.

The careful study of the multiple placenta of the Ruminants teaches much of value. Fabricius thought there was a direct communication between the vessels of the mother and the fœtus. Harvey, on the contrary, maintained that the fluid secreted by the cotyledons was absorbed by the villi. This view was sustained by Needham and Haller.

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[1] Ercolani, Utricular Glands of the Uterus, etc., page 85.

Ercolani has demonstrated that upon the site of the rudimentary cotyledons, which are never wanting in the non-pregnant state, there is developed during pregnancy a new organ of glandular secretory character and that this differs from the diffuse placenta chiefly in being developed over circumscribed areas and in possessing a more complex condition.

The study of the single placenta is beset with far greater difficulties. A transverse section of a gravid uterus of the rabbit at the placental site, the second week of pregnancy, shows three parts each clearly distinct.

1. The muscular wall of the uterus.
2. The placental neoplasm which is developed upon the inner uterine surface.
3. Above this is the old uterine mucous membrane tumefied and about to undergo destructive changes.

Two most noteworthy conditions appear.

The utero-placental vessels have a lumen almost double the vessels from which they proceed, and notwithstanding their greater volume they show in their walls none of the anatomical characteristics, which serve to distinguish the arteries from the veins and which are clearly visible in the uterine vessels.

The second and even more important fact is that these vessels, instead of the ordinary walls, are surrounded with a uniform envelope of cells of a special character, which cannot be distinguished from the cells of the decidua serotina and the maternal placental tissue. The special characteristic of the walls of the vessels, composed as they are seen to be of only the endothelium, is an objection to the supposition that their development could proceed from the vascular network of the old transformed mucous membrane. This is emphasized by the exceptional fact that the old uterine mucous membrane is detached from the muscular wall, and with its glands is undergoing a process of rapid destruction. These facts place it beyond all doubt that the utero-placental vessels and their surrounding cells are the result of a real neo-formative process.

In all animals having a single placenta, the decidua vera and the decidua reflexa are of new and distinct formative character. The decidua vera is composed of newly formed cells which are early arrested in their development and superimposed upon it is the old uterine mucous membrane, usually easily distinguished by its glands, and which later is seen in process of destruction and interblending between the external layer of the ovum and the internal uterine wall.

In most animals the newly formed placental vessels have a uniform diameter and constitute a network of small meshes which reaches as far as and is firmly joined to the chorion.

In the meshes of this network, are found the fœtal vessels which unite and enter the cord. By injection of these we have a better demonstration of the relations established between the fœtal and maternal portion of the placenta.

A careful study affords convincing proof that the umbilical arteries ramify and terminate in a thin fine network of capillaries in the chorial villi and are in close contact with the cellular covering which clothes the maternal vessels. Demonstrative evidence of this relationship in the human placenta has been frequently observed by myself, where the placenta has been undisturbed in its connections with the uterus for a considerable period after the death of the fœtus.

The chorial villi become shrunken and easily separate from the decidual sheath, the cells of which remain unchanged.

In certain of the lower animals (*e. g.*, the *Cavia cobaya* or guinea pig) Ercolani has observed that the maternal vessels become dilated in their terminal loops. This is of especial importance since it furnishes an example in rudimentary condition, of that which takes place in an extreme degree in the human placenta.

The destructive changes in the uterine mucous membrane of woman after conception are limited to the epithelial layer. After the separation of the decidua vera from the internal uterine surface, it is observed that the muscular wall is covered with a mucous membrane identical with that lining the uterine cavity before impregnation. The uterine mucous membrane is little more than a simple layer of epithelium and the reproduction of this is all that is required to restore the internal surface of the uterus to its primary state. During the early period of development of the human placenta, the decidual cells form a richly vascular compact layer surrounding the chorial villi. For a limited time the relationship between the parts is not so intimate that they cannot be separated. At this stage, before the maternal vessels become ectasic, the form of development is not unlike the diffused or disseminated placenta.

The walls of the proliferating branches of the villi are necessarily forced against the decidual layer of cell covering the ectasic, thin walled maternal vessel. Thus this is introflected and a close union ensues. In this manner the so-called placental lacunæ are formed in the fully developed placenta, and, from this very deceptive appearance, it has been held as a demonstrated fact, that the fœtal villi swim in the maternal blood.

The epithelial covering of the vascular loop of the fœtal villus is wanting because of the intimate relation which has been established between it and the secretory villus, which *never* loses its own epithelium. The form of the vascular loop of the maternal villus varies greatly. In the quadrumania and in woman the dilatations become lacunose and are actually enormous.

All the vertebrates during the period of embryonic life, in order to complete the marvellous phases of their development, require a special nourishment which is always furnished them by the mother. This is conveyed to and converted into its own substance by the embryo, and however many and considerable may be the differences met with, in these two fundamental factors, the unity running through them readily appears under two general forms, represented by the yolk of the egg in oviparous animals and by the placenta in mammals. In the first, the maternal aliment is stored up in a mass by the mother and emitted with the ovum in the quantity needed by the embryo to complete its development.

In the second, the nutritive material is furnished by the mother from the placenta which elaborates it as needful for the developing embryo. These materials in every case are absorbed and conveyed to the fœtus by means of its own vascular appendages. Thus nutrition is carried on during the development period in all vertebrates by a single law of physiological modality.

The typical anatomical form in the two fundamental parts of the placenta, however wide the variation, is always identical:—a villus of new formation maternal as well as fœtal, each composed of an internal vascular loop, surrounded by a cellular layer and covered with an outer epithelium. The office only of the villi is different, the fœtal *absorptive*, the maternal *secretive*. These villi always come in contact more or less intimate, but the walls of the vascular loops never in any instance touch. This is the more to be emphasized since it invalidates the idea hitherto held, and yet almost universally taught, that the mode of the nutrition of the fœtus is an interchange between the two bloods by endosmosis and exosmosis. The different ways of the interblending of the fœtal and maternal villi give the complex and confusing picture of placental formation. The belief that the lacunæ were really large cavities as they certainly appeared, and not the maternal vessels greatly dilated, was held as a truth proved and indisputable, and it was through this belief that two other deceptive appearances were received as actual truths, namely, that the villi floated in the maternal blood and that the epithelium covering them appertained to the fœtus instead of the mother.

THE MORPHOLOGY OF ARTERIES. By FRANK BAKER, of Washington, D. C.

[ABSTRACT.]

THE arterial system is remarkable for irregularity and variability; yet an attempt should be made to bring it under morphological laws.

Ontogenetic development shows that the embryo has at first no vessels. These arise by vacuolation of cells which unite to form capillary tubes commencing at the periphery of the area vasculosa and approaching the heart. Uniting, these form trunks along lines of axial growth. This *centripetal* development is to be considered when attempting to interpret the laws which govern the arterial system.

Phylogenetic development confirms and supplements this evidence. The lowest animals possess no proper vessels, but obtain food by protoplasmic imbibition. The alimentary canal may ramify in the sarcode, curiously resembling a vascular system, but analogous only. The water-vascular system formed by a split between the two layers of the body wall is possibly the origin of the lymphatics. The blood-vascular system commences by longitudinal trunks. The movements of the body being more frequent in the direction of the mouth tend to develop such trunks. In higher forms these are supplemented by transverse trunks corresponding to the somatomes. In animals having limbs the limb-trunks show a law of uniform dichotomous division relating to the segments of the limb. If we admit Gegenbaur's theory that limbs arise as buds upon which other buds form in a radiate manner, the arterial ramification can be explained. Some of the arterial trunks belong to aborted rays. When arteries do not divide at the normal point they have a tendency to do so at the node next nearest the heart. This is caused by variability of convergence. One artery may take the place of another by inter-substitution. Functional activity may greatly modify the vascular system. In the intestinal canal (splanchno-pleure) protected from external influences, the cell-life is simple and there has been less differentiation than at the periphery (somato-pleure). The primitive type is more nearly preserved there, and the vessels resemble a capillary plexus vastly enlarged.

The phenomena of ramification can be explained by reference to an ancestral archetype from which the arterial systems of all vertebrates are derived.

The paper was profusely illustrated by drawings and diagrams.

Illusions of Motion. By H. P. Bowditch and G. Stanley Hall, of Boston, Mass.

[ABSTRACT.]

I. It is a very old observation that when the eye has rested for a time upon a moving object or a series of moving objects and is then transferred to a stationary object the latter seems for a few moments to be animated by a movement in the opposite direction.

Examples of this phenomenon are furnished by watching a waterfall and then looking at the stationary bank, or by looking at the floor of a moving railway car after watching the apparent motion of the landscape without.

The term "subjective complementary motion" has been applied to illusions of this sort on account of the analogy which they present to complementary after images. Prof. S. P. Thompson who suggests the use of this term formulates the following law: "The retina ceases to perceive as motion a steady succession of images that pass over a particular region for a sufficient time to induce fatigue, and on a portion of the retina so affected the image of a body not in motion appears by contrast to be moving in a complementary direction." (See Quarterly Journal of Science, March, 1879.)

The observation of a revolving disk on which a spiral line is drawn affords perhaps the readiest means of studying this illusion, at the same time that it shows the inadequacy of the explanations which have been hitherto offered. If the disk be revolved in such a way as to produce a contraction of the spiral line towards the centre, an apparent expansion is observed when the gaze is directed to a stationary object. Now, if the stationary object thus viewed be a sheet of paper on which a straight line is distinctly ruled, and the eye be directed to a point near but not upon this line, the surface of the paper appears to be animated with a movement of expansion from the point looked at, while the line does not appear to alter its straight direction. In other words, the field of vision as a whole appears to expand, while the separate points in it do not alter their relative position in regard to each other. This is best explained on the supposition that the apparent expansion is a phenomenon connected with the visual centres in the brain, and not with the optical or mechanical apparatus of the eye.

II. The apparent rotation of a set of concentric circles when a "rinsing" movement is imparted to them, as described by Thompson, may be partly accounted for as follows. When a set of concentric circles is moved to and fro across the field of vision, that portion of the circles in which the direction is the same as that of the motion will be mostly distinctly visible. The blurred figure of the circles will thus be divided by two narrow sectors in which the lines will be distinctly seen. Now, when a "rinsing" movement is given to the circles, the direction of the movement is continually changing, and consequently the narrow sectors in which the lines are distinctly seen will appear to move around the circles and thus cause the appearance of rotation. Another factor in the production of this phenomenon is the apparent rotation round a point in its circumference which is produced whenever a circle is carried with a rinsing movement through a circle equal to itself.

III. The apparent slow rotation of a circle with internal cogs in the direction opposite to that of the "rinsing" movement, a phenomenon also described by Thompson, appears to be due to the fact that the end of the cog is most distinctly seen when it is in that portion of its course which is most distant from the line from which it arises, for here it is least obscured by after-images of the line. The phenomenon can be equally well observed when two horizontal lines with cogs directed towards each other are subjected to a rinsing movement, the two lines of cogs appearing to move in opposite directions. These experiments will be reported in full in an article which will shortly appear in the Journal of Physiology. (See Journal of Phys. III, 297.)

DEMONSTRATION OF A SERIES OF BRAINS PREPARED BY GIACOMINI'S METHOD. By WILLIAM OSLER, of Montreal, Canada.

[ABSTRACT.]

THE brain (human), as fresh as possible, is placed in saturated zinc chloride solution, in which it floats, so that it has to be turned or covered with cotton wool soaked in the fluid. On the second day the membranes are removed. This is the only tedious part of the process, as the dissection must be carefully made about the nerves. After having been in the solution for about a week, the brain is transferred to alcohol (methyated spirits) in which it sinks so that cotton wool must be placed beneath to obviate the effects of pressure. The organ can remain in spirit an indefinite time, but after six or seven days is ready for the third stage of the process,—immersion in glycerine (best), in which it floats. It remains in this until it sinks to the level of the fluid which usually takes a week. It is then removed, the surplus glycerine washed off and the organ exposed to the air in a protected place. When dry it receives a coating of ordinary varnish and remains unchanged for years. The method is applicable to the brains of small animals, and the stages of the process are shorter.

CROSS HEREDITY FROM SEX TO SEX. By Mrs. A. B. BLACKWELL, of Somerville, N. J.

[ABSTRACT.]

THERE is much evidence that each organic cell and organism assimilates food, builds structure, and performs functions under the same law of equal action and reaction that rules inorganic nature and maintains the balance of work among correlated energies. Thus, each higher organism is differentiated to perpetuate, not another organism like itself, but incomplete elements, the incident forces, which, coöperating with complementary elements, can establish the new organism. Hence a permanent moving equilibrium of male and female influence in the new being; and, by the law of averages, each determines the sex in about equal numerical proportions. There is also a well known family resemblance among brothers and sisters which underlies even unlike physical details and various mental ability, and is often an obvious balance of the dissimilar characters of the parents. Mixed races are the conspicuous blending of both types of progenitors.

But, since special characters are entailed to the same sex exclusively, then a corresponding inheritance of more general characters should be entailed to the opposite sex distinctively, if the balance is maintained.

Several classes of facts were given by the writer in evidence of cross inheritance.

With feeble mothers and strong fathers, daughters, at birth, have an apparent advantage over their brothers; but if the inequality is reversed in the parents, the sons begin life better endowed than their sisters.

If parents are strongly marked contrasts, the sons most often resemble the mother and the daughters the father. When this is not readily evident, intimate acquaintance will often bring out cross inheritance in some other form of compensation.

Idiosyncrasies and traits independent of sex are most often reproduced by cross heredity.

In mixed races, Indian and negro, white blood is presumably a male inheritance, and the women as a class are certainly lighter and more Caucasian in features than the men. But in special cases, where the mother is white, the male children more often

resemble the whites. The same law holds in all mixed nationalities to a noticeable extent.

There are complications arising from the general blending of many characters, and therefore room for difference of conclusion; yet, if attention can be definitely called to the facts, it is believed that the compensative law of cross inheritance, from sex to sex, may be definitely established.

THE ORGANIC COMPOUNDS IN THEIR RELATIONS TO LIFE. By LESTER F. WARD, of Washington, D. C.[1]

[ABSTRACT.]

ORGANIC compounds may be defined as "substances whose highly complex and very unstable molecules are composed of those of inorganic compounds or of organic compounds of lower organization, formed on the cooled surfaces of fully developed plants at life-supporting temperatures."

So far as the cosmical origin of the various substances composing the earth's crust, whether elemental or compound, is understood, it seems to have conformed to the following law: the molecules constituting each progressively more complex unit exhibit *increase of mass* accompanied by *decrease of stability.*

The artificial synthesis of organic compounds has obliterated the line formerly supposed to exist between the chemical constitution of inorganic and of organic compounds.

The properties of substances depend upon their molecular constitution: the more complex their constitution the more active their properties. Thus the properties of compounds are as a rule more active than those of elements, those of organic are more active than those of inorganic compounds, those of the alkaloids more active than those of the amyloids, and those of the albuminoids more active than those of the alkaloids. In the last case, however, the activity manifests itself in a different manner, viz., through rapid changes of internal structure including the phenomena of isomerism; thus partially ceasing to be molecular and in a certain way affecting the mass.

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[1] This paper was published in full in the American Naturalist, Vol. xvi (December, 1882) pp. 968-979.

These nitrogenous compounds themselves possess grades of complexity and instability, and thus we arrive in an ascending series at proteine and finally at protoplasm.

Protoplasm is a chemical substance whose relatively large molecules may have been compounded of those of the less complex albuminoids.

The activities manifested by protoplasm consist in actual alterations in the mass of the substance, which is the fundamental fact involved in the phenomena of life. Life is therefore essentially a property of protoplasm.

All the higher manifestations of life are reducible to protoplasmic activity, and the enlarged scale on which they operate is due to subsequent *organization*. A living being is only a quantity of organized protoplasm, and tissues are merely the framework and material machinery by means of which the life-substance is enabled to multiply effects.

CLASSIFICATION OF ORGANISMS. By LESTER F. WARD, of Washington, D. C.[1]

[ABSTRACT.]

THE terms *animal* and *vegetable* have proved wholly inadequate to express the distinctions which are found to exist among organisms. As popular terms they are useful, but as scientific terms they have already led to much fruitless discussion.

The fundamental distinction in biology should be drawn between those organisms which are capable of assimilating chemical or inorganic matter, and those which depend entirely upon the appropriation of matter already so manufactured. We would thus have two classes of organisms, viz., first, assimilators, tissue manufacturers, or *autogens;* and second, *parasites.*

The first of these classes might be subdivided into three groups: 1. Those that manufacture protoplasm only and consist entirely of that substance. 2. Those that manufacture both protoplasm and also some form of protective integument or framework. 3. Those which decompose carbonic dioxide and employ the carbon thus liberated as the strengthening material of their tissues, viz., plants proper.

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[1] This paper was condensed from the writer's work "Dynamic Sociology" (New York, D. Appleton & Co., 1883, Vol. 1, pp. 347–355), published since the Montreal Meeting.

The second class is also divisible into three groups: 1. Those which appropriate matters already manufactured by organisms of the first class (or, as in the case of lichens, take them at third hand from organisms of the same class), but which are not only fixed like plants but are of low organization and simple cellular structure. 2. True parasitic plants, whose organization plainly indicates that they have descended from chlorophyl-bearing plants, of which they are degraded types. 3. Animals proper, which live on the substance manufactured by the first class of organisms, either by taking it directly from plants, or by preying on others of their own group which have derived their sustenance from plants.

TITLES OF OTHER PAPERS READ IN SECTION F.

NOTE ON THE OCCURRENCE OF TRACES OF A NORTHERN FLORA IN SOUTHWESTERN OHIO. By E. W. Claypole of Yellow Springs, Ohio.

NOTE ON THE STERILITY OF THE CANADA THISTLE AT YELLOW SPRINGS, OHIO. By E. W. Claypole of Yellow Springs, Ohio.

INSECTS VERSUS FLOWERS IN THE MATTER OF FERTILIZATION. By E. W. Claypole of Yellow Springs, Ohio.

A SKETCH OF THE HISTORY OF OUR KNOWLEDGE OF THE BUDDING OF SALPA. By W. K. Brooks of Baltimore, Md.

FRITZ MÜLLER AND THE NAUPLIUS OF DECAPODS. By W. K. Brooks of Baltimore, Md.

DESCRIPTION OF A NEW GENUS AND SPECIES OF ALCYONOID POLYPS FROM JAPANESE WATERS, WITH REMARKS ON THE HABITS AND STRUCTURE OF RELATED FORMS. By Robert E. C. Stearns of San Francisco, Cal.

ON THE POLYMORPHISM OF LYCAENA PSEUDARGIOLUS. By W. H. Edwards of Coalburg, W. Va.

ACHAENODON FROM THE BRIDGER EOCENE BEDS. By Henry F. Osborn of Garrison-on-Hudson, N. Y.

AN EXAMINATION OF SOME CONTROVERTED POINTS OF THE PHYSIOLOGY OF VOICE; ESPECIALLY THE REGISTERS OF THE SINGING VOICE AND THE FALSETTO. By T. Wesley Mills of Montreal, Canada.

BLASTESIS TRIDENS; A PEAR TREE FUNGUS. By William H. Seaman of Washington, D. C.

PROGRESSIVE GROWTH OF THE DERMOID COAT OF THE MEMBRANA TYMPANI. By Clarence J. Blake of Boston, Mass.

REMARKS ON THE TURBELLARIA. By Wyllis A. Silliman of Clarkson, N. Y.

NOTES ON THE FLORA OF THE ROCKY MOUNTAINS OF IDAHO AND MONTANA. By Sereno Watson of Cambridge, Mass.

ON THE VENOMOUS PROPERTIES OF THE TOAD. By J. A. Crevier of Montreal, Canada.

NOTES ON SPIDERS. By Henry C. McCook of Philadelphia.

SECTION G.

HISTOLOGY AND MICROSCOPY.

ADDRESS

BY

PROFESSOR ALBERT H. TUTTLE,

VICE PRESIDENT, SECTION G.

FELLOW-MEMBERS OF THE SECTION OF HISTOLOGY AND MICROSCOPY :—

It is my esteemed and honorable privilege to preside over the first session of this body held with its present organization and standing as an independent Section of the Association.

In assuming the duties and the honors conferred upon me, I find my mind reverting to the antecedents of this Section and the days of its beginnings. Some, at least, among you will recall with me the daily gathering of a little company in an out-of-the-way room during the Salem meeting of the American Association in 1869, to which the name of "the Subsection of Microscopy" was first given.

Long shall I remember the sessions of that meeting, half formal, half informal. There we debated (or listened while others debated) gravely whether or no the "nineteenth band" had really been resolved. We triumphed unitedly in the clearness of the demonstration by one of the more expert of our number of the "basket-work" upon *Surirella gemma*,—*ultima thule*, then, to us of test-objects ; and some of us, at least, looked for the first time, at and through what was for us the crowning wonder of the day—a Wenham binocular ! We discussed there in simple faith the merits and demerits of "that quality in an objective termed penetration," without a shadow of doubt as to whether we agreed as to what we meant by the expression, or any dream of the latter-day skepticism as to the very existence of the "quality" we so earnestly discussed. We listened eagerly while the late Mr. Bicknell described the method by which he made his then unrivalled injections, the open secret of which appeared to be, when all was told, that the way to make good injections (or good anything else) was to take each step intelligently, carefully and correctly; and we gazed with interest, if not with admiration, at a demonstration, which some who were present then will perhaps recall, of how a microscope can be made to serve as almost anything that it was not intended to be, if you only modify it sufficiently, and add pieces enough.

This sounds like childish talk, but such was the infancy of the body that sits here to-day; and this, let me add, was in what might almost be called the childhood of microscopy in America. True, there were already in this country many excellent microscopes (including a due proportion of the wonderful binoculars) and a goodly number of excellent microscopists, not a few of whom had risen far beyond the level indicated by the scenes that I have described. True, much excellent research, some of which stands to-day as no less excellent, had been carried on in America long before the times of which I speak, by solitary workers, scattered here and there over the eastern states, the names of many of whom will be recalled by each of you as I speak; but that general interest in the microscope and its revelations which forms so large a part of the intellectual activity of to-day was as yet hardly known, and among those devoted to scientific and even to biological research the microscope was far too often an infrequent and unimportant auxiliary.

If microscopes were not as numerous then as now, neither were they as easily to be had. In that day he who wanted to buy a microscope had to hunt for it. To-day I had almost said that a man must be rather ingenious to avoid buying half a dozen. Then, he who wished an instrument from a foreign workshop had to make it the subject of a special order; to-day half a dozen importers carry extensive stocks of the instruments most likely to be in demand. Then, as regards American-made microscopes, if a stand was wanted, the buyer looked (and not unwisely) first of all, to a little shop in Philadelphia where labored and still labors—long may he do so—one, the thoroughness and fidelity of whose handiwork alone enable him still to hold his own against a score of competitors, who by hand and by steam are putting yearly upon the market countless instruments of every conceivable plan and of all grades of excellence. If one sought for objectives of the highest character he must perforce seek the atelier in Boston of one who then stood without a rival on the American continent. Though his work is, as I believe, yet to be surpassed, one may look to-day not unprofitably in three or four other directions to find makers of thoroughly excellent objectives; objectives, if you choose, whose available angle of aperture includes, as a still skeptical friend of mine dryly puts it, by far the greater portion of a circumference: while as regards low powers and moderate angles, the market now abounds with excellent American-made objectives.

I would not overlook the fact that there were other makers in this country at that time; men who made excellent stands, and men who made excellent objectives; but I am confident that I do no injustice to others in saying that the two whom I have indicated were the only ones at all widely known.

The increase I have indicated in the number of prominent makers, is, of course, directly associated with an increase alike in demand and in production; but who can realize the magnitude of that increase? The makers of that day counted their results by dozens; the yearly aggregate production of to-day must be told by thousands; and it can then hardly keep pace with the demand, in spite of the large additional importation of English, French and German instruments. Competition has, as usual, been followed by the two substantial benefits of improvement in quality and reduction in price. I think sometimes almost with regret of my first venture, recalling how little I then obtained for my scant eighty dollars, and seeing how much that is really excellent, both mechanically and optically, can be commanded by the beginner of to-day for the same or even a smaller sum. At that time, too, books, appliances of every sort, and, most valuable of all, personal hints and suggestions from those of experience, were scarce and hard to be obtained. To-day few communities are so small or so remote as not to furnish all three in varying quality.

Such, as I recall them, were the impressions made upon a young western teacher, himself then a beginner (and even now but little more), of the times and the sessions of the first Subsection of Microscopy of the American Association. The effort made at that time to bring together the workers with our favorite instrument was renewed from year to year at succeeding meetings, the movement gathering strength at Troy, at Indianapolis, at Dubuque, at Hartford, at Detroit, at Buffalo (where the Subsection acquired the dignity of a permanent organization), and at each succeeding meeting, until the result has been obtained which we witness to-day.

While the vastly increased interest manifested in America to-day in the microscope and its use must, of course, be chiefly attributed to that phenomenal movement in biological science which we have been so happy as to witness, and from which we have derived so many and so varied benefits, I think I may justly claim that no small credit should be given to the yearly meetings of the Subsection, with their usual exhibitions and soirees, giving to the people of the various communities in which we have met notice at least of the existence of such a thing as a microscope, with some scant glimpses of the wonders that it has to show; awakening here and there an interest that has ended in something more than mere curiosity-seeking and amusement; drawing together and uniting those of a common interest: to which last effect may, I think, be at least indirectly traced a result whose future offers great promise of good, the organization of the young and vigorous American Society of Microscopists; an organization in whose power it is to do a great amount of excellent work that could be done by no other means as well. The influence exerted in one way or another by our half score of sessions would, I think, if it could all be measured, far exceed in value the papers that have been read at them, though these have been neither few nor unimportant.

Such has been our prentice-work; we stand henceforth among our equals as a Section. It would be pleasant here to pass from recollection to prediction, and to spend the remainder of the half-hour in the discussion of great possibilities; we shall do otherwise, however, if we give heed to the pithy suggestion contained in an ancient but excellent volume of practical philosophy (with which I trust that most of you are familiar) concerning boasting and the putting on of armor.

And this the more wisely because another topic of a far different nature presents itself, and, I may almost say, demands attention.

Before telling what we intend to do, we are in a certain way called upon to defend our right to be. It is commonly known, I think, that not only in the Standing Committee of last year, but also throughout the membership of the Association, there have been felt and expressed grave doubts not only of the necessity but also of the propriety of raising the Permanent Subsection of Microscopy to any higher dignity: while the extension and associated limitation of its province which the new name of the Section connotes, though they have so far satisfied these objections as to lead to its establishment, have called forth from others even stronger criticism, as raising to the rank of a distinct department of science that which they had hitherto regarded as holding a far subordinate position. Respect for the candor and the ability of our critics, as well as for their zeal for the wise ordering of the affairs of the Association, would of itself demand that these objections be met and answered. I shall deal with them to the best of my ability; should my arguments fail to carry conviction, I trust that the weakness will be attributed to the advocate rather than to the cause.

To the question first referred to, it has been considered a sufficient answer to urge the fact, which will, I think, be universally conceded, that there is no other instrument of research known to scientific men that has attained so high a degree of development as the microscope; no other whose parts both mechanical and optical have been the subject of so much prolonged, earnest, and fruitful discussion; none which has so many and so varied accessory appliances pertaining to it, in relation with its almost endless diversity of uses. Furthermore, this high degree of development calls for a proportional degree of training in the worker. There is a great amount of study of the instrument itself demanded of him, side by side with an equally great amount of labor that may be regarded as gymnastic in its nature (in which we see the final cause of diatoms for all but cryptogamic botanists, and of ruled plates for all mankind); while the various processes by means of which objects of study themselves are brought into a suitable condition for examination and preservation call for no little skill and practice. It is urged, therefore, in view of the peculiar character and wide applicability of the instrument in question, and of the extent and arduousness of the discipline that its skillful use requires, that they who have in greater or less measure acquired this discipline are by virtue of that fact drawn together, forming a body of sufficient coherency to be entitled to independent standing as a Section of this Association.

I have endeavored to state the argument in favor of a Section of Microscopy briefly but fairly. Love of justice makes me anxious not to understate it, for, as many of you are well aware, I am one of those who have questioned strongly the propriety and wisdom of organizing such a Section; and I must confess that after an honest endeavor to put the case as strongly as possible in my own mind for those who think otherwise, and to convince myself of the truth, whatever it may be, I am still of the same opinion.

For I do not see what more we can say of Microscopy than that it is a technique; so elaborate, so complex, that its acquirement in any considerable measure is in itself a valuable discipline, but a technique still. No one feels more strongly than I its fascination; no one is more alive to the feeling of sympathy that unites the brotherhood of the tube. I have as lively and practical faith in the value of societies of microscopists and in journals of microscopy, as I have in other technical societies and periodicals. I believe, as I have already said, that they can under certain circumstances do a work for science that can be done in no other way as well; but I cannot see the propriety, in an Association whose aim is first, last, and always the advancement of science, of establishing a Section on a purely technical basis.

Nor, were its propriety conceded, would it, in my judgment, be either wise or expedient to do so, considering either the efficiency or the stability of such a body. The almost universal applicability of the microscope is in itself a bar to that unity of purpose which should be the first characteristic of a working organization. I can perhaps illustrate this to advantage by referring to another instrument of research which has attained in its various forms and uses a very high degree of development, and whose use calls for no small amount of training and experience: I refer to the polariscope. Were we to gather into one body all the chemists, physicists, mineralogists, and biologists who make more or less extensive use of this instrument in scientific investigation (to say nothing of the various groups of those who apply it extensively to the affairs of every-day life) we might make a Section of Polariscopists; but it would soon unmake itself.

I know that this will be regarded by some as a false analogy; and that appeal will be made to the prevalent idea — far too prevalent in my judgment, since it is largely responsible for that diffusion of energy whose practical exponent is the fact that the number of microscopes sold yearly in this country is in proportion to the number of valuable original contributions on microscopic subjects annually produced as over fifty to one — that the expert microscopist is he who, having a good stand and a number of good objectives which he can handle dexterously, has also a large cabinet of objects, mostly of his own preparation, illustrating as many of the various applications of his instrument as possible; one who, not specially a botanist, is at home with diatoms and algæ; not calling himself a zoölogist, has a certain familiarity with infusoria, entomostraca and rotifera; not claiming to be an histologist, has cut and stained numerous sections of both plants and animals; neither a geologist nor a mineralogist, has ground, polished and mounted many sections of rocks and corals; not a chemist,— but why extend the category? one, in fine, who, in accordance with a tradition whose history would be full of interest had we but time to trace it, has acquired skill enough and expended brains and labor enough in a dozen fields to have made himself a master workman in any one of them. I confess that I cannot but regard this idea as an idol of the tribe: to my mind the skillful microscopist is he whose hand has learned all necessary manipulations, whose eye has been trained to nice and quick distinctions, and whose judgment, resting on accumulated experiences, can interpret rightly the appearances presented to his eye: and he is most successful who having chosen a definite field of labor has worked most diligently therein to increase his own or the world's stock of knowledge.

I would not be unmindful of the fact that there are those who, working with the microscope, have entered many fields, and gleaned, nay, reaped richly in them all; but they are few: neither would I pass unkindly criticism on those who using the microscope avowedly only as a recreation find pleasure and substantial profit in passing from time to time to fresh fields and pastures new; I take it, however, that the Sections of this Association are organized primarily, if not solely, with the view of bringing together most closely those whose labors have most in common; and however it may be with objectives, we may be sure that for Sections of this

Association not only penetration but definition also is in inverse ratio to angular aperture.

It was therefore the judgment of the Standing Committee (and this judgment was ratified by the vote of the Association at large) that this Section should be established not purely nor indeed chiefly for that technique which the word Microscopy implies; neither should that name stand as the label for a receptacle for communications having only this in common that the objects of investigation are too minute or of too fine a structure to be examined by the naked eye. They have, instead, established a Section devoted to that department of science whose investigations must be wholly carried on by the aid of the microscope, and to the technique of the instrument as auxiliary to it. As in the Section of Chemistry papers discussing appliances and methods whereby a more accurate analysis or a more complex synthesis may be obtained find place and welcome, or in the Section of Physics discussions of means as well as of results of investigation are gladly received, so here the presentation and description of new apparatus, the discussion of new methods of preparation, of manipulation, or of demonstration find appropriate and ready audience; but I present as my opinion, which I hope will find echo in your judgment, that papers on subjects pertaining to micro-chemistry (if there be such a thing as micro-chemistry) will best be read to chemists: that papers in what are termed micro-geology and micro-petrography will find readiest and most appreciative listeners in the Section of Geology. I would even suggest that a paper on the entomostraca or the rotifera, particularly if it deals with the morphology or the systematic position of the form or group in question, will find its normal auditors among the zoölogists in the Section of Biology.

What then remains to us? I have spoken of that department of science whose investigations must in the nature of things be carried on wholly by the microscope, as being designated by the organic name of the Section as its particular field of labor. I say, that department of science; knowing that the expression must call forth that adverse criticism to which I have alluded and to which I must now address myself. It has been urged that Histology is in substance but a finer Anatomy; a dissection made with the eye where the scalpel cannot go; at best no better than the handmaid of Physiology: and that to dignify it with the rank of a distinct department of science is to give to the servant the honors due to the mistress.

This, I acknowledge, is the traditional view: and this, unfortunately, describes only too well the Histology that is too often studied and too often taught to-day: but I am persuaded that the beginnings have already been made of a newer and a greater Histology than this: the leaven that has leavened the whole lump of biology in the last quarter century is working here as elsewhere; and the resulting recombination of ideas has given us a conception of the significance of the phenomena of cell-life of which a previous generation hardly dreamed.

As a rich and fertile soil whose yearly harvests pay abundant rental for its tillage may long overlay unsuspected mineral wealth of far greater value, so it often happens that a discovery that proves itself at once of great practical importance may remain for a long time with its philosophical significance unrecognized. The results of that enquiry into the structure of the organs of living beings which the immortal discoveries and inventions of Lister and his contemporaries first made possible were found to have so much practical value to the physiologist that they were at once appropriated to his use. This analysis did not at first go beyond an idea of fibres, cells and membranes; it did not seek and only vaguely dreamed of any kinship between them; later, when their essential affinity was at first surmised and then demonstrated, the fact was, save by a few, regarded as of secondary importance to what had now come to be commonly known as Physiological Anatomy.

To-day, however, the situation is changing rapidly. The conception long since advanced by one of the greatest of living thinkers that the continuous processes of adjustment of relation which make up all we know of life are the resultant or algebraic sum of the activities of the structural elements which make up the living being, and that organs and organism alike must be regarded as the consequence of the integration of countless cells or nucleated corpuscles, is yearly gaining wider acceptance. Some one has said that the doctrine of the survival of the fittest is the equator of the sphere of biology: may I express the opinion that the conception to which I have referred will one day be for physiologists at least its prime meridian? However this may be, this conception, it will be readily admitted, has greatly enhanced the significance and the dignity of the study of cell-life.

There is another and even more important relation of Histology to Biology that is very apt to be overlooked by those that regard it as adequately described by the name of Physiological Anatomy. I refer to its relation to morphology, and its bearing on questions of affinity and classification. Were it proposed to establish in this Association a Section for Comparative Anatomy, or for Comparative Embryology, no one, I am sure, would for a moment question the propriety of such action in either case: I think I am warranted in the assertion that Comparative Histology stands to-day where the former did at the beginning and the latter at the middle of the present century, and in the opinion that the day is not far distant when its indications will be as confidently received and as diligently heeded as those of either of its allies. From a number of illustrations that occur to me I will take time to mention but two. You are all, doubtless, familiar with the homologies that were once regarded as established between the Trematoda and the Cestoda: you are equally well aware of the doubts that have been thrown upon them in later days by the failure to discover associated embryological resemblances: in my judgment these doubts will disappear in the mind of any one who will carefully study the histology of a tape-worm and of a fluke. As a representative of morphological problems requiring to the full as much aid from Histology as from Anatomy or Embryology, I would suggest the great one yet unsolved presented by the nervous axis of vertebrates; I am convinced that we shall wait for a clear and accurate conception of the morphology of the brain, and of the homologies existing between the various parts (our present ignorance of which we have buried beneath a mountain of terminology) of the widely differing brains of mammals, sauropsidians, amphibians, selachians, teliosts and marsipobranchs, until, beginning at the bottom of the series and ascending along diverging lines, we have worked out by histological methods the structure, development, position and connections, both primary and adapted, of every mass of gray matter in that galaxy of puzzles.

The claim of Histology to rank as a distinct department of science might well be allowed to rest upon the amount and the nature of its increased importance, both physiological and morphological, that I have indicated: there is another consideration in its favor, however, that I regard as far weightier. I have hitherto spoken of tissues and their activities as related to the functions and the structure of organs and organisms. I would now call your attention to the value of the study of cell-life considered in itself. We have most of us had our eyes opened for some time to the magnitude and the significance of the struggle for existence that goes on at all times between organisms; it is only recently, however, that our attention has been definitely called to the struggle of parts within the organism, and to the probable truth that every organ is where it is and what it is through a long series of adaptations to its environment. And this analysis must be carried still farther: for as the body is built up of its organs, so, as I have already said, is each organ the result of the integration of its cellular elements. The great law of biology that gives order and system elsewhere leaves no chaos here; and our Histology is not complete until every tissue and every cell has been studied in its light.

Here, then, is our province. Is it not ample enough? It embraces the study of cell-life, in all its bearings, in plant and animal alike. Since sound fundamental ideas of the laws which govern cell-life can only be obtained by prolonged and careful study of those plants and animals which consist but of single cells, or of undifferentiated social aggregates of cells, I claim for hearing here all papers relating to the Protozoa and the Protophyta, including particularly the ferment-organisms on account of their frequent pathogenetic function. Before us come, of course, all papers dealing with cell-life in the higher organisms; papers alike upon the morphology of cells and on higher morphological questions treated by histological method; papers alike on the development of cells and on the structure and significance of embryonic layers and tissues. The newer Histology is still the helpmeet if not the servant of Physiology; while large place will be given here to questions of cellular teratology—for what is Pathological Histology other than this? Lastly, but not leastly, we shall welcome here all papers and discussions calculated to help us either to better microscopes, or to a wiser use of those that we now have.

I have felt, for special reasons, that it was particularly incumbent upon me before presiding over the first session of the Section of Histology and Microscopy, to present to you my interpretation of the organization of the Section, and my idea of its future policy. Since this meeting has it in its power to do much to determine the future line of action of the Section, I trust that we shall all agree upon the one here presented or a better. Should your judgment approve of the view that I have taken, I am confident that you will join me in a modest expression of the desire that as in the past the Subsection of Microscopy did much to cultivate a widespread popular interest in the microscope, so in the future the influence of this Section may do much to concentrate and economize the energies of microscopists, and of the hope that as the parent Association represents the aggregate of American scientific research, so our little body may one day come to be the representative of all who labor with us in our chosen field.

PAPERS READ.

PLANT-"CELLS" AND LIVING MATTER.[1] By LOUIS ELSBERG, of New York, N. Y.

[ABSTRACT.]

THIS paper, illustrated by carefully prepared drawings, records the discovery of perforations in the cellulose walls of plant-cells giving passage to protoplasmic offshoots from the interior and demonstrates how in a plant, as in an animal, the living matter throughout is uninterruptedly connected.

THE THIRD CORPUSCULAR ELEMENT IN THE BLOOD. By W. OSLER, of Montreal, Canada.

[ABSTRACT.]

BY the third corpuscular element in the blood I mean certain small colorless discoid bodies $\frac{1}{3}-\frac{1}{8}$ the size of red blood corpuscle, quite common in the blood of mammals and about which much discussion is at present taking place. They circulate free in the blood with the other corpuscles, but in the withdrawn fluid tend to adhere together forming irregular clumps or collections. They correspond to Zimmerman's "elementary corpuscles," to the individual elements of Schultze's granule masses, to the Hæmatoblasts of Hayem and to the "Blutplatchen" of Bizzozero. Scanty in the blood of healthy adults, they abound in the blood of the young and in that of individuals in an anæmic or cachectic condition. The most suitable object for their study is the blood of the young rat, in the vessels of which they can be seen as isolated corpuscles.

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[1] Published in the Quarterly Journal of Microscopical Science, London, January, 1883, p. 87.

The paper considered, first, the possible connection which these bodies have with the development of the blood; and second, their influence in determining the formation of fibrin.

THE DEVELOPMENT OF BLOOD CORPUSCLES IN THE BONE-MARROW. By W. OSLER, of Montreal, Canada.

[ABSTRACT.]

A BRIEF resumé was given of the views current regarding the development of blood corpuscles.

The red marrow always contains a form of corpuscle, thought at one time to be peculiar in the mammal to the blood of the embryo, viz., the nucleated red cell. This is an intermediate stage between certain of the colorless marrow cells and the ordinary red corpuscles. The stages as traced by the author are (1) the colorless marrow cell becomes less granular, the protoplasm gets more homogeneous, (2), it becomes gradually tinged with hæmoglobin, the tint becoming progressively deeper until it may attain that of the ordinary corpuscle. The nucleus remains unaffected. In this form they constitute the well known nucleated red corpuscles. (3) The nucleus or nuclei gradually disappear, often first breaking up into smaller portions and the entire protoplasm of the cells is uniform and undifferentiated.

These observations of the mode of production of the red non-nucleated cell in the marrow accord in the main with those of Neumann. The view expressed by some authors (Rindfleisch *et al.*) that the nucleus migrates from the cell is believed to be incorrect. The appearances on which it is based are known to the author, but they are believed to be the result of physical changes.

NOTE ON THE MICROCYTES IN THE BLOOD AND THEIR PROBABLE ORIGIN. By W. OSLER, of Montreal, Canada.

[ABSTRACT.]

MICROCYTES are exceedingly tiny red blood corpuscles $\frac{1}{5000}$ to $\frac{1}{8000}$ of an inch in diameter which are met with (1) in the blood of embryos and of the new-born. (2) Occasionally, and in small numbers, in the blood of healthy individuals. (3) In certain diseases, particularly the severer forms of anæmia and (4) after severe hemorrhages.

No satisfactory explanation has been given of their origin. The author has, in the spleen tissue and in the bone marrow, seen their production by a sort of budding from the ordinary red blood corpuscles. In the conditions above mentioned, particularly after hemorrhages and in profound anæmia the red corpuscles often present great irregularities in outline and projections (Porkilocytosis of Quincke). In fresh specimens of spleen and bone marrow the author has seen these little projections become detached from blood cells — moving about in the currents in the slide — and when so detached they presented all the characters and appearances of the microcytes in the same field.

THE HISTOLOGY OF UTERINE FIBROID TUMORS. ILLUSTRATED BY A SERIES OF MICRO-PHOTOGRAPHS. By HENRY O. MARCY, of Boston, Mass.

[ABSTRACT.]

THE histological elements of the uterine fibroid tumor have usually been considered to consist either in very large share of connective tissue, or of a blending of connective tissue with muscular fibres; *i. e.*, a fibrous or a fibromyomatous growth. The objects of the investigations, of which the micro-photographs are shown in illustration, have been to determine more thoroughly not only the composition of these growths, but also their relations to the surrounding uterine tissue. It is scarcely pertinent to the objects of this meeting to enter into a detailed discussion of the pathological changes which ensue, but the subject is rather introduced to show the advantages of micro-photography and its easy reproduction by the heliotype and other similar processes. Its great value in histological and pathological teaching is undoubted.

The sections were made by the use of Dr. Hailes' freezing microtome and in their preparation and mounting, as well as photographing, I am largely indebted to the valuable assistance of Dr. Alfred F. Holt, of Cambridge.

The photographic work was done in the studio of Mr. Black, of Boston, an expert in this direction well known to America, and in my judgment challenges favorable comparison with even the justly celebrated work of Dr. J. J. Woodward of Washington.

The conclusions reached, as the results of these observations, are briefly: that the origin of these growths is not in reproduction of connective tissue cells, having as a point of departure a vessel or a nerve, but that it consists of an extraordinary development of true muscular fibres of the uterus, usually arising from several centres, resulting in a multi-lobular growth, and may consist of one or more independent tumors.

In their development, these fibres are more or less altered in character by compression, and when of considerable size, by lack of nutrition. The connective tissue stroma is not usually increased, but is often less than that of normal uterine tissue. The tumors are rarely supplied with vessels of any considerable size but depend for their nutrition in large share upon the blood supply of the network of surrounding vessels which is usually exceptionally developed and the vessels are often ectasic in character.

The capsule is composed of the muscular fibres of the surrounding uterine wall, its cells greatly altered by continuous tension and compression, and is not in any degree a proliferation of connective tissue.

Around the tumor there are usually several of these capsular layers which may be peeled or stripped off. They are so changed in their character that it is not remarkable they have been considered as made up from connective tissue.

On the contrary the tumor attachment to these layers, and to the more dense uterine structure, is very slight and is composed of delicate bands of connective tissue. The histogenesis of the myomatous tumor and especially of its relations to the unchanged surrounding uterine structures, in its surgical bearing, is of the first importance.

SOME VEGETABLE POISONS. By J. T. BURRILL, of Champaign, Ill.

[ABSTRACT.]

DURING several years I have occasionally noticed within the closed cells of apparently healthy plants of divers kinds, numerous actively moving particles which in form and motion were indistinguishable from some kinds of lower organisms we call bacteria.

Knowing that such cells have for their exterior a cellulose membrane in which there are absolutely no imperforations large enough to be discovered by the best and highest powers of our modern microscopes, the idea of living organisms gaining entrance to the cell and dwelling imprisoned there was further stifled under a presumed impossibility. This also must have been the case with many other botanists and microscopists, for the phenomenon is by no means rare, yet unrecorded so far as I know.

But the evidence is too strong to admit of further doubt that bacteria of several kinds do inhabit the cells of apparently healthy plants.

Some of the best instances observed by myself have been in the cells of certain mould fungi and especially in the fertile threads of *Ascophora mucedo*, a common black mould on damp bread, decaying fruits, etc. In the higher plants the most satisfactory cases have been in the cells of hairs through whose transparent walls everything within can be easily seen; but sections of tissues frequently give evidence of the same thing in the deeper layers.

These things, however, did not attract any considerable attention until it became manifest to me through a series of investigations, that bacteria cause *disease* in plants, especially such as we call "blight" in the pear tree, apple tree, etc. The older observations were then recalled, and upon further study, satisfactory evidence of the existence in plant cells of veritable living organisms, capable of propagation outside of the tissues was gained.

Surprising as these results are, they may not be more so than the known fact that bacteria occur in and on the bodies of healthy animals. In human beings they are found in the scurfy skin of the scalp, in the perspiration from the face, and especially in the epidermal exudations from portions of the body covered with clothing, in the mouth, the stomach and the intestinal tube, and sometimes in the blood. Some of these may perform important physiological functions and therefore may be essential to our well-being, while it is positively known that some specific kinds are our mortal enemies, constituting the active principle of pestilence and contagion.

Another circumstance called still further attention to the living organisms in plants. A lady was severely poisoned with *Rhus toxicodendron*. The exuding serum from the inflamed part, and the leucocytes from the miliary vesicles, were found to contain swarms of bacteria of the genus *Miccococci* as defined by Cohn. It was demonstrated that these organisms transferred from the inflamed to healthy parts of the body communicated the severe malady, that they rapidly increased in numbers and were a true *contagium vivum*.

Search was now made and similar organisms found in washings of fresh leaves and in the juices of the poisonous plant. To rigidly test the matter, a little of the milk sap was secured free from anything not contained in itself and diluted with distilled water to such an extent that the fluid had a slight milky appearance, yet was nearly transparent. A minute drop of this was placed on my own arm after marking the spot by clipping the hairs thereon. The strongest chemical caustic known would not thus cause any serious lesion of the skin, diluted as it was a hundred fold. No sensation was produced at the time, neither was there any marked indication of effect during ten hours following. But on awaking the next morning, eighteen hours after the application, the spot was evident from its redness, and there was a slight sensation of heat in the part. These symptoms rapidly increased, becoming strongly marked by the twenty-fourth hour, and at the forty-eighth my arm was much inflamed and considerably swollen. There was intense itching; the inflamed area increased until by the sixth day my whole arm was involved. After about ten days the malady had evidently run its course and gradually abated. The microscope showed the characteristic bacteria; these were again proved to transmit the trouble to a healthy part of the skin and there increase as before. Indeed nearly my whole body became more or less affected and the difficulty became chronic in certain places, lasting for months though less severe than the original inflammation.

There is positive proof that individuals are sometimes poisoned by simply approaching, without touching, this plant as well as some of its congeners. Here again, the only explanation is that the minute quantity of the poison received is capable of increase by growth. Having seen by the aid of the microscope a something from the plant, and also in the inflamed tissues, which does thus increase and which being placed on a healthy part reproduces itself and the disease, we conclude this something is the *vera causa* of the infection.

When the living organisms have gained access to the deeper layers of the skin, it is difficult to reach them by medicinal applications; but in other cases, any antiseptic substance tried proved quite efficacious. If a little diluted carbolic acid is applied to the skin soon after the poisonous juice has been experimented with, no evil consequences follow. Quinine seems to be an excellent remedy.

The organism is spherical, usually single, but often seen in twos, seldom in strings of four or more, about $\frac{1}{2}$ μ (.00002 inch) in diameter.

The above, though the best and clearest, case of plant poisons being due to bacteria is not the only one. There can scarcely be a doubt but that the ill effects of numerous species of different orders and classes are produced in the same way. I will mention only the poisonous fungi sometimes unwittingly used for food.

There are two pretty well marked modes of mischievous action from the use of such plants, operating either singly or combined. In one case the bad results are soon made known by pain and vomiting, in the other the unfortunate victim perceives nothing wrong for six, ten or more hours after eating the poisonous food. The first has been ascribed to a peculiar acrid, the second to a narcotic principle in the mushrooms. The former, though violent, is not nearly so dangerous as the slower action of the second. When both act together, or rather one follows the other, peril is imminent.

The first symptoms of the latter kind are usually a heaviness of the head, a confusion of ideas, an inclination to stupor; these are succeeded by sickness at the stomach, vomiting and purging and a burning thirst. Convulsions follow, cold perspiration appears and death occurs either in the agonies of pain or in a profound and hopeless stupor.

Four persons died in this way in Brazil, Indiana, in October, 1881, from fungi cooked and eaten at noontime, Oct. 16th. Two died in the afternoon of the eighteenth and two on the twenty-second. The last, a strong man, was able to walk about the room until a few hours before his death. Towards the last there was no particular pain; but decomposition evidently set in before death.

I relate these terrible accounts to show what effects may follow the use of such poisonous plants for food, yet I have myself eaten in quantity, and have known many others to eat, of the very same species which caused these deaths in Indiana. The plant is a very large parasol-like fungus sometimes found in great abundance in pastures and meadows (*Lepiota Morganii* Peck). In the making of catsup it is well known that all sorts of mushrooms are collected and used for the purpose without ill effects. I was once asked to taste of some supposed mushrooms which had been put into strong vinegar some weeks before and which, thus pickled, had been eaten with nothing but pleasant remembrances of the fine flavor; yet these were certainly poisonous species. The vinegar had also been swallowed without injury, showing that the poison was not simply extracted; was it not killed? Is not the immunity mentioned above explained better by the cooking than by anything else? The fungus just named belongs to a very suspicious group and it was only by careful experiment that I came to eat it and then only after thorough cooking. To this it is usual to answer that the poison is volatile and is driven off by heat; but why so tardy in its action followed at length by such fearful results? That living micro-organisms exist in such fungi is certain; do they not in some instances cause the mischievous results? The bad effects occurring soon after swallowing the material is quite surely due to a poisonous substance and it is still doubtful what effect cooking may have upon this; but the consequences are never so severe as with the slower acting so-called narcotic effect.

Upon a careful perusal of the exaggerated injuries ascribed to the famous Upas tree there does not seem to me any other theoretical interpretation of the facts so suggestive as this of the action of living organisms tenanting its tissues.

NOTES ON SOME OF THE STRUCTURAL AND PHYSIOLOGICAL PECULIARITIES INCIDENT TO THE DISEASES OF FRUITS. By D. P. PENHALLOW, of Mountainville, N. Y.

THE facts contained in the following notes have been obtained as the joint results of investigations conducted on the one hand by Dr. C. A. Goessman, of Amherst, Mass., who has examined the subject from a strictly chemical standpoint; on the other hand by myself, using the microscope and viewing the question from a botanical standpoint. My object in presenting these notes to the Association is not to bring forward any well established conclusions, but rather to offer a report of progress upon work which demands much time and critical examination before it will be safe to present anything more definite.

Our work has been largely confined to studying the peculiarities of peach trees affected with the yellows, and the following remarks will bear directly upon that subject alone.

In his studies upon this subject, Dr. Goessmann has shown from ash analyses, the relative importance of certain mineral constituents in the vegetable economy and the relation which they bear to a healthy and diseased condition[1]. The following analyses, calculated upon a basis of 100 for the five constituents given, will show the importance of the relations first referred to, comparing the fruit and branches of the healthy, with those of the diseased tree.

Fruit of Early Crawford.

	Healthy. Per cent.	Diseased. Per cent.
Ferric oxide	0.58	0.47
Calcium oxide	2.64	4.68
Magnesium oxide	6.29	5.48
Phosphoric acid	16.03	18.07
Potassium oxide	74.46	71.30
	100.00	100.00

Branches of Early Crawford.

	Healthy. (Restored)	Diseased.
Ferric oxide	0.52	1.44
Calcium oxide	54.52	64.23
Magnesium oxide	7.58	10.28
Phosphoric acid	11.37	8.37
Potassium oxide	26.01	15.68
	100.00	100.00

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[1] Observations Regarding the Yellows of the Peach, C. A. Goessmann, Ph. D. Trans. Soc. for Prom. of Agricultural Science, 1882, Montreal.

From these analyses, the relative importance of calcium and potasssium oxide is shown in a striking manner; a decrease of the former in healthy structure corresponding very closely with an increase in the latter.

Our studies conducted with the aid of the microscope have served to throw much light upon this subject, and not only have they confirmed the deductions from a chemical standpoint, but they appear to be in accord with the earlier investigations of Nobbe and Schroeder on rye and buckwheat, during which they demonstrated the superior value of chloride of potassium over the sulphate, as establishing in the plant a normal distribution of assimilated material to the growing parts.

Growing upon diseased plants, and often producing by their action, some of the more conspicuous features of the disease, are fungi of various forms. These, however, are such as may be generally found upon decaying organic matter, such as old stumps, and do not belong to that class of fungi generally considered capable of immediately producing disorganization in highly vitalized structures. Moreover, while found upon the outer dead bark, and, under favorable conditions in the interior structure, the same forms occur on both the diseased and healthy trees and what is more striking, they also are to be found upon trees which once badly diseased are now restored to a condition of vigorous health. We are led to believe, therefore, that while these parasitic growths do produce certain well defined effects under certain conditions of the plant, their relation to disease is of secondary importance; we must seek the cause of disorder in conditions of soil and food.

An examination of the cellular structure and cell contents has shown:—

1. Healthy wood shows comparatively little stored starch in the bark.

2. Diseased wood shows an abnormally small development of the cells and the invariable presence of large quantities of starch in the bark and pith.

3. There is little or no difference between the tissues and cell contents before and after the leaves fall.

These conclusions appeared justifiable at a comparatively early stage of our inquiry and have been abundantly confirmed by subsequent examinations. A general predominance of and correspondence between calcium oxalate and stored starch in diseased structures, seems to offer another confirmation of the chemical deductions previously referred to.

It has been customary, until a very recent date, for students to approach the subject of diseases of plants from a strictly botanical standpoint, giving little or no weight to other considerations. The old view, stoutly maintained by some even at the present time, that these diseases originate in fungoid growths, gave rise to the application of certain remedial measures, the aim of which was to arrest disease by destroying the parasite. No successful case of such treatment has yet been recorded.

The new method while recognizing the important connection of parasites with disease, aims first to promote a highly vigorous condition in the plant and thus enable it to throw off the unwelcome guest. Badly diseased trees, treated upon this principle, have been restored to vigorous health and for the past two or three years have borne fruit commanding the best market prices.

From this brief statement and review of the case, it will be apparent that in the future, we must not depend upon either the microscope or the chemist alone, but if we are to obtain a correct solution of these difficult and most important questions, we must unite by rational method the deductions of the chemist with those drawn from microscopic study.

A NEW FORM OF CONSTANT PRESSURE INJECTION APPARATUS. By WILLIAM LIBBEY, JR., of Princeton, N. J.

In studying the circulatory system, the means of injection are an indispensable aid. This operation can be performed in several different ways, but the most satisfactory method would of course be that which would in the simplest manner utilize the materials used and at the same time place them under the most perfect control of the operator.

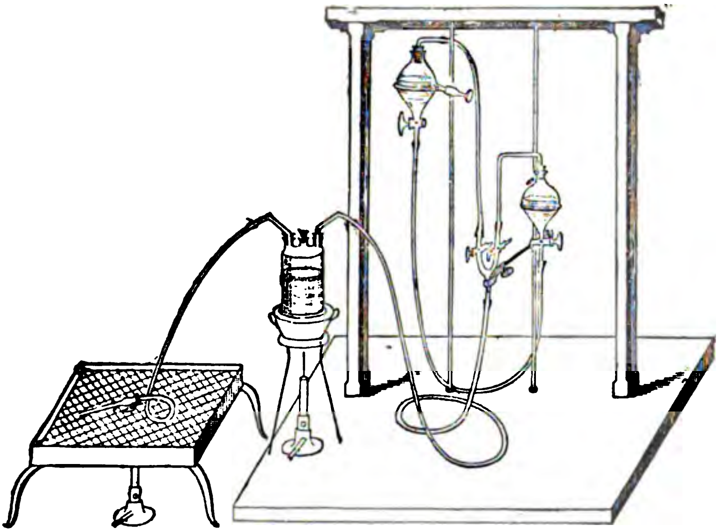
In the piece of apparatus described below, the wants of my laboratory have been especially consulted, and the object kept in view was the construction of a machine which would perform the most delicate injections in a satisfactory manner. In a sense, it may be said that this object has been realized; but, as the element of good judgment can never be made part of a machine, the degree of success will still be in proportion to the skill in manipulation. All such machines must have a certain similarity in their construction, but may differ essentially in the arrangement of their several parts, and for this reason no great originality is claimed for the apparatus; the method of the operation in the machine under consideration is as follows:—

We will suppose the animal or organ to be injected to have been prepared as usual and placed over a water bath arranged to secure a proper degree of temperature during the operation; and, the injection mass having been also previously prepared and placed in a three-mouthed wolffs-bottle (prepared as a wash-bottle with closely fitting rubber corks) which should, of course, be placed over a water-bath to keep the mass liquid.

The source of the power utilized is twenty pounds of mercury which, by changing position from a higher to a lower level, forces the air out of one of two glass filtering funnels alternately, as one placed below or raised above the level of the other which should be kept stationary. These two filtering funnels are supported on iron rings with clamps which can be attached to two iron rods at any given height. The iron rods should be about thirty-six inches high being held in their places by a framework that rises on the back part of a wooden platform which rests upon the table (or may be built into the wall, or placed between two shelves far enough apart in a closet in the laboratory) and serves as a convenient place for any accessory apparatus or instruments. The funnels, referred to above, are connected at the bottom of each by a rubber tube, and have stop-cocks above the points where the tubing is attached. The opening at the upper part of these funnels is closed with rubber corks through which a glass tube is placed after having been once bent at right angles.

The means of securing the operation of the currents of air in such a manner as to perform their works properly is an arrangement of three-way-stop-cocks of a peculiar construction. These stop-cocks are each so constructed that when turned in one position they have the form of a regular spigot, while by a quarter turn from that position, a straight connection is made through the body of the stop-cock to a tube opening at that point.

Two of these stop-cocks are placed, one on either of the upward branches of a glass tube having the form of the letter Y. This arrangement affords the means of transmitting a current of air under pressure through that stop-cock which may be turned so as to open a straight connection through it, when that branch of the Y-tube of which it forms a part has been connected with the glass tube at the top of the filtering funnel, from which the air is forced by the flow of the mercury to it from the other funnel which should occupy a higher position. It also affords the means of allowing a current of air to flow to the surface of the mercury in the higher vessel, through the other stop-cock which should be turned so as to afford ingress to the air; this will be secured when the stop-cock is placed in a position at right angles to that of the first described stop-cock. This branch of the Y-tube should be connected with the tube at the top of the other filtering funnel.


A Constant Pressure Injection Apparatus.

The lower branch of the Y-tube should be connected with the short arm of the bottle containing the injection mass. All the joints made with the tubing should be securely wired and all the other connections made as tight as possible.

The Y-tube described above should be securely supported by extension test-tube clamps, attached to the iron rod, to which the stationary filtering funnel has been fixed, and below the funnel.

The long arm of the bottle containing the injection mass should be attached to the nozzle-stop-cock which fits the canulæ to be used in the injection.

Now we shall suppose that the mercury has been placed in the stationary filtering funnel and this latter firmly fixed a little above the middle point of one of the iron rods. The stop-cock of this funnel should be turned so as to prevent the mercury flowing from it, then the three-way-stop-cock connected with it should be turned so that the air may have access to the surface of the mercury, and the other three-way-stop-cock turned so that the air from the funnel with which it is connected will flow directly through the glass-tube to the wolff's-bottle.

Then if the second filtering funnel be placed in a lower position than the first, above described, and clamped securely, the machine is ready for use.

If the mercury should then be turned on it will force the air out of the lower filtering funnel and this air will then act upon the injection mass, forcing it along in proportion to the difference in level of mercury, or the amount of mercury turned on.

If a larger quantity of injection mass is required than can be discharged from the wolffs-bottle by once emptying one funnel into the other, after the flow of mercury has been stopped by turning the stop-cock of the lower funnel, an additional quantity of the fluid can be poured through the central mouth of the wolffs-bottle.

The positions of the three-way-stop-cocks should now be reversed to permit the current of air to act from the other funnel upon the injection mass.

Then the funnel which is full of mercury should be raised to a position on its rod as much above that of the stationary funnel as it was below it before, when it will in its turn become the source of power when the mercury is turned on.

Thus the operation may be proceeded with, and by repeating this simple process an injection may be performed lasting any reasonable time, with very little trouble or interference on the part of the operator and, so far as my experience goes, with great satisfaction.

It will be readily seen that absolute constancy of pressure is not even obtained in this instance because the shifting lines of level of the surface of the mercury, in the firmly fixed funnels, introduce a small error: this might be overcome by suspending the funnels from springs so adjusted that as the mercury changed its level, a compensation would be brought about by a change in the position of the funnels; but after having tried the firmly fixed funnels and not having experienced the slightest difficulty from this source of error, I am inclined to believe that a change would be an almost needless refinement of accuracy and in addition would somewhat doubt the prudence of having such a weight of mercury suspended in that manner.

The piece of apparatus has been thus described, somewhat at length in the hope that histologists will find in it an aid in their investigations such as it has been to myself.

A FILTERING WASH-BOTTLE ESPECIALLY ADAPTED TO THE USE OF THE HISTOLOGIST. By C. E. HANAMAN, of Troy, N. Y.

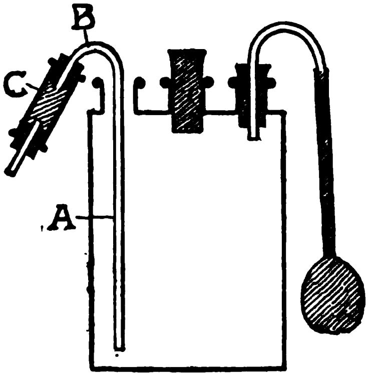
[ABSTRACT.]

It is a matter of every-day experience, with those who employ the microscope in histological research, that however careful they may be to filter their reagents before the day's work begins, ere long, particles of foreign matter are apt to make their appearance in the various solutions employed.

These particles not unfrequently become attached to the most interesting portions of valuable specimens, thereby marring their beauty, and sometimes detracting from their value. I have not found the usual forms of paper and cotton filters as convenient as I could wish, and it was with a view to the production of a simple apparatus, which would deliver as required any quantity, from a single drop to a full dish of any staining fluid or other reagent, that I devised, some two years ago, the apparatus, a description of which is the purpose of this paper.

A bottle with three necks is taken and through a cork in one of the necks is passed a bent glass tube (A) reaching nearly to the bottom (as in the ordinary wash-bottle) but cut off at the upper end a short distance beyond the bend or neck (B). At this point there is attached by a cork, a short piece (say three inches long) of a tube of wider lumen (C) than that used in the long tube (A). Into the open or lower end of the wide tube, is inserted a cork, through which passes a short delivery tube.

Over the corks inserted in either end of the wide tube, and covering the ends of the smaller tubes where they open into the larger one, are stretched pieces of fine muslin, and between the two corks the tube is filled with a rather loosely packed plug of "absorbent cotton." The second or middle neck of the bottle is closed by a well fitting cork. Through a cork in the third neck is passed a piece of bent glass tubing, which reaches just below the cork on the inside of the bottle, and extends (either in the form of an arch, or bent at right angles) a short distance from the top



A Filtering Wash-Bottle.

of the cork through which it passes. Attached to this tube is a piece of rubber tubing of a suitable length, at the end of which, either a short glass mouth-piece, or the rubber bulb of a Davidson syringe is attached. To use the apparatus the bottle is filled, through the middle neck, half or two-thirds full of the reagent to be employed and the cork is again firmly inserted. It will then be found, that by compressing the rubber bulb, or blowing through the mouth-piece, the air in the bottle will be condensed and the fluid will be driven through the cotton plug, either in drops or in a stream, according to the quantity of air forced into the bottle. A very gentle squeeze of the bulb, or a gentle breath at the mouth-piece, and a single drop is delivered, while by rapidly condensing the air with the bulb, a stream will flow from the delivery tube, which can be instantly stopped when the desired quantity has been obtained, by simply loosening the cork in the middle neck. When the plug of cotton becomes foul, as it will after a time, the cork in the lower end of the wide tube is taken out and the plug pulled out with a bit of bent wire, and a fresh one substituted.

The same plug may be used for a long time with most reagents before renewal is required. With reagents like ½ per cent or ¾ per cent salt solution, in which the strength of the solution would be altered by condensation in the cotton plug, a daily renewal may be necessary, but with even so troublesome a reagent, as the Hæmatoxylin staining fluid, the plug will not require renewal oftener than once or twice a week, when used daily.

The convenience of the apparatus will I think be apparent to all, and I hope may prove to be as great a source of comfort to others as it has been to myself.

DEVELOPMENT OF THE PLANULA OF CLAVA LEPTOSTYLA, AG. By J. H. PILLSBURY of Springfield, Mass.

[ABSTRACT.[1]]

AFTER referring to the habitat of this hydroid the author gave a brief description of the familiar method of its development from the formation of the germinal dot to the escape of the planula from the sporosac. The special point to which the author wished to call attention is the fact that the cilia are developed before the planula bursts forth from the sporosac, a fact which has been either overlooked or denied by previous observers, and the probable service the ciliary motions render in accelerating the escape of the planula.

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[1] Published in full in the American Monthly Microscopical Journal, for October, 1882.

TITLES OF OTHER PAPERS READ IN SECTION G.

ON A NEW FORM OF DRY MOUNTING. By Wm. A. Rogers of Cambridge, Mass.

A STUDY OF THE PROBLEM OF FINE RULING WITH REFERENCE TO THE LIMIT OF NAKED EYE VISIBILITY AND MICROSCOPIC RESOLUTION. By W. A. Rogers of Cambridge, Mass.

ON THE EPIDERMIS OF MARSIPOBRANCHS. By Albert H. Tuttle of Columbus, Ohio.

THE HOUSE-FLY CONSIDERED IN RELATION TO CONTAGIOUS POISONS. By Thomas Taylor of Washington, D. C.

A NEW FREEZING MACHINE FOR SECTION CUTTING. By Thomas Taylor of Washington, D. C.

DEMONSTRATION OF THE BACILLUS OF TUBERCULOSIS. By William Osler of Montreal, Canada.

SECTION H.

ANTHROPOLOGY.

ADDRESS

BY

DANIEL WILSON, LL.D., F. R. S. E.,

VICE PRESIDENT, SECTION H.

SOME PHYSICAL CHARACTERISTICS OF NATIVE TRIBES OF CANADA.

IN welcoming the Anthropological Section of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE to its meeting here on Canadian soil, it will not, I trust, appear unsuitable to the occasion, if I invite attention to some of the physical characteristics which distinguish certain native races of the Dominion; and especially to the significance of certain typical head-forms, and their bearing on our special researches in reference to the origin, distribution, and classification of races.

In so doing, it is important to keep in view the prevalence throughout the American continent of various artificial modifications of skull-forms. This strange custom is probably at the present time carried on more systematically among the different tribes of Flathead Indians of British Columbia, than in any other region; though abundant evidence exists to show its prevalence both in past and present times among many tribes and nations in very different stages of progress, alike in North and South America. It has, indeed, attracted more general attention than most other characteristic practices of the American aborigines, owing to its prevalence alike among the most barbarous and the most civilized races. To all appearance the Peruvians and Mexicans had developed independent phases of progress in arts, science, and social policy, without any knowledge of each other. Nevertheless, we trace the singular practice of moulding the human head into abnormal forms, alike among the civilized races of Peru, the ancient lettered architects of Central America and Mexico, and among barbarous tribes both to the east and west of the Rocky Mountains. The earthworks of the Mississippi Valley Mound-builders have been found to cover artificially flattened crania; and the student of American native civilization, as he turns from pondering over the bas-reliefs and hieroglyphics on the sculptured slabs of Palenque and Uxmal, is startled to find that the cranial forms and strange physiognomical contour of the architectural race of Central America are reproduced among some of the most barbarous living tribes of Oregon and British Columbia. But, now that the study of craniology has been carried out by many intelligent observers, the fact is becoming familiar to us that artificial cranial deformation is no peculiarity of the American continent, either in ancient or modern times. The compressed crania of the Asiatic Macrocephali attracted the attention of Hippocrates five centuries before the Christian era; and Blumenbach, the foremost of European craniologists, figured in the first fasciculus of his "Decades Craniorum," in 1790, an imperfect compressed skull, received by him from Russia, with the information that it was probably that of a Tartar. This he unhesitatingly designated an Asiatic Macrocephalus. The conclusion thus arrived at has been sustained by subsequent discoveries; and as attention is more widely directed to the general subject the results are found to have a special value for the American ethnologist.

It seems probable that the name of Macrocephali, like that of our own Flatheads, did not properly belong to any single tribe, or even distinct race of ancient Asia; but had its origin in the effort, by artificial means, to produce the patrician head-form, primarily characteristic of some dominant, or conquering race. Among the Chinooks and other Flathead tribes of this continent, and also, as I believe, among the ancient builder-races of Yucatan and Peru, certain head-forms were recognized as an attribute of the ruling cast. Within the Flathead area of British Columbia the compressed and distorted skull is even now the symbol of aristocracy; and adopted captives, or slaves, are precluded from giving the prized deformity to their offspring. Hippocrates refers in his "Dé Aere, Aquis, et Locis," to the Macrocephali as a people among whom "those are thought the most noble who have the longest heads." Skulls of this type have been recovered in recent years from ancient graves in the Crimean Bosphorus, and the valley of the Don. Still more illustrative of the effort at superinducing a novel dolichocephalic form among races of brachycephalic type, are the examples of compressed Hun or Avar skulls found from time to time on the line of march of the great Hunish invasions of Europe in the fifth and sixth centuries. One of the first examples of such mediæval compressed crania which attracted special attention in Europe was a skull found, in the year 1820, at Fuersbrunn, near Grafenegg, in Austria. Count August von Breuner, the proprietor of the soil, acquired possession of the interesting relic, and at once ascribed it to the Avarian Huns, who occupied that region from the middle of the sixth until the eighth century. Of this compressed Avar skull, Retzius gave a description in the proceedings of the Royal Academy of Sciences of Stockholm, in 1844; and showed that the skull, which had been regarded as remarkable for its great elongation, was in reality a true brachycephalic skull, such as the Mongol affinities of the Avars would suggest, but that by artificial compression it had been elongated, vertically, or rather obliquely. An additional interest is conferred on this European example of artificial cranial deformation by the fact that scientific observers were persuaded for a time to regard it, not as European, but as an intrusive American example, brought thither soon after the discovery of this continent. The well known traveller Dr. Tschudi communicated to Müller's "Archiv für Anatomie" a memoir, in which he instituted a comparison between this Grafenegg skull and the compressed crania of ancient Peruvian cemeteries, whence he arrived at the conclusion that scientific men had been deceived in ascribing to any Asiatic or European source a skull which must have been originally derived from Peru. In confirmation of this, he recalled the fact that, widely as Austria and Peru are now severed, in the sixteenth century the Emperor Charles V embraced both within his dominions. He accordingly conceived it no improbable conjecture that the compressed skull was brought to Europe, as an object of curiosity; and being afterwards thrown aside, it was mistakenly assumed to pertain to native sepulture when recovered at Fuersbrunn in the present century.

More recent discoveries of artificially compressed crania on European sites, have removed all doubts of their native, or intrusive Asiatic origin. It thus appears that the barbarous practice is neither recent, nor peculiar to the New World. Neither to America nor to Europe do those examples of mediæval and ancient compressed crania really belong, but seemingly to the nomad Mongols and Ugrians of the steppes of Northern Asia, in the vast wilds of which we lose them as they spread away eastward toward the Okhotsk Sea and the Aleutian Islands. We are thus guided by unmistakable indications backward, as it seems, on this ancient trail, down the valley of the Danube, and beyond the Caspian and the Ural Mountains, to a region outside the farthest limits assigned by Hippocrates, Strabo, Pliny, or Mela, to the Asiatic Macrocephali; and recover traces of the strange practice of the American Flatheads far to the northeast of the Altai chain, in the valleys that skirt the Yablonoi mountains, as they trend eastward towards the Okhotsk Sea. It may indeed be an American practice which Asia borrowed, for the affinities of race between the tribes of the islands and Asiatic mainland immediately to the west of Behring strait point to a migration to Asia from America. Such, however, is limited and exceptional. On evidence which embraces the ethnical characteristics of a very wide Asiatic area, the Mongolian classification of the American Indian is confirmed by many significant points of resemblance in form, color, texture of hair, and peculiar customs and traits of character, which fail us when we turn either to the Asiatic Aleutians, the Namollos, and other allied tribes of the older continent, or to the true Eskimo. The striking resemblance noted by Humboldt as existing between the American race and the Mongols of Asia, received independent confirmation from Dr. Charles Pickering, as the result of his extensive observation of the races of both continents, in his capacity of ethnologist to the American Exploring Expedition. Such affinities are still further confirmed, as we recover the traces of the singular practice of cranial deformation extending in ancient and mediæval times eastward from the Euxine and beyond the Altai mountains. To those little-known areas of northern Asia the ethnologist and the archæologist have yet to turn in quest of the footprints of one of the immigrant routes to the new world. There it is, in the vast unknown regions of Asiatic Russia, that we may hope to recover evidence confirmatory of at least one source of the Asiatic relations of the American race.

It is now a recognized fact that the artificial head-forms characteristic of diverse tribes of North and South America vary greatly, from the extreme depressed forehead and laterally compressed skulls of races that rivalled the ancient Macrocephali in their estimation that "the most noble are those who have the longest heads," to some among the Cowlitz or Chinook tribes of British Columbia, whose heads are compressed into a flattened disk. The two artificial extremes find their analogues in the distinct ethnical divisions of dolichocephalic and brachycephalic head-forms among well-known northern tribes. The predominant natural form, characteristic of the more southern tribes of North America, appears to have been brachycephalic, or, as it is sometimes called, globular. But along the regions of the great lakes, in the valley of the St. Lawrence, and northward throughout the whole Eskimo area, the dolichocephalic head-form prevails. The native races of the Dominion, and especially the earliest known aborigines of Upper and Lower Canada, including the province in which you are now met, appear to have been all of the same dolichocephalic type ; and so to have formed a class markedly distinct from the short, or globular headed races of the south, whose head-form was long regarded as typical of the whole American race. Of the Indians of Hochelaga, first met by Cartier, in 1535, we are able to judge from crania recovered from their cemeteries. The palisaded Indian town of Hochelaga occupied, in the sixteenth century, the site where we are now assembled ; and in the museum of McGill College may be seen examples of the crania, as well as specimens of the flint implements and pottery dug up on its site. Its traces revealed nothing suggestive of any other rudiments of civilization than have long been familiar to the American student of primitive arts in the abundant remains of Indian settlements throughout the area of the eastern States, and on the sites of the Iroquois Confederacy in the State of New York. Their earthenware pots and bowls of various sizes were decorated with rude yet tasteful incised patterns ; and the handles were further ingeniously modelled at times into human and animal forms. Tobacco pipes also, both of stone and earthenware, here as elsewhere, were special objects of artistic ornamentation. Stone and flint implements, bone needles and bodkins, also abounded ; but of metal only very rare traces of the cold-wrought copper tool gave any indication of even the first rudiments of metallurgic art. In truth, Canada has no such evidences, even of an incipient native civilization, as the remarkable earthworks which abound in the great river valleys to the south of Lake Erie. To all appearance, through unnumbered centuries, the tide of human life has ebbed and flowed, to the north of these great lakes, and in the valley of the St. Lawrence, as unprogressively as on the great steppes of Asia, among the Bedouin tribes of the Arabian peninsula, or around the tropical lakes of equatorial Africa. Such footprints as the wanderers have left on the sands of time tell us no more than the ripples on the sea beach, and are indeed still more evanescent. Nevertheless, in all their distinctive characteristics, the tribes of our Canadian forests and prairies present much in common with those by whom the whole area of this northern continent, southward to the Gulf of Mexico, appears to have been occupied when first brought under the notice of European explorers.

It is indeed a noticeable fact in reference to the entire population of this western hemisphere, throughout areas so widely differing in climate and physical geography as are embraced within the region extending from the arctic circle to Terra del Fuego, that the ethnical diversities are slight when compared with those which pertain to what, historically speaking, are the older continents. It seems to force on us the conclusion that, however remotely we may trace our way back into unrecorded centuries, ere we reach the time when man made his first appearance here, so far as the multiplication of diverse racial varieties afford any evidence, it is recent when compared with the peopling of the ancient world. To this indeed one important exception has been suggested in the assumption of a direct affinity between the hyperborean tribes of this continent and the men of Europe's palæolithic era; and I shall accordingly refer to it in its bearings on the general conclusions to which we are thus led.

Great, however, as is the superficial resemblance which seems to pervade the diverse tribes of the American continent, some of the underlying differences were noted from the first. Columbus, with an eye quick to discern all that was peculiar in the novel scenes on which he was the first to gaze, failed not to note the marked distinction between the fair complexion of the Guanches, who were brought under his notice on his first voyage, and the reddish-olive of the ferocious Caribs. Apart from this purely physical distinction, these Guanches attracted his attention by their gentle manners and inoffensive habits. From them he learned of the Caribs, as a fierce, warlike people occupying the neighbouring islands and the mainland, of whom they lived in constant dread; and who subsequently became familiar to the Spaniards as a ferocious, crafty, and revengeful race, delighting in cannibalism. Hence we perceive that from the first both physical and moral differences, of a sufficiently marked character, were observed among native tribes of the New World. Nor indeed did Humboldt, or even Morton, entirely overlook the existence of considerable varieties in color and complexion, from nearly white to a dark brown; though they were led, from different causes, to underestimate the extent of diversity prevailing among the widely scattered nations of North and South America.

But while it is deserving of notice that the aborigines of Canada do differ in certain physical characteristics from those especially of the more southern states of North America, it is undoubtedly true that an approximate correspondence in ethnical characteristics is common to many tribes both of North and South America. It is not, therefore, to be wondered at that the idea of their constituting one native stock distinct from all the races of the Old World, and agreeing in the possession of physical characteristics peculiar to themselves, should have been accepted for a time as indisputable. The vague generalizations of travellers, and the current forms of popular belief, however, gradually acquired consistency as an accepted canon of ethnical science; until, in the final embodiment of Dr. Morton's matured opinions, he affirmed the American race to be essentially separate and peculiar, and with no obvious links, such as he could discern, between them and the people of the old world, but a race distinct from all others.

The geographical facilities for intermixture among the very diverse races of Asia, Africa, and Europe, account for many intermediate and transitional races; but this increases rather than diminishes the difficulty of referring to any satisfactory source, such primary types of extreme diversity as the Negro, Berber, Mongol, Malay, Arab, and Saxon. Here, on the contrary, so far as now appears, approximate types were hemmed in between the Atlantic and the Pacific; and in so far as they intermingled, the tendency necessarily was to diminish, if not to efface, any strongly marked distinctions; just as, in prehistoric centuries, the blending of the aboriginal savage with intruding races is assumed to have begotten the Melanocroi and the Xanthocroi of Europe's ethnological classification.

Here, undoubtedly, as well as in Europe and Asia, extreme diversities have been modified; but from the first these differences must have extended over a narrower range on the American continent than that which finds such curious illustration in the ancient sculpture and paintings of the Nile valley. The baso-relievos of Yucatan, the terra cottas of Mexico, and the pottery of Peru, furnish analogous evidence of considerable diversity of type among the prehistoric, as well as the historic and civilized races of the New World. Nevertheless, after the fullest recognition of all that such evidence indicates, the fact remains that great as is the divergence of the Eskimo from the Mexican, or the Peruvian from the Patagonian, the difference becomes almost insignificant in comparison with that which distinguishes the Aryan Hindo from the Andaman Islander, the Arab from the Chinese, or the insular Malay from the Negritto. Yet all of those pertain to a continent which is only separated from our own by Behring Strait. So noticeable indeed is the prevailing correspondence in ethnical characteristics among the various races of this continent, that the elements of diversity were long overlooked, even by acute scientific observers. Malte Brun affirmed as the result of a long course of observation, "that the Americans, whatever their origin may be, constitute at the present day a race essentially different from the rest of mankind." A more notable authority, possessed alike of rare capacity for accurate discrimination, and of opportunity for extended personal observation,—the distinguished scientific traveller, Humboldt,—remarked in the preface to his "Researches:" "The nations of America, except those which border the polar circle, form a single race, characterized by the formation of the skull, the color of the skin, the extreme thinness of the beard, and the straight glossy hair."

Until very recent years this was accepted as no less indisputable than any axiom of Euclid. American ethnologists were agreed as to the predominance of one ethnical type throughout the whole western hemisphere; while those of Europe, with rarer opportunities for personal observation, were predisposed by all the narratives of early voyagers to accept the conclusion that the man of the New World was a well-defined variety, if not a distinct species, of the genus *Homo*. Prichard, Lawrence, Wiseman, Knox, Morton, Agassiz, Squier, Gliddon, Nott, and Meigs, might each be quoted in confirmation of this opinion, and especially of the prevailing uniformity of certain strongly-marked cranial characteristics. Agassiz, for example, affirmed in very explicit language: "with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South America, but dividing into innumerable tribes; whilst, in the Old World, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries." Morton, again, viewing the subject in the light of his own special evidence, designated a markedly brachycephalic skull, with flattened occiput, recovered from one of the mounds in the Scioto Valley, "an aboriginal American head," and added: "this is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection." Accordingly, after indicating these in detail, he affirms: "it is the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate."

Among what may be designated typical Canadian skulls, those of the Hurons of the region lying around the Georgian Bay have a special value. They represent, as we believe, a native race which, under various names, extended from the Lower St. Lawrence westward to Lake St. Clair, the *Ouane-doté:* including the Petuns, Neuters, Hurons, Eries, and other Wyandot tribes, of the same stock as the Iroquois; but to whose implacable enmity their extermination was ultimately due. The native population first met with by Cartier and the French explorers of 1535, is believed to have been of the same Wyandot stock; but before the return of the French under Champlain, in 1603, they had been exterminated, or driven westward to the later country of the Hurons, on the Georgian Bay. There they were first visited by Champlain in 1615, and subsequently by the French Jesuit missionaries who, in 1639, found them occupying thirty-two palisaded villages. Brébœuf reckoned their number in 1635 at thirty thousand, and they are estimated, in the "Relation" of 1660 at thirty-five thousand. Already, at that early date, the whole country westward from the Ottawa to the Huron country around Lake Simcoe, had been depopulated, and reduced to a desert, by the wrath of the Iroquois. Charlevoix assigns the year 1655 as that of the destruction of the Attiwendaronks, or Neuters, who occupied the fertile Niagara peninsula between Lake Erie and Lake Ontario; and the Eries, whose name is perpetuated in the great lake on whose shore they dwelt, had already been exterminated by the violence of the same kindred race, before the French explorers had even ascertained the existence of the lake which bears their name. In the earlier French maps an imaginary river extends uninterruptedly from Lake Huron to Lake Ontario.

Minute information has been preserved of the Hurons in their later home, derived chiefly from the "Relations" of the Jesuit Fathers, communicated to the Provincial of the Order at Paris, from 1611 to 1672. We thence learn accurate details of their great "Feast of the Dead" celebrated at intervals of ten or twelve years, when the remains of their scattered dead were gathered from old scaffolded biers, or remote graves, and deposited with grand ceremonial and mourning in the general cemetery of the tribe. In the vicinity of the sites of their palisaded villages extensive ossuaries have repeatedly been found; and there are now preserved, in the museum of Laval University at Quebec, upwards of eighty skulls recovered from the Huron cemeteries of St. Ignace, St. Joachin, St. Mary, St. Michael, and others of the Huron villages, so designated by the French Missionaries who visited them in the seventeenth century, and labored for the conversion of the Indians there. Other examples are preserved in the museum of Toronto University; and I may add that Dr. Taché, by whom the most extensive researches were carried on, presented ten Huron skulls to the London Anthropological Society; and I have since forwarded specimens to Dr. De Quatrefages for the museum of the Jardin des Plantes at Paris.

A special value attaches to the skulls recovered from those Huron ossuaries, from the fact that the race was exterminated, or driven out of the country, by their Iroquois foes, in 1649: and hence the crania recovered from their old cemeteries may be relied upon as giving a fair illustration of the physical characteristics of the race. The descendants of a small band of Huron refugees, rescued from the general massacre, and brought by the French missionaries to Quebec, still survive at the Huron village of Lorette, on the St. Charles River; but they have long since lost the pure traits of full-blood Indians, and are chiefly interesting to the ethnologist now from the evidence they give of the survival alike of native intellectual and physical traits, after an interval of well nigh two centuries and a half passed in intimate intercourse, and latterly frequent intermarriage with the French habitans.

The Huron skull is strongly marked as of the dolichocephalic type. The careful measurements of thirty-nine male skulls yield a mean longitudinal diameter of 7.39 to a parietal diameter of 5.50; and of eighteen female skulls, a longitudinal diameter of 7.07 to a parietal diameter of 5.22. One essential characteristic, therefore, that of great relative length, is unmistakable. I specially refer to this now, because we possess, in the collection of the Canadian Institute at Toronto, a skull recovered from one of the Huron ossuaries near Lake Simcoe which differs essentially from this Huron type. It is a short skull,—shorter even than that from the Scioto mound,—of the same, so-called, globular type, measuring only 6.00 in longitudinal, and 6.40 in parietal, diameter. Reverting, therefore, to Dr. Morton's ascription to the Scioto mound skull of national characteristics, which constitute it "the perfect type of Indian conformation, to which the skulls of all the tribes from Cape Horn to Canada more or less approximate," this northern example, if it stood alone, would seem to confirm his assumption. But it is a wholly exceptional case; so distinct from the true Huron type that, after a careful study of one hundred and twenty-six crania from ossuaries of the Huron country, including considerable deviations from what may be regarded as the normal type, I have not found one other example approximating to it. It differs little less essentially from the race-form of the people whose grave its owner shared than that of a Chinese from the normal skull of the pure Anglo-American; and may be assumed as that of an Indian belonging to some far southern tribe, whom the chances of Indian warfare had made a captive, or an adopted member, of the Huron tribe in whose cemetery he found his final resting-place.

Such indications of physical diversity, among the nations so widely scattered throughout the New World, accord with philological and other evidence. Not by one, but by diverse routes have the fathers of the American nations found their way thither: some by Behring Strait and the Aleutian Islands; others by more southern routes across the broad Pacific, aided by winds and currents, and passing onward from island to island of the great archipelago; others, as we know, by Iceland and Greenland, across the northern Atlantic; and others again—as philological evidence seems to indicate,—along the same route as that which Columbus successfully pursued in 1492. But to the primary migrations we know not how remote a date to assign, in order to allow of the interblending of intruding races, and the development of the native American "Red Man" with all his distinctive traits of individuality. For, while it is important to note the elements of diversity, it is nevertheless true that the New World does differ from the Old in the narrow range of such variations of race-type through all extremes of climate from arctic to temperate, tropical, and antarctic. The European traveller who surveys his own continent from the northern habitat of the Fins and Lapps, and the corresponding Asiatic hyperboreans, and then traverses the eastern hemisphere to the Cape or to the Indian Ocean, comes in contact with all intermediate varieties between the two extremes of the white and black races; and recognizes in western Europe the Melanocroi who seem to be the resultant of their inter-blending in prehistoric times. But in America we seem to see no more than a result analogous to the latter; and this as the product of more nearly allied primitive stocks, the largely preponderating element of which has been derived from the Mongol area of eastern Asia. Philological evidence, on the other hand, no less clearly indicates the remoteness of the migrations by which this first colonization of the New World was effected; it may be, indeed, that they pertain to periods when the physical geography of both continents, and of the intermediate archipelago, afforded facilities for migration altogether wanting within historic times.

But such ideas of a derivative origin of the American aborigines are of very modern growth, and are only now displacing long accredited beliefs. That the man of this New World must prove a being essentially different from any known race of Europe, Africa, or Asia, was an opinion which assumed ever stronger confirmation, as the idea of Columbus that he had landed on the eastern Continent faded away from the minds of his successors. The Indians of his new-found world were no natives of Cipango, or the valley of the Indus; and the literature of the fifteenth and sixteenth centuries abounds with evidence that it was much easier to persuade the men of that age that Calibans and monstrous Anthropophagi peopled the strange regions beyond the Atlantic, than that these were inhabited by human beings like themselves.

Even Columbus, it has to be remembered, in searching for evidence to confirm his own scientific demonstration that the world was a sphere, and so, that the eastern continent could be reached by a western route, attached special value to indications of the existence of a transatlantic continent, derived from the fact that the bodies of two dead men had been cast ashore on the island of Flores, differing essentially in features and physical characteristics from any known race. When, at length, the great discoverer set foot on the islands first visited by him, the peculiarities which marked the gentle and friendly race of Guanahanè were noted with curious minuteness; and their "tawny or copper hue," their straight, coarse, black hair, strange features, and well-developed forms, were all recorded as objects of interest. On his return, the little caravel of Columbus was freighted not only with gold and other coveted products of the New World, but with nine of its natives, brought from the Islands of San Salvador and Hispaniola, eight of whom survived to gaze on the strange civilization of Spain, and to be themselves objects of scarcely less astonishment than if they had come from another planet. Such was the earliest knowledge acquired by the Old World of the type of humanity generically designated as the Red Indian; and the attention which its peculiarities excited when thus displayed in their fresh novelty has not yet exhausted itself, after an interval now little short of four centuries.

Of all known races of the New World, the Eskimo alone presented, at first, a seemingly marked diversity from the other aborigines; though the grounds on which such a conclusion was based are traceable far more to Arctic conditions of life, than to any ethnical peculiarities definitely assigned to them.

This is apparent from the terms employed by the historian Robertson, who, writing in 1777, says: "The Esquimaux are manifestly a race of men distinct from all the nations of the American continent, in language, in disposition, and in habits of life. But among all the other inhabitants of America there is such a striking similitude in the form of their bodies, and the qualities of their minds, that, notwithstanding the diversities occasioned by the influence of climate, or unequal progress of improvement, we must pronounce them to be descended from one source."

The idea thus definitely set forth by the Scottish historian of the last century was placed on what seemed to be a strictly scientific basis by the author of the "Crania Americana." Dr. Morton's diligence in the accumulation of evidence merits our highest gratitude. But he had unfortunately set out with the idea of one nearly uniform race peculiar to the New World; and with all the evidence before him which has since sufficed to convince others of the prevalence of great diversity of head-forms among American races, he persisted to the close in maintaining the physical unity of the Red Race from the arctic circle to Cape Horn.

Of the three propositions with which Dr. Morton sums up the results borne out, as he conceives, by all the evidence advanced in his "Crania Americana," one is, "That the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character." Any difficulty arising from physical or other differences, he sought to overcome by the application of the hypothesis that "these races originated in *nations*, and not in a single pair; thus forming proximate but not identical species." Subsequent to his death his collection of crania was acquired by the Academy of Natural Sciences of Philadelphia, and thereafter supplemented in its most important branches by many valuable additions. This greatly augmented collection was classified anew and catalogued by the late Dr. J. Aitken Meigs; and after having thus had all the evidence which it presents brought in systematic order under his own notice, he contributed to Gliddon's "Indigenous Races of Men," a paper entitled "Cranial characteristics of the Races of Men," in which he thus reiterated the Mortonian dogma: "Through *Crania Americana*, it has long been known to the scientific world that a remarkable sameness of osteological character pervades all the American tribes from Hudson's Bay to Terra del Fuego. It is equally well known that the researches of Humboldt and Gallatin have demonstrated a conformity not less remarkable in the language and artistic tendencies of these numerous and widely-scattered aborigines."

The artificial transformations of the Indian head were at first a source of difficulty in any systematic classification of head-forms; and the views of Dr. Morton underwent considerable modification on some points relating to the influence of this custom in perpetuating certain types of head; but he finally reverted to the original idea of one predominant cranial type to which all the races of the American continent more or less nearly approximated.

The evidence which the mediæval European examples of cranial deformation supply suggests the origin of this barbarous practice in an aim at conformity with the natural head-form of a patrician or conquering race. Dr. Fitzinger, who has carefully investigated the whole subject of the discovery of macrocephalic skulls in ancient Austrian sepulchral deposits, after tracing the evidence supplied by the allusions of classic writers, mentions an interesting independent illustration of the subject. A medal, struck apparently to commemorate the destruction of the town of Aquileia, by Attila the Hun, in the year 452, came under his notice. On one side is represented the ruined city, and on the other the bust of the Hunish leader in profile, with the same form of head as that shown in the supposed Avar skulls found in the valley of the Danube. One of this type obtained by M. Hippolyte Gosse, from an ancient cemetery in Savoy, presents the favorite Hun or Avar form when viewed in profile, with the singular vertical elongation which appears to have constituted an ideal type of masculine beauty among the Asiatic followers of Attila, as among the Natchez, the Peruvians, and other nations of the New World. It was found at Villy, near Reigner, and has been engraved by Retzius, from a drawing furnished to him by the discoverer.

Thierry, in his "Attila," refers to the artificial means resorted to by his followers in order to give a Mongolian physiognomy to their children. The Hunish leader welcomed every able bodied recruit to his standard, and was in reality as much a leader of Goths as of Huns; though the black Huns from the dreary Siberian steppes constituted the aristocracy of his wild followers, whose Mongolian physiognomy formed the ideal of ethnic beauty. At this the Gothic mother accordingly aimed, by bandaging the nose, compressing the cheek bones, and giving an artificial form to the cranium of her infant. Such practices, however, when once brought into general use, continue long after the reason for their adoption has ceased. It need not therefore greatly surprise us to learn that the practice of distorting the skull in infancy still prevails in some districts of France. Among the examples of such cranial malformation engraved by Dr. Foville, in his work on the "Anatomy of the Nervous System," there is one which might take its place alongside of some of the most exaggerated specimens brought from Peruvian cemeteries.

But however the Gothic mother might labor to make the natural development of her infant's head conform to the Mongolian model, the traces of the originally dolichocephalic type could not be wholly eradicated. This is seen on comparing examples in any large collection of American Indian skulls. In the compressed and distorted Peruvian crania, traces of two distinct types appear to me still uneradicated. The same is noticeable in the sculptures of Central America, as in the Palenque bas-reliefs where deities and chiefs treading kneeling figures underfoot, present the long, sloping forehead in a line with the straight nose, with other features of the strange profile peculiar to the old race; while the subject race is hook-nosed, with high foreheads, and heads seemingly uncompressed. If the idea is well-founded, which thus traces the origin of this barbarous practice to the efforts of an inferior, or subject race to approximate in outward appearance to the privileged class, its very occurrence points to the existence at some previous time of races essentially diverse in physical character. And if we assume their relative positions to have been akin to that of the conquering Hun and the enslaved Frank or German, the motive to such a practice is sufficiently obvious. Were it possible for the colored population of Canada and the United States, at the present day, by any analogous process to assimilate their offspring to the Anglo-Saxon type, how irresistible would the motive be to its use.

Such ideas, however, found no favor with the author of the "Crania Americana;" and in some of the conclusions finally adopted by him he has been sustained by authorities of just weight in science. In the latest record of his matured views, as set forth in a posthumous paper contributed to Schoolcraft's "History of the Indian Tribes," he remarks: "I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form; and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that all these variously formed heads were originally of the same rounded shape."

In that same final contribution to his favorite science, Dr. Morton's matured views on the whole subject of the cranial type of the American continent—based on the additional evidence accumulated by him, in the interval of twelve years which elapsed between the publication of the "Crania Americana" and his death,—are thus defined: "The Indian skull is of a decidedly rounded form. The occipital portion is flattened in the upward direction, and the transverse diameter, as measured between the parietal bones, is remarkably wide, and *often exceeds the longitudinal line.*"

It is curious to observe in this latter statement the evidence of a careful and most conscientious observer allowing all the proofs of varying physical type which his own indefatigable industry had accumulated, to be subordinated to this foregone conclusion. Here Dr. Morton must have had in view his theoretical type, rather than the results of his own careful observations, for even if he accepted as evidence the artificially abbreviated and flattened skulls, his "Crania Americana" furnishes only one exceptional example, from a mound on the Alabama river (Pl. LIV), of which he says: "It is flattened on the occiput and os frontis in such a manner as to give the whole head a sugar-loaf or conical form, whence also its great lateral diameter and its narrowness from back to front." The idea had, in fact, received nearly universal acceptance that the European immigrants of the fifteenth and subsequent centuries intruded upon races of wholly distinct origin from themselves, and were displacing the true American autocthones, with whom they had nothing in common.

When Prior Fernando de Talavera of Salamanca summoned a meeting in the Convent of San Estebán, in the year 1487, to take into consideration the proposition of Columbus that the earth was not a plane, but a sphere; and that, by sailing in a western course, land, which he assumed must be the most eastern coast of Asia, would be reached: the assembled philosophers and theologians gravely pronounced the idea of the earth's spherical form heterodox, and a belief in antipodes incompatible with the historical traditions of our faith; since to assert that there were inhabited lands on the opposite side of the globe, would be to maintain that there were nations not descended from Adam, it being impossible for them to have passed the intervening ocean!

We smile at the orthodox philosophers and theologians of the fifteenth century, who, with the help of St. Jerome and St. Augustine, proved this western hemisphere of ours to be an impossibility; yet it is curious to detect the same old prejudices unconsciously influencing the minds of some of the acutest men of science in very recent years. What else was it, if not this "impossibility for them to have passed the intervening ocean," or in other words, to have sprung from the same stock, which led one of our own most valued associates, the late Professor Agassiz—a scientific observer of rare sagacity and experience, and one who regarded the entire question of American ethnology from a point of view peculiarly his own,—to adopt the conclusions of Dr. Morton, in spite of the palpable inconsistency of the evidence which he was so well qualified to estimate at its true worth? In his "Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man," while appealing to the results arrived at by Dr. Morton, in reference to the imagined unity of the whole American aborigines as "a single race," he reaffirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel grounds. After defining the evidence on which his general conclusion is based, that the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man, he proceeds to show that America, including both its northern and southern continent, differs essentially from Europe and Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions, than comparison enables us to trace in the Old World. He then adds: "With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and indeed the most extensive investigation of their peculiarities has led Dr. Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent. But, at the same time, it should be remembered that, in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another."

It is interesting thus to recall the matured opinions of this leader of scientific thought among ourselves in very recent years, and review them now in the light of the great revolution since wrought in the entire compass of ethnical and biological science. The author of the "Indigenous Races of Men" scornfully assailed "the Monogenists' idea" of a unity of race, and summed up the practical results which to him appeared to be settled beyond farther cavil, with this fancied demonstration: "It has been shown, 1st, that in America, humatile men and humatile monkeys occupy the same palæontological zones. 2nd, That whilst all such remains of man are exclusively of the American Indian type, the monkeys called *Hapale*, *Cebus*, *Callithrix*, etc., are equally 'terræ geniti' of this continent. . . Finally, that *permanence of type*, as well for humanity as for simiadæ, is firmly established in both genera, from the hour in which we are living, back to a vastly remote, if not incalculable, era of unrecorded time." To the evolutionist of our own day, the very result of such reasoning is to lead to ideas of unity of origin vastly more comprehensive than that which, within a period so very recent, was thus rejected as wholly incompatible with deductions from much industriously accumulated evidence.

It is curious, indeed, to endeavor now to realize to ourselves what distinct idea was present in the mind of Agassiz when he apportioned his essentially diverse types of man to their specific "natural provinces;" or what Dr. Morton conceived in his own mind when, after affirming one of the three propositions which he believed himself to have established, to be "that the American nations, excepting the polar tribes, are of one race and one species, but of two great families, which resemble each other in physical, but differ in intellectual character:" he fancied that any difficulty, arising from such physical diversities as it was impossible for even him entirely to ignore, could be removed by advancing the hypothesis, that "these races originated in *nations*, and not in a single pair; thus forming proximate but not identical species." The only thing which is at all clear is the assumption of what may be called a gregarious creation: the summoning into existence, by some unknown process, or creative fiat, of an entire race, or nation, at the first peopling of this New World with its own specific aborigines, "essentially different from the rest of mankind."

But Malte Brun and Robertson, Humboldt, Morton, Meigs, Gliddon and Agassiz, all concur in excepting the polar tribes, or Eskimo, from the assumed American race peculiar to this continent. Latham says of the Eskimo: "physically he is a Mongol and Asiatic; philologically he is American, at least in respect to the principles upon which his speech is constructed." But whencesoever we may derive them, they too are ancient and widely scattered occupants of the strange inhospitable region appropriated to themselves. One branch of them, the Labrador Eskimo, borders on our own Eastern settlements on the St. Lawrence: beyond these are the East and the West Greenlanders, including the natives of the Danish trading settlements. To the north of them are the Eskimo of the west coast, north of Melville Bay, styled, by Sir John Ross, the "Arctic Highlanders." But their extreme northern limits have yet to be determined. The most advanced arctic explorers have either come in contact with the natives, or found traces of their habitation; and their habits and indifference to the extremest rigor of the climate, justify the assumption that only the absence of game will restrict the limits of their habitat. They occupy the whole coast regions of Behring Strait; and extend beyond that to the islands and neighboring continent, westward even to the shores of northeastern Siberia. The collection formed by Professor Nordenskeold in his Vega expedition — part of which was exhibited at Edinburgh during the present year,— includes an interesting series of implements used by the Chukches of Siberia and the Asiatic Eskimo in fishing and hunting. They employ the same kind of harpoon for hunting the walrus; use a long spear of nearly the same fashion, generally furnished now with an iron head, for hunting the bear; while their arrows are still pointed with walrus ivory. Such traces alike of community of arts and of race, within the arctic circle of the Asiatic and American continents, and even extending to Europe, show that, whatever may have been the ancient lines of migration, the overflow in later centuries across Behring Strait has been from the American continent westward into the Old World.

This widely scattered race, though corresponding in ethnical character, is broken up, by the exigencies of their rigorous climate into small tribes and isolated bands, dispersed for the most part over a coast line extending from Labrador to Behring Strait upward of 5,000 miles, and migrating with the animals on which they depend for subsistence. They are hunters and fishers. The deer, the polar bear, the wild goose, swan, and other birds that resort to arctic breeding grounds, are alike objects of the chase; but they primarily depend on seals and cetaceous animals, the blubber of which furnishes food calculated to beget the animal heat which enables them to brave the severity of an arctic climate. *Eskimántzik* appears to be an Abenaki term signifying "eaters of raw flesh;" and as such indicates the surprise with which even the Indian nomads of New England viewed the strange habits of the hyperborean hunters with whom they were occasionally brought into contact. The Eskimo, however, is neither ignorant of the use of fire, so indispensable to him in his rigorous climate; nor is he an exception to the fitting definition of man as "the Cooking Animal;" though in his peculiar condition of exposure to an arctic winter, raw blubber is at once a necessity and a luxury.

In one respect, as already indicated, the Eskimo occupy a peculiar position on this continent. They are the only race common to the Old and the New World; and, if we accept the conclusion arrived at by the author of "Early Man in Britain," they constituted an Old World race to all appearance before this New World had come into existence. The cave men of Europe's palæolithic era, the contemporaries of the mammoth, and other long-extinct mammals of central Europe, have naturally excited an unwonted interest, as their arts and their remains have been brought to light in recent years. A people of lowest type, as illustrated by the famous Neanderthal skull, that of the Forbes quarry near Gibraltar, and of the Gourdon grotto, with some imperfect traces of others, all classed under the common term of "The Canstadt race," is now assumed to represent the earliest, if not indeed the primæval man of ancient Europe. So far as rudest flint implements afford any evidence of his condition, we might class him with the Bosjesman, the Australian, or the Patagonian of our own day. The evidence, however, in proof of the existence of this Canstadt savage race of palæolithic Europe, rests as yet on insufficient grounds. Curiously, indeed, Professor De Quatrefages has drawn attention to the fact that not only are heads of the Neanderthal type to be met with in modern Europe, in some examples pertaining to men of exceptional intelligence; but the skull of Saint Mansuy, Bishop of Toul, of the fourth century, surpasses the Neanderthal cranium in some of its most simian features; and that of the sagacious and politic hero of Scottish independence, Robert the Bruce, "is a reproduction of the Canstadt type."

But however uncertain our conclusions may as yet be relative to this assumed primæval European type, there is no doubt as to the Cro-magnon race of the reindeer period of southern France. Examples have, indeed, by no means been confined to that area. The Enghis skull was found, with other human remains, embedded in a breccia along with teeth of the fossil mammoth, rhinoceros, horse, and reindeer, in a cavern on the left bank of the Meuse; and the Mentone cave, to the south of the Alps, disclosed an undisturbed sepulchre of the same ancient hunter race. But a special interest attaches to the remains brought to light in 1858, in the rock shelter of Cro-magnon, in the valley of the Véserè. Three men, a woman and a child, had all been buried in the cave. From their remains it is seen that the race was unusually tall, and bore equally little resemblance to the Neanderthal or "Canstadt" type, or to the modern Eskimo. The best preserved skulls—those of an old man and a woman,—are finely proportioned, with large, high foreheads, and great cerebral capacity. M. Broca stated that of the man to be fully 1590 cubic centimetres, or 96.99 cubic inches; and Dr. Pruner-Bey says of two of the male skulls and that of the female, they "have a cranial capacity much superior to the average of the present day." It may remind us of Mr. Alfred Russel Wallace's remark that "natural selection could only have endowed savage man with a brain a little superior to that of an ape, whereas he actually possesses one very little inferior to that of a philosopher."

Whatever differences of opinion affect the determination of the probable age of the Cro-magnon race, they unquestionably pertain to a period so remote that the very earliest historical traces of man in southern France scarcely seem to bring us any nearer to the period which they represent. Their physical characteristics have, therefore, a special significance. The skulls are dolichocephalic, with the frontal bone high and well arched, a graceful fronto-occipital curve, and well-balanced symmetrical proportion throughout. The profile of the old man indicates an expressive contour, the face long, the nose very prominent, and the frontal sinuses but slightly developed for a male. The full face presents a well-proportioned oval, with pointed chin. The one feature detracting from its otherwise attractive expression would seem to have been the unique character of the long and narrow eyes, as indicated by the unusual form of the orbits. At the same time it is to be noted that this well proportioned head bears ample evidence of the exposed life of the wild hunter. The features are rugged, as of one subject, through a long life, to all the hardships of a rigorous climate; and numerous strongly marked impressions of muscular insertions accord with the conditions of savage life.

This is the type of an altogether remarkable prehistoric people, the artistic race of the palæolithic era, to whose skill we owe the contemporary etchings and carvings of the mammoth, the fossil horse, the reindeer, and other mammals of that strangely remote era, when the conditions of life in southern France most nearly resembled those of Rupert's Land or Labrador at the present day. Some of their artistic efforts embrace vegetable as well as animal life; and in their graphic outlines, there is a freedom of handling, an eye for perspective, and even what may truly be called an inventive skill, altogether remarkable in a people ignorant of metallurgy, and living in the condition of rudest hunter life.

Such are the characteristics of the cave men recovered from the rock-shelter of Cro-magnon, in the valley of the Vésère, lying above the long accumulated debris which proved the palæolithic era of its occupants. These ancient hunters of the Garonne and the Pyrenees, the contemporaries of the mammoth and other extinct mammals, and of the grizly bear, musk-sheep, reindeer, and other species now existing only in extreme northern latitudes, had occupied the cave throughout a prolonged period. The charcoal deposits prove the kindling of their fires through long series of years; with recurring intervals of considerable duration. Intermingled with, and overlying, the beds of charcoal, are flint implements, broken bones, and the like debris of a savage hunter's dwelling: in this respect presenting considerable resemblance to the site of an Eskimo settlement. Those accumulations went on until the cave was filled up so nearly to the roof that the hunter could no longer even crawl into it for shelter; and then it was devoted to its final use as a place of sepulture. Had the human remains recovered under such circumstances been characterized by extreme development of the superciliary ridges, a low, narrow, retreating forehead, and other brutish characteristics assigned to the "Canstadt race," there would probably have been no questioning the assumption that in them we have the type of the earlier Palæolithic occupants of the cave: the artistic hunters, to whose skill we owe the spirited life-sketch of the mammoth found in the neighboring La-Madelaine Cave, engraved on a plate of mammoth ivory. But the hypothesis adopted by Professor Boyd Dawkins that the clew to the ethnology of the palæolithic race of the Caves is recovered by the identification of the ancient tool-makers with the Eskimo, could only be maintained by assigning the human remains to a later age than the underlying debris. He accordingly rejects the opinion, so consistent with the general evidence which has satisfied M. M. Lartet, Hamy, De Quatrefages, and other equally qualified judges, that in the old man of Cro-magnon we have the type of the palæolithic race contemporary with the mammoth: the artistic sculptors and draftsmen of that remote European era.

In discussing the fascinating idea which would recover, in the hyperboreans of our own northern frontiers, the men of the same migratory race that, before the close of the pleistocene age, followed the musk-sheep and the reindeer into their northern haunts, Professor Dawkins reviews the manners and habits of the Eskimo, a race of hunters, fishers, and fowlers, accumulating round their dwellings vast refuse heaps similar to those of the cave-men of ancient Europe. The implements and weapons of both do indeed prove that their manner of life was the same; and as he notes the use at times by the Eskimo of fossil mammoth ivory for the handles of their stone scrapers, he adds: "it is very possible that this habit of the Eskimos may have been handed down from the late pleistocene times." But what strikes him as "the most astonishing bond of union between the cave-men and the Eskimos is the art of representing animals;" and after noting those familiar to both, along with the correspondence in their weapons, and habits as hunters, he says: "all these points of connection between the cave-men and the Eskimos can, in my opinion, be explained only on the hypothesis that they belong to the same race."

The hypothesis is a bold one which would thus assign to the rude arctic hunters of this continent a pedigree and lineage compared with which that of the Pharaohs is but of yesterday. To the geologist who fully realizes all that is implied in the slow retreat of the palæolithic race of the valley of the Vésère over submerging continents since ingulfed in the Atlantic, and through changing glacial and sub-glacial ages, to their latest home on the verge of the pole, the time may suffice for any amount of change in the physical characteristics of the race. But if these have vanished how is the lineal descendant of the palæolithic cave-men to be identified? Not by mere imitative art; for that is common to many widely dissimilar races of the American continent. Professor Dawkins says truly of the cave-man, "he possessed a singular talent for representing the animals he hunted; and his sketches reveal to us that he had a capacity for seeing the beauty and grace of natural form not much inferior to that which is the result of long-continued civilization in ourselves, and very much higher than that of his successors in Europe in the Neolithic age. The hunter who was both artist and sculptor, who reproduced with his imperfect means at one time foliage, at another the quiet repose of a reindeer feeding, has left behind the proof of a decided advance in culture, such as might be expected to result from the long continuance of man on the earth in the hunter state of civilization." All this is correct in reference to the art of the Cro-magnon carvers and engravers, and seems in full accordance with the fine heads and great cerebral development of the ancient race; but it would be gross exaggeration if applied to such conventional art as the Eskimo arrow-straightener which Professor Dawkins figures, with its formal row of reindeer and their grotesque accessories. The same criticism is equally applicable to numerous other specimens of Eskimo art, and to similar Innuit, or western Eskimo representations of hunting scenes, such as those figured by our associate Mr. William H. Dall, in his "Alaska," which he describes as "drawings analogous to those discovered in France in the caves of Dordogne."

The imitative faculty and artistic skill of the old Moundbuilder race are very familiar to us; and have furnished valuable evidence of a knowledge by them of a tropical fauna, including animals of the southern continent, suggestive of the probable direction of their own migrations, and their consequent affinity to southern races. Within our own Canadian Dominion the arts of the Queen Charlotte islanders are no less worthy of note. Their curiously conventional style is shown alike in their idols, or manitous, elaborately carved in black argillaceous stone, and in the corresponding decorations of their lodges. In front of each Haida dwelling stands an ornamented column formed in many cases of the trunk of a tree large enough to admit of the doorway being cut through its substance. This column, or obelisk, is carved throughout its whole length in their peculiar conventional style of ornamentation, suggestive at times of affinities to Peruvian sculpture; or, again, of borrowed art of possible Japanese origin. But already the imitative faculty of the Haida artist leads him to revert to European models; his traditional patterns and devices will speedily be among the lost arts of this continent, and the race itself, it is to be feared, is doomed to speedy extinction. All the more urgent is it that no time shall be lost in the accumulation of every available fact, and illustrative specimen of their curious art. Already a valuable contribution to this has been furnished in

Dr. George M. Dawson's "Report on the Queen Charlotte Islands," published as one of the Reports of the Geological Survey of Canada.

The Tawatin Indians on the Fraser River work with no less ingenious skill, and in a like style of combined imitative and conventional art, suggestive at times of curious analogies to some of the finished sculptures of Yucatan. Some of their ivory carvings are executed with a minute delicacy of workmanship such as no Eskimo carver could surpass; but with the same kind of conventional ornamentation as is in use by the Haida artists, strongly suggestive of inherited modes of thought, and traces of intercourse or relationship with the ancient civilized races of Central America.

There is thus no need to assume for the imitative arts of the New World a European source in the remote dawn of pleistocene times. Nor is the identity discernible between certain harpoons and other implements of the ancient hunters of Central Europe and those of the arctic Americans of our own day much more demonstrative of derived arts or community of race. Within the comparatively narrow range of needful weapons or implements, the correspondence notable between some of those of the palæolithic cave-men and of the Eskimo amounts to little more than what is seen in flint arrowheads, stone hammers, and the more common primitive tools of all kinds, executed under nearly similar conditions of life. "The absence of pottery" proves little more than the absence of tropical vegetation; for both were nearly equally impossible under the conditions of climate. The preference for bone and ivory as the materials for their arts is equally due to climatic conditions which render rock and flint generally inaccessible throughout the greater part of the year. The points of agreement are, in truth, little more than are to be anticipated among savage tribes living under similar conditions of climate.

If, however, the skulls of the Cro-magnon cave-men resembled those of the Eskimo, or the underlying debris revealed any traces of crania of the Eskimo type, there would then be good reason for giving consideration to the bearing of any supplementary evidence depending on correspondence in arts, usages and habits. But neither the Cro-magnon cave, nor any other of the caverns of the district, otherwise so rich in archæological and palæontological traces, has yielded the needful evidence. The contrast between the large, well developed Cro-magnon race and the stunted, almost dwarfish Eskimo at once attracts attention. But much greater difference in stature would find ready solution in the privations of an arctic habitat prolonged through unnumbered ages. The notable fact, however, is the absolute contrast in every respect. The Eskimo physiognomy is of a poor Mongolian type. The nose is flat, and the cheek bones are very prominent; the tendency in the skull is towards an acrocephalic form, narrow and long, with the parietal bones frequently meeting at an angle at the sagittal suture. The one possible point of resemblance that could be suggested with any acceptance would be the eye, which in the Eskimo seems often narrow and oblique. This, however, may be apparent only, traceable to the habits of a people one-half of whose year is an unbroken midnight; and who grope in the darkness of their obscurely lighted snowhuts. Certain it is that the long, narrow orbits of the Cro-magnon skulls are not represented in the modern crania.

Sufficiently extensive opportunities of studying the Eskimo cranium have come within my reach to afford me some fair means of forming an idea of the predominant type. In 1862, through the kind services of the late Dr. J. Aitken Meigs, I enjoyed the advantage of carefully examining a series of one hundred and twenty-five skulls, obtained by Dr. Hayes during his Arctic explorations, and making drawings of some of the most marked examples. I have also examined and taken careful measurements of other examples including Western Eskimo, Innuit, and Tschuktchi crania, in the collections at Washington. With the resulting impressions in mind, it is impossible to look on casts of the large and finely developed Cro-magnon skulls now in my possession without being struck with the extreme contrast between them and the Eskimo crania. No wonder that they prove a stumbling block to evolutionists, who look for something of a totally opposite character in the Troglodytes of the palæolithic, or pleistocene age. M. M. Lartet, Hamy, De Quatrefages, the editors of the *Reliquiæ Aquitanicæ*, and other equally competent authorities, have had no difficulty in accepting the evidence that the reindeer hunters of the Vésère lay there intombed in the cave which had so long been a shelter to men of the same race. Had the Neanderthal skull been found under similar circumstances, no doubt, founded on its lower cerebral capacity, would have interfered to prevent its recognition as the type of the artist race to which we owe the life-picture of the mammoth. But Professor Dawkins not only notes that the human remains were deposited in an abandoned palæolithic cave, when it had been nearly filled up with the accumulated debris of successive occupants; but he assigns the remains to the later Neolithic age, notwithstanding the absence of any accompanying relics of the art of the polished stone period.

But I have already exceeded the reasonable limits of an address to this Section of Anthropology, and must leave unnoticed various further points in reference to the aborigines of the Dominion, illustrative alike of the physical characteristics of our native Canadian tribes, and of some special points of significance in relation to their arts. One deduction, however, may be worthy of future consideration. If it be a fact borne out by much independent evidence, that from the extremest northern range of the arctic Eskimo, southward to the Great Lakes, and beyond this, especially to the east of the Alleghany Mountains, amid considerable diversity of ethnical characteristics, the dolichocephalic type of head prevailed; whereas among more southern tribes, such as the Osages, Ottoes, Missouris, Dacotas, Cherokees, Seminoles, Creeks, and many others, including the Florida Indians, the short, rounded, or brachycephalic head appears to have been universal: this seems to point to a convergence of two distinct ethnical lines of migration from opposite centres. In this, as I believe, the evidence thus derived from physical characteristics confirms what is indicated by wholly independent evidence of language, traditional customs, and native arts.

PAPERS READ.

NOTES ON MOUNT ATLAS AND ITS TRADITIONS. By R. G. HALIBURTON, of Ottawa, Canada.

[ABSTRACT.]

IN Dec., 1881, to while away the time at Tangier, where continuous east winds for weeks kept invalids within doors, I turned my attention to the traditions and folklore of the natives of Marocco. The field was represented as a barren one. I was told that the country was inhabited mainly by the Berbers (or Barbars, whence the name Barbary); that, though composed of different tribes, they spoke substantially the same language; and that having adopted the religion of Islam, they had forgotten their old traditions and superstitions. The Rifflans, known still in history as the Riff pirates, inhabit the northern portions of Mount Atlas. To the south of that mountain is a tribe of excellent artificers in brass and copper, called Shelluhs, Shilhas or Silhachs, who inhabit the province of Sūs, and are therefore called Susis. There are other tribes still farther south.

It soon leaked out that the faith of Islam sits very lightly on some of the Susis, and that many of them really have no religion, or have some ancient superstitions which they preserve in secret. Whether they really believe the myths and traditions which they repeated to me, or merely regard them as our peasants do their fairy tales and folklore, it is hard to decide. One of them told me he was not sure that he was not as much of a Christian as of a Mohammedan — a statement that if overheard by the Moors might have cost him his life.

I made careful inquiries from several Rifflans, a Maltese who had travelled in the interior of Africa, disguised as a Moor, the town time-keeper or astronomer at Tangier, some Jews of Casablanca and Ophran, two Susis at Tangier, and one at Mogador, nearly all of whom, though illiterate, and unable to read Arabic, were learned in oral tradition and Berber folklore.

The results of my inquiries proved that there is a marvellous collection of ancient myths, legends, etc., among the Susis, which carry us in succession to Britain, Greece and Rome, Phœnicia and Egypt, and even to Babylon, while one very remarkable festival seems as if it had reached them from the Aztecs.

The Great Mother of the Greeks, Damater, appeared as *Ta Mata*, "the mother, who presides over the corn fields." Apălo, "a good god who comes and plays on a harp," suggests the inquiry, is not Apălo the original form of the name of the god Apollo?

At certain intervals, as the Greeks believed, Apollo used to desert his shrines, and go far off to the blessed Hyperboreans, with whom he danced and sang until the rising of the Pleiades.

The ancient Greeks themselves seem to have regarded the Atlas country as the favorite abode of Apollo. Atlas was a Hyperborean, and the western Ethiopians, who inhabited that country, were also Hyperboreans, a semi-divine race, from whom the Greeks derived many of their most ancient rites. It is supposed that they were called Hyperboreans from their enjoying a climate where the cold north wind was unknown, but the same name would be even more applicable to the people of the interior of Africa. I am inclined to think that it meant "those who dwell beyond Borium," a very ancient town on the borders of Mauritania, to which I shall hereafter refer.

A Susi described to me a staff ornamented with ribbons, which is called a *thurosis!* The names too of Mata, Kŏra and Zerēa, recall that of Ceres. The "Great Mother's" image is bathed at the end of her festival, as it was in the holy island of the Germans, at the river Almon, and in Athens at a feast of Minerva, which was therefore called *Plynteria*. We are even reminded of a similar rite at the end of the feast of the god of agriculture of the Fijians.

The god Adon is still believed to have been slain by a boar, and heaven and earth all weep for him. "He was greatly beloved by Tachal and Isai." It seems that some *festive dirges*, like the Maneros of the Egyptians, and the Linus of the Greeks, which were sung at their banquets, can still be traced in the *Accasili Maneros* and the *Wālinas* of the Susis. Diodorus Siculus tells us that in the Atlas country a divine youth, Hesperus, went up at night to the summit of a mountain to study the stars, and a great wind carried him away. To this day (I am told by the Susis) "the women go up on the mountains with music, weeping, in their search for Walinas. He was the brother of Panis, an old god who invented pipes called Kraf or Kalifer, and who was also called Itada."

One of the Susis asked me if I would like to hear "the story of the man who wished to steal the cows," and upon being asked to proceed, retailed a familiar bit of classical mythology: "There is a great mountain in the sea, where there were 300 cows the property of Geryon, and when the sun set they used to appear. It was a very rich place, and a navigator, who wished to steal the cows, sailed thither, and entered a great cave, from which he never came out; and his name was Herakles."

There can be no question that this mountain, the true Mount Atlas, was the Peak of Teneriffe, in which there is a vast cave that has never been fully explored. The Atlas country was the scene of the labors of Hercules, and of the feats of Perseus, who turned Atlas into a mountain by showing him the Gorgon's head.

Herodotus says that the dress of the statue of Minerva was borrowed from the Atlas country, where clothes of kid-skin are made and colored with great skill. Such dresses may still be seen in the Museum of Las Palmas, taken from the mummy caves of the Guanches. He also suggests that the story of the head of Medusa being encircled with snakes, arose from the head having been placed on a shield ornamented by the Atlantes with a fringe consisting of long strips of leather, which at a distance might well look like snakes. These fringes are still used by the Susis.

Even the Greeks admitted that one of their most unintelligible myths, that of the fifty daughters of Danaus, who were doomed in Hell to the task of filling sieves with water, came from Africa, and they therefore gave the brother of the Danaides the name of Ægyptus, though modern Egyptologists have failed to meet with the myth in the religion of the ancient Egyptians. Many years ago a devout believer in "Arkite lore" detected an allusion to the deluge in the name of Danaus, which he traced to *dan*, "water" or "rain." If he had called the fable a "rain myth," he would perhaps have been nearer the mark. It was evidently carried by the natives of the Atlas to Greece, where in time its original meaning was forgotten, for we still find it in the folklore of the Susis, one of whom told me that "there is an old King in the stars of rain, who has many dancing women, who hold sieves filled with water; and when he wishes them to dance, he thunders. The louder grow the peals, the quicker and wilder grows the dance, during which the sieves are emptied, and the water falls to the earth in a thunder shower."

These are a few only of the traditions and beliefs that carry us to Greece and Rome. We meet with Phœnician traditions also as to "Isiri who taught the three letters;" while the belief in an imperfect creation, in which the forms of animals and men were blended together, recalls a similar tradition of the old Chaldæans. Of Egyptian ideas there are perhaps traces in a belief as to seven brothers who sail in their ship across the sky, and carry with them the spirits of the dead.

The Susis have a May day festival at which the "pole of Maia" is set up, at the summit of which is a doll composed of heads of wheat. Saints climb up the tree and scatter the wheat among the people, calling it "our life," "our sustenance." The Mexicans used to erect an enormous cross, the symbol of rain, and on its summit was placed a similar doll, which when reached by those climbing the pole, was scattered among the people, who treasured the fragments as something sacred, while the deity represented was called "our life," "our support." The coincidence is certainly very remarkable, for precisely the same words were addressed by the Iroquois to the three beneficent maidens residing in the Pleiades, who brought each her gift to mortals, the maize, the squash, and the bean.

Herodotus has mentioned that the peculiar cry of the women at the rites of Minerva, called *ololuzein*, was borrowed from the Libyan women, "who sing it very sweetly." I have heard this peculiar chorus or cry which consists of a quick repetition of the word ălō,— ălō ălō ălō ălō. I am told that it is raised at the end of the feast of Mata when water is poured over the image of the goddess; and am reminded of the shout that resounded throughout Fiji at the close of the feast of Ratimaimbulu, who no doubt was the same as the god Alo-ălo of the adjacent Friendly or Tonga Islands. It would be exceedingly interesting if it should prove that the cry raised and carried from town to town in Fiji was, like that at the feast of Mata, *ălo ălo ălo ălo.*

If there is any foundation for the belief of the earliest nations and of the Susis themselves, that that country was once the seat of an ancient civilization, how can we account for the early rise of a great commercial and maritime people near Mount Atlas? The answer may be given in one word—*gold.* One of the natives examined at Mogador was Mordecai Rhibo, a Jew from Ophran, which he described as a very ancient town, which from remote ages has been the entrepôt of the Timbuctoo gold trade. At that point the caravans separate and go in different directions, one to the city of Marocco, and the other beyond Tripoli.

There are Jewish tombstones there dating back nearly to the beginning of our era. There is also a vague tradition that there is somewhere in the interior a tribe of Jews who are warlike and independent, and who have no knowledge of the Second Temple.

I ventured to suggest, when I read my paper, that we have good reason to believe that the *Ophir* of the Bible, and *Saba* or *Sheba* may yet be traced to that part of Africa. This has since been confirmed by my finding in Procopius, that one of the two great divisions of Mauritania was called *Zaba*. He also states that there was in his day (*circ.* A. D. 550) a very ancient city in Numidia, on the borders of Mauritania, called *Borium*, which from the most remote times had been inhabited by Jews, who had never paid tribute to any one. There was there too a very remarkable Jewish temple, which the Jews believed had been built by King Solomon. As the adjoining country was called *Zaba*, and as no trace of the first temple has ever been found in Jerusalem, it is not impossible that this was the temple to which the Queen of Sheba paid her famous visit.

It is probable that nomad Berbers, known as Sabæans, had, in the days of Abraham, as they still have, the monopoly of the gold and ivory trade of central Africa, and with their caravans carried its products, including slaves, parrots and incense, to central Asia. The Sabæans were, like the Susis, astrologers, necromancers, traders and robbers, believing in the seven heavens, and worshipping the seven stars. It has been conjectured that in the Puranas, traditions of an earthly paradise, differing from that of the general Hindoo system, seem to point to Africa (see Smith's Dictionary of Bible, *tit.* "Eden"). But the place to which we must turn cannot be, as Smith's article suggests, in southern Africa, but rather in northwestern Africa, where the garden of the Hesperides and the Islands of the Blessed were situated.

The Susis have a belief in seven heavens called *Saba Samagwats*. *Saba* means seven; the other word, by the aid of a pre-Malayan language in Malacca, can be interpreted, as it is the same as *samangats*, "the spirits of the Blessed," who reside in Pulo Bua, "the fruitful island in the west." I have also been shown a diagram of the mystic ladder for the descent and ascent of souls, called *Azacol*, representing "the path of the spirits" (*Azero*). It is like the ladder with seven lamps that typified the seven "houses" or "gates of heaven" in the rites of Mithra. These Susis are necromancers and astrologers, resembling the gipsies in looks, habits and ideas. One of them at the outset offered to bring up any spirit that I might wish to do my bidding,—an offer that recalled the question of the Witch of Endor, "And the woman said, whom shall I bring up unto thee?"

To show how indestructible the peculiar traditions of these people are, I may mention that a Susi told me gravely of a remarkable race at the river Byblah (Byblus?) "which is near the centre of the world somewhere between us and the Soudan," who have the faces of dogs. Herodotus, 2,500 years ago, was told precisely the same story, a belief in which, among the Egyptians, can be traced in their mystic *cynocephali.*

It may seem premature to endeavor to account for this strange collection of myths from apparently all parts of the world. A few facts connected with the history of Mount Atlas may be suggestive of interesting inquiries.

The earliest traditions of Greece point to Mount Atlas and to the garden of the Hesperides, which was on the flank of that mountain. The Susis told me that their people is the most ancient in the world. Diodorus Siculus says that the Atlantes claimed to be the most ancient of nations, and that their country was "*the birthplace of all the gods of antiquity.*" Solon was told by the Egyptian priests the same tale, that the Atlantes were the first great commercial and maritime people, and exceeded in wealth all the great nations of later times, and that they extended their conquests as far as Greece; but in consequence of a sudden irruption of the sea, the great island they inhabited was buried under the waves in a single night. History proves too that the Berber race was once dominant over northern Africa, and it is probable that they supplied the Hycsos or Shepherd dynasty that ruled over Egypt for centuries, and who have been connected with the Moors and Berbers by Movers.

One of the names of the Atlantes was *Maxyans*, which is possibly derived from a word in Arabic meaning "sheep." Even Amazighi (one of the numerous forms of their name) may come from *am* "of," and *ashigir*, "a bullock." Atlas, who, as a daring navigator, "knew all the depths of the ocean," and who taught Hercules astronomy, was also called "*a shepherd.*" The Susis (at least the nomad portion of them) are still called by the Arabs *Beni Baccar*, "the sons of the cow," the pastoral people. Even the word "Sos," according to Herodotus, meant, in the sacred language of Egypt, "cattle."

It has been conjectured that the light-haired race, that from the most remote ages occupied the Atlas country, lost their language and adopted the Berber tongue. I have recently found not less than seventy familiar English words in use in Marocco, and it is probable that careful inquiries will show a still larger survival of them in that country.

That Tarshish was a port in the Atlas country seems exceedingly probable. It is admitted that it was situated either in Spain or somewhere on the Atlantic seaboard. It was apparently the Birmingham of early ages, for its brazen dishes were an extensive article of export even to Phœnicia. We have now no trace of such an industry in Spain, but we have the clearest evidence that from the most remote times it flourished in Sus, the people of which seem to be a survival from the Bronze Age, for their principal trade is the manufacture of brass dishes, which they chase with marvellous taste, and which are perhaps known to us as the "beaten dishes" used in the Temple of Solomon. Leo Africanus says that Ifran was in his time (A. D., 1550) a seat of this industry, from the existence of extensive copper mines near that place. Though it is only known throughout Marocco as *Ephran*, its inhabitants call it *Ophran*, and such is the name given to it in the latest English map of that country.

To the ancient Jews it was probably known as Ophir, that mysterious city which tradition says was the capital of the Sabæans, and connected with Tarshish. That the latter was a port of Ophir or Ophran, and situated on the coast of Sus, seems probable from some curious incidents in the history of Jonah, who instead of going eastward from Jerusalem to Nineveh, went to the remotest west, in a ship bound to Tarshish.

To my surprise one day, a Susi told me that a great prophet was swallowed by a large fish and cast up by it on the coast of Sus, and I at first assumed that he had picked up this story from some Jew; but I have since discovered that it is an ancient local tradition as to Hercules, the hero of the Atlas, who must have sailed from a port in western Marocco, for on ancient maps we find a harbor there called "the port of Hercules." The two stories (whichever may have been the original one) point to a tradition connected with the Atlantic and the coast of Sus.

Many myths have a local origin in some natural phenomenon that primitive races cannot explain, except by the supernatural. We may find the key to this venerable tradition of antiquity as to Hercules in the existence, near the place in question, of sharp pointed rocks, which are fatal to whales that may be driven on them by a storm. Hercules, we are told (see Took's Pantheon, Part II, ch. 1; Ovid Met., II) "delivered Hesione, the daughter of Laomedon, King of Troy, from the whale in this manner; he raised, on a sudden, a bank in the place where Hesione was to be devoured, and stood armed before it; and when the whale came seeking his prey, Hercules leaped into his mouth, slided down his throat, destroyed him, and came away safe."

The tradition, which I had heard from a Susi, also existed in the days of Leo Africanus, who tells us that in the town of Messa, the name of which means "Lord," and at which the natives believe that the promised Messiah will appear, is a very ancient and sacred temple, the rafters of which all consist of the bones of whales, in commemoration of Jonah having been cast up by a whale on the adjacent seashore; and that in confirmation of this belief, the Moors pointed to the fact that all the whales immediately die that pass to the right of the temple. The historian was inclined to think that there might be something in the story, from his seeing at the time a dead whale floating near; but a Jew ridiculed the superstition of the Moors, and explained the origin of it, by the existence and effects of the reef in question,—that bank, no doubt, that Hercules raised suddenly for the destruction of the whale. I think there can be little question that this ancient whalebone temple of Jonah was originally built in honor of Hercules, the hero of the Atlas country; and it is possible that the story connected with it may have been carried back to Palestine by the Jews of Ophir, and have been preserved by them in the history of the prophet Jonah.

May not they also have brought back from Ophir the Book of Job? That work is a singularly faithful picture of the productions, animals, traditions, manners and astronomical ideas that are still to be found south of Mount Atlas. There to this day wandering bands from the desert sweep down upon the herdsman and the shepherd, and rob them of their herds and flocks; and the ostrich, the hippopotamus, the monsters of the ocean, the birds, the beasts, the treasures of the mine, and the stars, that are described by the patriarch, are still familiar to its inhabitants. Even that auspicious constellation whose "sweet influences" are celebrated by him, is known by the same name to the Susis, who call it *Kimah* ("a furrow"), or "the stars of tillage" as it is termed by the far distant Bechuanas of south Africa (*Silemela*).

The people of Sus also believe that there is a certain night in the year when the stars hold a solemn festival, in which all the angels and the spirits of the great kings of old take part. The very words of the song of the Pleiades, who are known in the New World as well as in the Old, as "the dancers," "the Celestial chorus" of the Greeks, "the Heavenly Host" of the Hebrews, and "the seven dancers" of the North American Indians, are familiar to ears that can catch "the music of the spheres," and have been repeated to me by one of those favored mortals, a Susi wanderer from the Sahara:

> "Oh Moon, oh Mother, we hold our feast to-night,
> We are dancing before God, between heaven and earth,"

words that recall Milton's allusion to those "morning stars that sang together with joy" at the creation,

> "And the Pleiades before him danced,
> Shedding sweet influences."

This celestial festival evidently takes place on that night in November, when the full moon and the Pleiades are on the meridian together, for there is a Susi love song,

> "Oh come to me my love, and long remain,
> For the Pleiades are meeting the moon to-night."

On that very night in November some tribes of the Australians still celebrate "the sweet influences of the Pleiades," and hold a grand corroboree in their honor, for "they are the children of the Sun and Moon," and "are very good to the blackfellows."

Even the early Egyptians seem to have borrowed many of their religious ideas from an older civilization in the Atlas country, for it has been conjectured that all the magical features of the ritual of the Egyptians, and their belief as to the dangers attending the passage of the soul to Hades, were derived from the people south of the Atlas (see Smith's Dict. of the Bible, *tit.*, "magic").

It is surprising to find that a country, venerated in the days of Homer, as peopled by "the just Ethiopians" who were nearer to the gods than other men, and at whose banquets even Jupiter was sometimes a guest, a country, too, associated with paradise and the abodes of the blessed, should, a few hundred years after his time, have been lost sight of by the world. Herodotus does not refer to its past history, and learned little of the country south of the Atlas. Strabo says that in his time it was a *terra incognita*, for armies and even travellers had seldom reached it, and the few natives that visited Greece either invented fables about it, or were unwilling to tell what they knew about it. To this day it is closed against Europeans, none being able to visit it except by the hazardous experiment of passing for a Moor.

Leo Africanus, however, himself a Moor, who has described that country as it appeared in his day, and has told us how the Arabs had ravaged it, destroying the cities, and burning the ancient books of the Berbers, states that near the walls of one town, the stones of which, as large as those employed in the construction of the Coliseum at Rome, had defied the fury of the invaders, gold and silver medals are to be found, with characters which he had in vain endeavored to decipher; and that everything indicates that at a former period these cities must have been the homes of a prosperous people.

This paper is the first attempt that has been made to draw attention to the traditions of a country that must once have played an important rôle in history. It is possible that future and more careful inquiries as to it may throw much light on the commerce, and perhaps on the origin of the Jews, and on many obscure points connected with early civilizations and mythologies; and that they may even justify the belief of the Susis that they are descended from the most ancient nation in the world, and the boast of the old Atlantes, that their land was "the birthplace of all the Gods of antiquity."

I intend, if my health permits me, to revisit that country, and to follow up these researches.

SOME HITHERTO UNNOTICED AFFINITIES BETWEEN ANCIENT CUSTOMS IN AMERICA AND THE OTHER CONTINENTS. By J. H. PHENÉ, of London, England.

[ABSTRACT.]

THE author of this paper explained that as one of the great problems to be worked out, or which at least it is desired should be worked out by all who have given any attention to the subject, is that of the races and civilizations which once flourished at the southern end of North America, and which had evident connections with the more northern parts of what are now the United States, through those great ducts, the Mississippi and other vast river valleys, tending to the latter direction.

The customs which are shown to have existed in the great river valleys of America, though read with difficulty by the light of the strange monuments still existing there, seemed to the author to have had parallel existences on the other continents. In evidence of this he gave illustrations, by drawings and diagrams, of many earthworks and stone constructions which had been examined by him, and which agreed in the method of executing the earthworks, of arranging the plans and designs, and in the evidently similar purposes for which they were designed, and to which they had been devoted.

The points on which he rested his argument for the affinity of the American mounds and earthworks, and, necessarily, the customs with which they were connected, with those of Great Britain and a large number of similar works in France, Spain, Greece, Asia Minor, Persia and China, were, in the first instance, the existence of relationship to each other, always in the vicinity of rivers, of mounds representing animal forms, and—with some special exceptions—the close proximity of vast works of camps for defence, huge enclosures or oppida for civil occupation, and other evidently sacred enclosures for solemn rites, worship and sepulture. In America these evidences went no farther, but in the vicinity of the mounds he had traced in Western and Eastern Europe, and in Asia Minor, and still farther East, not only were all these features attendant but the localities also abounded with mythological and traditionary legends, and the retention of strange and weird ceremonies to the present day.

He selected as illustrations of the American mounds, those in the forms of serpents, the alligator (or mythical dragon), and the human form. The serpent was shown by Mr. Squire to have been executed in two ways, viz.:—by the solid continuous serpentine form, and by a series of symmetrical mounds uniformly placed in curves. The reader gave examples of each of these, on diagrams, and explained that they existed in large numbers in Great Britain, similar in proportions and construction to those of the American mounds, which were kindred in form, and accompanying these were, he stated, in every case, extensive areas occupied by similar camps and enclosures for civil occupation, or oppida, and also separate enclosures for worship and sepulture. The characteristic physical, natural and art features were also curiously persistent in each case, a triple imagery having been in very many instances studiously represented, both by places of selection, as the vicinity of triple peaked mountains, and in construction, by triangular chambers and triangular enclosures in or about the head of the animal forms, as well in the American as in other similar mounds. Since his arrival at Montreal he had heard of indications of this having been an Indian burying ground. They appeared often to have selected places of previous occupation, as in the mounds; and the triple hill and trianglar piece of land at its base were significant. The works in Great Britain, Spain, France, and other countries, were found to contain chambers filled with cremative matter, and had studied arrangements for preserving the outlines of the animal forms, as shown in the diagrams, and were generally surrounded with vast lithic arrangements, and each, as a rule, was in the centre of an extensive necropolis of the primitive inhabitants. The legends and traditions clustering about these places were always of the same class in the other continents, and almost always had reference to man as the possessor and the serpent or dragon as the persecutor or destroyer, and this whether the tradition was one of uncultured and primitive existence, or of the highest classical art ages and localities; and though such traditions did not exist in America, yet not only did their huge mounds simulate these forms, but there had been dug up from their mounds rude sculptured figures of the human form and also of entwined or coiled serpents, showing that in their construction the same ideas, if not similar legends and traditions, had existed, as those which originated the gigantomaxia of Pergamos.

Amongst other numerous and striking examples of these forms in Great Britain, corresponding with the animal and human forms in America, were some very remarkable and yet but little-known representations of the human form belonging to a rude and unlettered age, perhaps corresponding to that of the semi-barbarous age of Mexico, which are found on the coasts of Devonshire and South Wales sculptured in stone, a very remarkable example of which also existed near West Hoathly, in Sussex, which was traditionally stated to have been worshipped as a potent deity (the goddess Andras) by the early Keltic inhabitants. In the vicinity of these rude figures in the western and eastern extremities of the South of England, were two enormous *intaglio* representations of the human form, corresponding to the *intaglio* forms in Wisconsin, only one of which had been mentioned, and that in a very cursory manner, by writers on the comparison of these works with those of America, while their surroundings had been completely overlooked. The one not referred to was by far the more important, both in size and similarity, being in the exact attitude of the mounds in the human form of the American mound-builders. About midway between these two figures was an equally enormous and well-known representation by *intaglio* of a horse. These figures were all cut in the chalk hills, and might be said to be protecting deities of the three localities in England most abounding in remarkable and vast areas of camps, oppida or settlements, and places of worship. The whole district from Dorsetshire to Land's End being commanded by the gigantic form in Dorsetshire; that of the miles of stone avenues on both sides of the Medway in Kent, terminated by Kit's Coity House at one end, several *alleés couverts* at the other, with the huge stone figure at West Hoathly and the wood of Anderida to the south, were commanded by the similar gigantic *intaglio* figure in Sussex; and the central and more important districts of Stonehenge, with the primitive and stupendous temple of Avebury, its sinuous avenues of stone, and extensive earth embankments, the great tumulus of Silbury Hill, the avenues of stones near Amesbury, in the style of those at Carnac in Brittany, the large number of camps, the trilithon called the "Devil's Den," and the great dolmen known as "Weyland Smith's Cave," appear under the protection of the "White Horse." Metallurgy, a great feature with the mound builders of the Mississippi, was also the peculiar occupation of one, if not of two, of these three districts of southern England and of many of the other localities in which similar works were found in Europe and Asia. And such metallurgy was for all utilitarian purposes confined in every such country in which these works exist to the two metals—tin and copper—and analysis showed the proportions in the amalgamation of those metals to have approximated in America, Europe and Asia. The parallelism went further and was continuous in its tract eastward. Brittany, Spain and France on each side of the Pyrenees, and so on to Asia Minor, had the same combined features. In the valley of the Meander in Lydia, the vast figure of "Niobe," mentioned by Homer, still existed, sculptured on the side of Mount Tmolus, and, as well as that of "Sesotris" in an adjacent valley, also so sculptured, overlooked the district in which are the multitudinous tumuli near the Gygean Lake; and the fortifications and camps of the acropolis of Sardis corresponded with those already referred to in position and design; and the same features were found at Ephesus, Smyrna and Pergamos in a striking degree, and extended, as shown by careful drawings and diagrams, onwards eastward as far as China. Considering the skill and thought required to plan such enormous figures with any regard to proportion, and seeing that all the figures had similar accompaniments, the author concluded by observing that they seemed to him to have been the result of a practice and culture transmitted with concurrent customs by way of the Pacific from one continent to another.

THE CROSS AND THE CRUCIFIX. By CHAS. WHITTLESEY, of Cleveland, Ohio.

[ABSTRACT.]

THE purport of this paper was to recommend great caution on the part of archæologists in giving religious significance to letters, symbols or hieroglyphics that may resemble more or less some forms of crosses or quasi-crosses. It was held that the *crucifix* should not be confounded with the cross, which assumes a great variety of forms which were discussed.

The Christian symbol originated at the crucifixion, and had an upright part extending above the arms of the olden cross. In nearly all ancient inscriptions there are letters or symbols that have a greater or less resemblance to the ancient cross. Unless something more is found than a resemblance of form, it is unsafe to draw inferences that they are connected with worship. The Runic characters furnish several that might be called crosses, but which are only letters.

CHIEF DEITIES IN AMERICAN RELIGIONS. By ALBERT S. GATSCHET, of Washington, D. C.

[ABSTRACT.]

THE science of comparative mythology is acquainted with the full religious systems of a *few* American religions only; the majority of them are known to us in a very imperfect, fragmentary manner. When we speak of *the religions* of American Indians, this term is not to be understood as bearing chiefly on the moral side of religion, or connecting morals with religious beliefs, as it is done in the monotheistic religions of the eastern hemisphere. With us a religious person is equivalent to a moral, honest person, and God is considered not only as almighty and omniscient, but also as the God of truth, forgiveness, and as the embodiment of moral principles. But rude and savage nations do not connect such ideas with their deities. Their deities represent the objects and powers of nature, as far as these have any influence on the welfare of man. They do not embody originally any moral principle, for the winds, clouds, rains and celestial bodies, which they represent, have nothing moral or immoral in them ; they are purely physical objects or powers. To become objects of worship, the forces of nature have to be personified; that is, human souls, passions and powers of reflection have to be imputed to them. Man feels himself in his welfare dependent upon the powers of nature and if he thinks them to be animated with souls and minds like his own, he can at least cherish the hope to communicate with and perhaps to propitiate them in spite of their stern and threatening countenance or their cruel destructiveness. In this early stage of religious feeling men seek to turn the more obnoxious deities in their favor by offerings, sacrifices or ascetic exercises, while the spirits representing the powers of nature favorable to him do not enforce the same attention and are therefore neglected by the ungrateful crowd. This condition of religious ideas necessarily implies *polytheism;* but in adaptation to the objects worshipped by the natives of America, we had better call *polydaimonism* the mode of worship existing in the western hemisphere.

The nations, which by their climatic position are destined to reach a higher stage of culture, will, from the religious condition of polydaimonism, which is an imperfect anthropomorphism, ascend by gradual and easy steps to that of real anthropomorphism.

Man being the most perfect being in organic nature, the savage mind naturally regards his nature-spirits as human beings endowed with miraculous powers; or should he continue to see animal monsters in them, he at leasts invests these with human form and garb, arms them with the weapons of man and hears them speaking in the accents of human speech. The more perfectly his nature-spirits become anthropomorphized, in the same degree intellectual and *moral* qualities will be imputed to those regarded as favorable deities, mischievous and immoral characteristics to the obnoxious ones.

After a deity has fully assumed the human shape, it frequently figures as an apparently historical character, as a legislator, ruler in peace, culture-hero, teacher of plastic, fictile and agricultural arts, canalizer of rivers or originator of the calendar. In the tropical regions of America the culture-heroes generally have a duplicate deity of the same origin and representing the same powers of nature, but less approachable and less popular. When this euhemerizing process is completed, miraculous stories decrease in number, while in the earlier stages they made up the bulk of the myths of each spirit-deity. No religion is imaginable without miracles as its base and foundation. Miracles are the childlike efforts of youthful humanity to account for the mysterious working of the natural forces, and owe their origin to the earliest attempts of popular metaphysics and philosophy, of which religion forms a portion.

Ethnology in its present condition cannot assume that there is or was any people on the face of the earth that had no religion in the scientific acceptation of the term. All have some theories to account for the origin of all that exists; all possess definite ideas of the nature of their good and obnoxious gods and of the mode of propitiating or appeasing them, though we Christians prefer to call this superstition, not religion.

The purely physical base upon which the American religions are built up precludes the possibility of anything like our Holy Spirit, the Trinity, hell and heaven or the devil, although early navigators have not failed to discover the latter figure on many of the Indian idols. The Eastern (Asiatic, African) religions present comparatively solid systems when compared with the unsystematic character of the American religious beliefs. Many of the American religions, like those of the Aztecs and Peruvians, are the result of a combination of the beliefs of several nations and therefore necessarily contain contradictions in their myths; but even where the mythology is the product of a single tribe it will show a lack of uniformity. The American deities are but symbols of the objects and powers of nature, as of the winds, the earth, the sky, sun and moon, night and day, the clouds, the rainbow, etc., and the eternal motions, *strifes* and *conflicts* of these reflect themselves in constant contradictions in the myths. The deities of the agricultural nations of the eastern hemisphere have left behind them long ago this early stage, and some of their number have, even in prehistoric times, developed into gods with predominantly mental and moral qualities, because at that early period they had already reached the full anthropomorphic stage. When, finally, by the invention of alphabets the composition of ritual and canonical books of religion had become possible, the beliefs then prevalent were consolidated into dogmatic systems, a powerful priest-class arose to uphold and teach them to the people, and the more highly civilized the nation was, the higher usually was the standard of morals ascribed by the canonical books to their principal deity.

In polytheistic and polydaimonic religions the deified objects and powers of nature are numberless. Objects and phenomena that are of little account to us often strike the imagination of the rude native through some peculiarity, and the result is deification. To every strange object is ascribed a body and a spirit or soul, which can live inside the body and outside of it and is regarded as something supernatural. Prominent deities often receive epithets and when these epithets are no longer understood by the people, because they contain an archaic form of the language, they often become gods or goddesses for themselves independent of the parent deity. Even the emblems of the divinities may become deities for themselves. By such and other causes the number of deities increase, by other causes a diminution is brought about and only the fittest will survive. Some are worshipped only in one province of a country, while others enjoy a more general worship.

In this motley crowd of new and ancient gods a few only rise to general prominence, and among these few one is considered as the principal deity. Many deities are supposed to be antagonists to the rule of this chief god, and though he is the most powerful among the gods, he is not the most popular god among men. Chief deities represent a combination of several powers of nature united in one supernatural being; they are often placed in the background by one or two more brilliant and therefore more popular conceptions of the religious mind, and if his obnoxious or terrific qualities make him less attractive to mankind, his irresistible, demoniac strength will leave him the object of the most intense dread. His attributes as well as those of all other deities are the result of the climatic condition of the country or countries in which the religion took its origin. For American religions the term *chief deity* is preferable to that of supreme being or supreme god, for "*God*" is too anthropomorphic and "*supreme*" involves a position too high to be imputed to any of the deities found in American mythology.

After examining the religious beliefs of a large number of tribes and giving abstracts of the more important beliefs on this subject of chief deities, I arrived at the conclusion, *that the majority of all American tribes worshipping the sun regard the sun deity as their chief deity.* It is only the imperfect state in which tribal mythology is generally brought to our knowledge that prevents us from pushing our inquiries further in this direction. Even in the earliest prehistoric epochs the rudest savages must have comprehended that *to the sun* mankind owes not only its daily food and nightly rest, that the earth owes its fecundity to his balmy rays and the rains which cause the waters to flow everywhere; neither were they ignorant of the fact that man's life and existence are the sun's work and therefore worshipped in the solar spirit the creator of man, of plants, of animals, the earth and of the whole universe. In the eastern hemisphere sun worship has been largely superseded or placed into the background by other deities, chiefly of a moral or more abstract nature; but even there the features of this primeval cult are still perceptible to the trained eye in the religions of many tribes and nations.

The sun often appears in the form of two deities in the same tribal religion; one may be of an abstract, the other of a concrete aspect; or the deity is thought to rule in the sky, and his counterpart appears on the earth as a culture-hero with an apparently historical garb. The solar deity is always in conflict with the lunar god and the clouds, but not with the deity of the skies. The lunar god holds the position of a chief deity in a few religions, but never appears as the creator or maintainer of the whole earth and universe. When the sun is regarded as a female, the moon often, though not always, appears as a male genius and in some tribes it is difficult to distinguish between solar and lunar god, because both celestial bodies are called *by the same term.*

Among the tribes, who worship the solar deity as their chief deity, are the following:

The Tupi god Tupan is a thunder god, but his attributes prove him to have been originally a solar genius. Tamo-i of the Guarani is "the old man of the sky," who is equivalent to the sun, like Tiri of the Yuracares, and the white-bearded Bótchika of the Muiska in Bogotá, while Dabaiba, on the isthmus of Panama, was probably a sun goddess also. In Peru three deities represented this celestial orb: Inti, Viracocha and Manco Capac; their worship originated at different epochs.

Tucapacha was the chief deity of the tribes of Michoacan, Coxcox of the Chichimecs. The Aztecs worshipped the sun in an abstract form in Tonatiuh, in a more concrete shape in Tezcatlipoca, who is the antagonist of the moon-god Quetzalcoatl, the latter corresponding to Kukulcan of the Maya religion.

A few of the Pueblo Indians of New Mexico worship Montézuma as their chief god, creator of the universe and culture-hero, while among the Californians the "chief above" or the "old man above" has a large number of votaries. Other Californians, like the Pit River Indians, regard the coyote-wolf as the principal deity and ascribe to him the creation of many objects in this world; the same may be said of a few Selish tribes on middle Columbia River, Washington Territory and Idaho, and of the Pápagos in Arizona. Coyote-worship is *originally* identical with lunar worship. The Wintún in northern California call their almighty power Noamkléstowa or "Great Spirit of the West." The main deity of the Klamath Lake and Modoc Indians of Oregon is K'muka'mtchiksh, the "primeval old man."

The Flathead-Selish of Montana pay their respects to a chief deity called Amotekēn, who is invisible and resides in the sky; he is possibly a genius of the skies, not of the sun. To the Niskwáli, Washington Territory, the chief deity is not the sun, but the moon, Slokwalm. The Haida Indians worship an abstract and a material sun-god, the former being the supreme ruler of the universe.

West of the Rocky Mountains we find among the majority of the tribes a deity called the *Master of Life*, the *Master of Breath*, the *Great Spirit*, etc., and in addition to this, a series of solar deities differing among each other according to the tribes in which they are found. These are but the concrete expressions of what the *Master of Life* expresses in a more abstract form.

Sun deities and chief deities at the same time are: the Michabo of the Odshibwē, Areskui of the Hurons, Kutnähä or "midday sun" of the Shetimasha in Louisiana, the deer-god of the Floridian Timucua and many others. The sun-dance of the Sioux perpetuates sun-worship up to the present time, when a large proportion of them have become Christians.

INDIAN MIGRATIONS AS EVIDENCED BY LANGUAGE. By HORATIO HALE, of Clinton, Ontario, Canada.

[ABSTRACT.[1]]

IN this paper the author attempts to trace, by the evidence of language, reinforced to some extent by that of tradition, the course of migration which has been followed by the tribes belonging to some of the principal linguistic stocks of North America. The Cherokees are shown to belong to the Huron-Iroquois stock, but to have received accessions to their vocabulary from some other source. The Huron language is shown to be the oldest in form among the languages of this stock. The migration of the Huron-Cherokee tribes is traced in a course leading from the northeast to the southwest, that is, from the lower St. Lawrence to northern Alabama. The Dakotan stock is next considered. The Tuteloes of Virginia and North Carolina are shown to belong to this stock, and to speak a language which is older in its forms than the languages of the western Dakota tribes. The Algonkin tribes and languages are next examined, and the evidence is exhibited which shows that their migration probably flowed from Hudson's Bay and Labrador towards the south and west. The tribes of the Chahta-Muskoki family are noticed, and the fact is pointed out that their language, like that of the Cherokees, has apparently been modified by admixture with some alien speech. Some reasons are given for supposing that this speech was that of the Moundbuilders of the Ohio Valley. Traditionary and linguistic evidence is adduced to show that the Moundbuilders were conquered and partly exterminated by the Iroquois and Algonkins, and that the survivors, mingling with the Cherokees and Choctaws, caused great changes in the languages of those nations. The fact that the course of migration seems to have been from the Atlantic coast towards the interior is regarded as evidence that the ancestors of our Indian tribes were emigrants from Europe. In support of this opinion, reference is made to the close resemblance in structure between the Basque and the Indian languages. It is further suggested that if the Aryan intruders, entering Europe from the east, encountered and absorbed a population resembling the American aborigines, this fact would account for the great changes which the Aryan speech underwent in central and western Europe. It would also account for a very remarkable change which took place in the character of the intruding race. The Aryans, who in the east have always been a submissive and contemplative race, devoid of the idea of popular government, become in Europe a high-spirited, practical, and liberty-loving people. The conclusion is that the nations of modern Europe are a mixed race, forming a transition, in physical and mental traits, between the eastern Aryans and the aboriginal Americans.

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[1] This paper is published in full in the American Antiquarian for January and April, 1883.

THE KINSHIP SYSTEM AND MARRIAGE LAWS OF THE DHEGIHA, ILLUSTRATED BY CHARTS. By J. OWEN DORSEY, of Washington, D. C.

[ABSTRACT.[1]]

MARRIAGE laws based on kinship system, which, in turn, is founded on the gentile organization of the tribe. The last is affected by the customs pertaining to the buffalo hunt.

Chart I. The Omaha Tribal Circle, as formed during the hunt, and showing the gentes. The gentes divided into sub-gentes, for certain reasons. Father-right.

Chart II. "Consanguinities." Classes of the consanguinities of "SELF, a male."

Classes of the affinities of "SELF, a male," as shown in Chart III.

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[1] The charts illustrating this paper are by necessity omitted.

Peculiarities of the charts caused by the marriage law: "SELF can marry any woman whom his wife calls *sister*, *brother's daughter*, or *father's sister*." Actual and potential kindred. The kinship formed by the calumet dance.

From the examples given in the charts, it is plain that among the Dhegiha, terms of kinship are used with latitude, differing from our ordinary acceptation of those terms. Then follows the full definition of each term. Women whom a man cannot marry. Men whom a woman cannot marry.

Women whom a man can marry. Men whom a woman can marry. Marriage of a brother's widow. Law concerning widows and widowers.

THE SUN DANCE OF THE OGALALLA SIOUX. By ALICE C. FLETCHER, of Cambridge, Mass.

THE sun dance is a tribal festival at which attendance is compulsory. The dance proper is a strictly religious rite, entered voluntarily and performed in fulfilment of vows made in sickness or trouble in order to secure health and prosperity. The dance proper is a centre about which many festivities cluster, incident to a great annual gathering. Thanksgiving and a desire for future benefits are the essential mental characteristics; the recognition of supernatural powers and dependence upon them, together with obligations of faithfully performing a vow are the religious characteristics; the exact observance of certain forms and rituals is demanded of the priests and their coöperation is needful to secure the full blessing sought through the ceremony by those taking part in it.

The festival generally occurs in the latter part of June or early in July and lasts about six days. The time is fixed by the budding of the *Artemesia ludoviciana*. The people camp in a circle, with a large opening at the east. In 1882, over 9,000 Indians were so camped, the diameter of the circle being over ¾ of a mile. The tent set apart for the consecrating ceremonies, which take place after sunset of the first day, was pitched within the line of tents, on the site formerly assigned to one of the sacred tents.

Any number of persons can dance, provided they have vowed to do so, but each year there can be but one Leader. All the dancers must conform to fixed rules and regulations. The dance may be divided into four degrees.

1. That which only the leader can pass through. He must vow, procure the articles for the ceremonial, abstain from forbidden things and actions, fast, dance, give away property, be scarified and tortured.

2. To vow, abstain, fast, dance, give away, be scarified and tortured.

3. To vow, abstain, fast, dance, give away, and be scarified.

4. To vow, abstain, fast, dance, and give away.

Twenty-two persons danced and passed these degrees as follows: First degree; the leader only. Second degree; one man. Third degree; seven men, one woman (I could learn of no woman ever having been tortured). Fourth degree; eleven men, one boy (11 or 12 years old).

The leader is required to make the vow, depend upon it for recovery, if sick, or deliverance, if in trouble. Soon after, he must set about fulfilling the duties of the vow. These are, to kill a buffalo, preserve the hide, which must never be taken into a lodge, keep the fat in skin bags, save the skull and two chips; lay away a red-stone pipe, the stem ornamented with porcupine quill-work; accumulate possessions to give away at the festival; abstain from running, swimming, loud talking, boisterous actions; live at peace and spend much time contemplating the fulfilment of his vow. In this manner the winter is passed. Early in June the leader selects a friend to be his assistant and these two proceed to one of the Holy-men, who has learned the ritual of the dance, and with certain ceremonies offer him the charge of the dance. From the moment of his acceptance, the leader places himself under the implicit guidance of this officiating priest. Every dancer chooses a lay assistant and a priestly guide. The leader's assistant, at the direction of the officiating priest, obtains an axe, awl, needle and four knives, all of which must be new; these are for the consecrating ceremonies and to be used in the rites. The officiating priest summons the people to the place selected for the festival.

The opening ceremonies begin with the entrance of the officiating priest and the leader, accompanied by their attendants, into the tent before mentioned. All are painted red and wear the buffalo robe in the sacred ceremonial manner, the hair outside, the head on the left arm which is folded over the tail on the right arm. A fire is in the centre of the tent, a spray of sweet grass laid on it, the axe consecrated in the aromatic smoke and afterward used to cut the sod from an oblong space drawn on the earth back of the fire. The brown exposed earth is then mellowed, and a few live coals laid in the centre of the figure, sweet grass dropped on them, and through the smoke all the articles to be used during the ceremonial are passed for consecration, after having been first wiped with Artemesia. During this ceremony the priest chants the ritual, as he sits facing the east. The pipe is filled, smoked and passed, stem downward. The tent cloth is raised that all the people may hear the prayers chanted. These prayers are only spoken on this occasion, once a year.

The dancers every morning repair to this tent while certain rituals are sung, and in the evening they come here to dance and sing the songs of the festival. The people during these days enjoy feasts, dances, etc., incident to a reunion. The entire camp, however, is under the charge of men appointed by the priest to preserve order and carry out the regulations of the festival. On the fourth day the site of the pole is chosen by men set apart for the task, and the tree which is to serve as the pole is selected by young men appointed to the duty, and a man of known worth is designated to dig the hole for the pole. On the morning of this day the priest, leader and attendants repair to the selected tree, present the pipe, lifting the stem for the first time, after which a pipe is laid at the foot of the tree, a circle drawn on the ground near it, an offering of calico tied on the tree just below the branch which forks toward the east, and a man appointed to remain and watch the spot and suffer no living creature to pass over the consecrated ground. At noon the two singing societies repair to the place, and seven priests then light and smoke the pipe left by the leader, in accordance with special ceremonies. After this, five men and three girls are chosen to cut the tree, observing the order of procedure peculiar to this part of the festival. The tree is then struck with the sacred axe by the first young man, he recounting his claim to receive the honor; he leaves the axe sticking in the gash, the second young man pulls it out, lifts it, recounts his deeds, and strikes, leaving the axe in the tree, and so on by the five men, and the tree is thus girdled with gashes. The girls then fell the tree which must drop to the west. It is carried to the camp circle on a litter of sticks and must not be handled or stepped over.

Before it reaches the camp, the open circle is charged by all the able-bodied men in gala dress, their ponies decked and painted. They dash across the plain at full run, and skirt the outside of the line of tents. The pole is painted and the offerings tied upon it and erected with elaborate rites. After which a circular enclosure of boughs, having an opening at the east, and a roof formed of tent poles, resting on crotched sticks and covered with tent cloths. The poles and tent cloths are contributed by the different divisions of the tribe. The ceremony of peircing the children's ears is now in order, and a vast amount of possessions changes hands during these ceremonies. The rites incident to the puberty of girls take place at this time. On the following day at noon the embowered circle about the pole is charged by men on foot, who make four advances, led by the chosen warriors, and at the fourth charge, pass out at the eastern entrance. Soon after, the sun dancers enter, naked to the waist, with embroidered widths of cloth reaching nearly to their ankles and belted at their waists. Their feet are bare, on their heads are crowns of Artemesia with horns cut from rawhide and painted blue. In their hands are sprays of Artemesia. The leader carries in his arms the buffalo skull; with the priest followed by their attendants, they slowly pass round the circle by the south to east, the skull being placed opposite the pole and the entrance. The space behind the skull is occupied by the leader and officiating priest, the other dancers with their priests and attendants are to the right and left. The dancers have fasted, tasting neither food nor water, since the time the pole was cut, and they now dance, at intervals during the rest of the day and night and until after noon of the following day. When they dance they use a whistle which is made from the wing bone of an eagle, ornamented at one end by a peculiar, soft feather used in religious ceremonies. They blow this whistle to the rhythm of the song which is sung by the societies, accompanied by a drum made of a hide dried for this occasion.

On the morning of the sixth day, the buffalo skull is painted with a sign in blue color, indicating the four winds. An oval space is marked out on the earth behind it, the sod cut off and laid to the east of the oval; upon the top of this heap the skull is placed. The soil within the oval is then mellowed and a figure similar to that painted upon the skull is cut upon it, and into it tobacco is dropped. The soil of the oval and the skull are painted with dashes of red ochre. A rest for the pipe is set up, just behind the oval, the bowl of the pipe put on a buffalo-chip, the rest supporting the stem. Artemesia is spread back of this and another buffalo-chip set further back to receive the ashes. Sweet grass is placed in given spots and offerings of food placed on the ground near the skull. At this time all the dancers are painted according to the visions of their respective priests. Scarifications are next performed. After noon, the leader is led to the pole, when according to certain ceremonies the flesh is punctured a little above the nipples by a wedge-shaped knife, and a stick having one end embroidered with porcupine quills is inserted. To this skewer the raw hide rope fastened to the pole is secured, and the man is led out toward the east until the rope is taut. A large amount of goods are then given away, and many ceremonies take place to insure him a speedy release. When a certain song is started, he must put his whistle in his mouth and bracing himself pull with all his force, until he shall tear the flesh loose. When one side gives way he raises his hand, palm upward to the sky, as a sign of thanks for the deliverance. With one man the struggle lasted nearly twenty minutes. When these tortures are completed the festival is over.

THE DAVENPORT TABLETS. By HORATIO N. RUST, of Pasadena, Cal.

[ABSTRACT.]

BELIEVING that the pictures on the Davenport Tablets lack evidence of great antiquity, and are the work of the Indians who were recent Moundbuilders, I took representations of the Tablets to the Dakotas, showing them to several of the older members of the tribe, telling them how they were found, and asking what they meant. The answers were in each instance substantially the same and without hesitation, viz.:

That the picture of the "mound" represents an earth lodge, in which a dance was being held.

The "prostrate forms" represented those persons who were overcome by the efforts and excitement of the dance.

The "curling smoke" arose from a fire in the lodge, indicating that the dance was held in cold weather.

The "moon and stars" signify that the dance was conducted in the night.

The "upright marks" around the lodge represent a fence of sticks set in the ground. A common custom among them to-day.

The irregular markings which some persons have tried to interpret as evidence of a written language, were simply ornamental markings conveying no intelligence.

Similar earth lodges destroyed by time and other causes have formed many mounds and groups of mounds, which now may be seen upon the benches all along the Missouri River.

A FIND OF CEREMONIAL WEAPONS IN A FLORIDA MOUND, WITH BRIEF NOTICE OF OTHER MOUNDS IN THAT STATE. By A. E. DOUGLASS, of New York, N. Y.

[ABSTRACT.]

WHILE considerable attention has been paid to the sand and shell mounds upon the western or Gulf coast of Florida, as well as those upon the St. John's river and its tributaries, the mounds distributed in equal abundance along the Atlantic border of the State have up to the present time almost escaped notice. Gradually they are fading away under the corrosion of agriculture and the elements, and the more serious evil of the curiosity of relic hunters, intent simply upon the acquisition of some object of pecuniary value, indifferent meanwhile to the characteristics of the mound they destroy, and to the facts attending the locality of the objects obtained which may invest them with peculiar archæological value. Relics of metal have gone into the melting pot, and others of stone have been broken in the handling and finally thrown away and lost.

Employing a few weeks in each winter during the last four years, I have taken what I may call an enthusiastic interest in recording the characteristic features of and in excavating more than forty of these sand mounds, comprising all that I was able to hear of, on a seaboard of about one hundred and twenty miles in length, and averaging twelve miles in width. This district, commencing about ten miles south of the mouth of the St. John's river, extends to the southern limit of Mosquito Lagoon, and comprises the eastern halves of St. John's and Volusia Counties. Like the seaboard of North and South Carolina and Georgia, the coast of Florida presents a continuous line of outlying islands or long and narrow peninsulas, wholly or partially separated from the mainland by salt water lagoons which afford navigable water channels for the entire distance of three hundred miles from the mouth of the St. John's to Jupiter Inlet, except when interrupted by narrow tongues of land connecting with the mainland which interpose in the aggregate about twelve miles of land carriage.

The *shell mounds* in the district I have explored are mostly found upon the coast islands or peninsulas, while the *sand* mounds are, with very few exceptions, found upon the mainland, and generally near to streams of fresh water, which, draining the swamps, hammocks and pine barrens of the back country empty into the salt lagoons. The relics found in the sand mounds were quite abundant in St. John's County, but became more infrequent as I entered the northeastern border of Volusia County. The mounds themselves, however, gradually increased in dimensions, averaging north of the Halifax river not over ten feet in height, while those south of Mosquito Inlet ranged in height from twelve to twenty-three feet. Another remarkable fact was the prevalence of the celt, sometimes abundantly, in the St. John's County mounds, while south of the northern extremity of the Halifax river, in an examination of eighteen mounds, not one of these implements appeared.

The first sand mound I had the pleasure of excavating was what was called the Sanchez Mound, about eight miles north of St. Augustine, on a point of land formed by the confluence of the North river and Guano Creek. Though of moderate dimensions, nine feet in height with a base diameter of seventy-five feet, it produced a most abundant yield of celts (24) in connection with many other interesting objects. Four of these celts I venture to submit for your inspection as samples of more than one-half the number, the remainder being but little inferior except in condition and by reason of injury. The workmanship of these objects could hardly be excelled even with the superior tools we now possess, nor their symmetry more than equalled by the skilled artisan of the present day.

We estimated that there were over twenty original burials in this mound, if we may so term deposits of bones made after years of exposure to the elements had wasted all but the most substantial. These were deposited as nearly as we could ascertain about two feet above the original level of the ground, and occupied a circle of eighteen feet in diameter. Each cluster of bones was surmounted by the skull, and the whole mass encrusted with red paint which discolored the sand an inch or two around them, and proved a useful guide to the workmen in detecting their proximity. Only two flint spearheads were found, and it may be well to note that, in arrow and spearpoints combined, the aggregate product of forty-one mounds was less than twenty. This is a remarkable fact when we consider that the Spanish historians, without exception, make no mention of stone implements other than arrowheads in use by the Indians at the time of the discovery of Florida, while the abundance of celts exhumed in this and other mounds would indicate that at some previous time *they* were in general use. None of these celts were found in juxtaposition with the bones, but were scattered singly throughout the interior of the circle at depths from the summit plateau varying from two to six feet. No beads were found in this mound, but in others they were occasionally abundant and were generally found in quantities laid immediately upon the skull which capped each deposit of bones.

The most singular and interesting discovery, it has been my good fortune to make during these explorations, occurred while excavating Banner Mound, known generally throughout the country as Mound Oswald. This is situated upon a tongue of elevated land at the intersection of Tomoka creek with the Halifax river in the northeast section of Volusia County. This creek drains a large expanse of swamp lying some twelve miles east of the town of Volusia (upon the St. John's river) and is navigable from its mouth for a distance of ten miles for vessels drawing five feet water. Running for this distance nearly due north, it intersects the Halifax river, whose course is from northwest to southeast, at a very acute angle, and the point thus formed was under continued cultivation from early English times, say 1770 up to the Indian wars of 1837–42, when the settlers were finally driven off, and since when it has been deserted. It is now covered with a magnificent park-like growth of oaks, palmettoes and pines. The surface slightly undulating is elevated on the average ten feet above the level of the water, and about a mile and a half from the extreme point forms a series of parallel ridges, possibly the vestiges of an ancient sea beach. Upon the western termination of one of these ridges, this mound was raised, the constructors, as is frequently the case, availing of the natural elevation to give a more imposing height to the structure on one side at least. Hence, while on the north its perpendicular height appears as fourteen feet, on the south where the natural depression has been increased by excation for materials, its height appears as twenty-four feet. On the north the slope of the mound rises abruptly at an angle of forty-five degrees, while on the south it rises gently to the summit at an angle of thirty degrees. The plateau on the summit (common to most of these larger mounds) was slightly convex and twenty-eight feet in diameter. The southern half of this plateau was covered with a dense scrub, while the centre and northern area was crowned with palmettoes, oaks, hickories and sweet bay, all of great size. A trench twelve feet long, and from three to five feet wide had been excavated by some previous explorer from the southeastern edge of the plateau to a point about eight feet north of the centre. Commencing with a depth of three feet it gradually descended to a depth of eight feet at its terminus. The fruits of this enterprise, undertaken as I understood some years before by a party attached to the Coast Survey, I never learned; had the result been fortunate no doubt it would have been continued over the entire surface; possibly therefore it proved a failure. The labor of clearing the surface of a large mound of the growth of scrub and timber is very considerable, and, without doing this, it is exceedingly difficult to estimate the true position of the centre toward which the excavation must usually be directed as likely to produce the best result. After clearing the plateau of trees (a work of severe labor and occupying several hours), we commenced a trench along the edge of the plateau, twenty feet in length, four in width and the same in depth. Occupying this, the six men were then faced toward an objective point on the opposite edge of the plateau, and we proceeded to excavate a triangular pit, descending as we advanced till at the centre we reached a depth of fourteen feet. Before doing this, the surface of the mound was carefully raked over and the debris examined, as objects lightly buried are occasionally exposed by the wear and tear of storms for centuries, and might, without this precaution, be again buried. In this instance we found a stone ornament designed as we supposed to be used as the brooch for a necklace of shell. Our labor the first day was otherwise quite fruitless, but on the morning of the second day one of my crew, practised in the work, detected by the sound upon his spade a substance foreign to the sand, and dropping the implement went to work with his hands, and quickly unearthed a beautiful ceremonial weapon popularly known as a banner stone. Groping about the spot with his hands he encountered one after another until five had been brought to light all in perfect condition. They were lying upon the same plane, but a few inches from each other, and fourteen inches only beneath the surface. It was not till two hours later, that, working upon a lower level, he unearthed three more of the same objects. These latter were found immediately below the first five at a further depth of three feet, and were placed vertically one above the other, separated by a film or thin stratum of sand. With one exception these eight objects are as fresh and new, save a slight discoloration, as when they left the maker's hands. They are the only objects of the class I have ever found in the Florida mounds, nor have I heard of the discovery of any in the state. This fact coupled with the number in this find, and the evidently studied design in their arrangement, warrants me in considering them the stock of a manufacturer and trader in such objects, who had availed of this mound to conceal his store, while he singly disposed of them, or else finding his safety risked had sought to hide them until he could again venture into the neighborhood and resume his trade. All these objects, as I before said, are, with one exception, absolutely perfect. They might, save for a slight discoloration, have left the makers' hands but yesterday, so clean and finished and unblemished do they appear. This exceptional one, however, is plainly many centuries older. It is covered with an amount of patina only to be acquired by exposure for a vast period of time. Five of these objects are of this special pattern, and one of its characteristics is common to all the specimens, that is, the ridge which accommodates the perforation is on one side *rounded*, on the other *flattened*. It seems a fair inference after inspecting these objects, that the maker becoming possessed of this ancient specimen used it as a pattern for the rest, and may have used it for many another.

This specimen (fig. 1) consists of two semicircular flanges, or wings or blades of thin stone, starting on opposite sides from a thicker ridge perforated throughout its extent. From edge to edge of these flanges the distance is five and one-fourth inches, the length along the axis or ridge is four inches, the perforation throughout the length of the ridge is one-half inch in diameter and absolutely true. Two others of this pattern are about the same dimensions, and two others are slightly smaller. Again, two other specimens represent what may be called the double battle-axe pattern (fig.2).

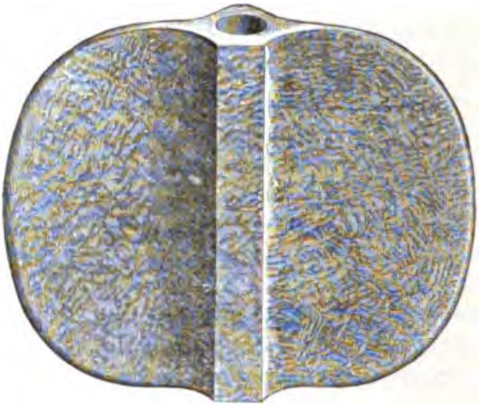

FIG. 1.

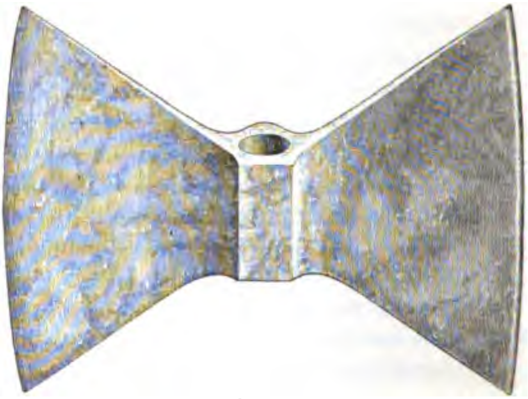

FIG. 2.

The flattened edge of the blades forbids any idea of their having been intended or used for any warlike purpose. The distance from one edge to the other is, in one of these objects, five and a quarter, and in the other, five inches. These blades or flanges contract in width as they approach each other and merge into an axis or perforated ridge two and one-fourth inches in length, seven-eighths of an inch in thickness, while the perforation through its length is one-half inch in diameter. These two similar objects laid athwart each other would fairly represent a Maltese cross. The eighth specimen (fig. 3) differs from the first five, only in suppressing the ridge on one side of the object altogether, and thus having one surface absolutely flat, while a rounded ridge shows upon the other. The peculiarity of all these objects is the possession of this ridge flattened upon one side and rounded upon the other. Of one hundred and fifteen objects belonging to the class of ceremonial weapons in my possession, these and one other in a fragmentary condition alone present this characteristic. It is remarkable how from the condition and arrangement of this deposit one can trace the mode in which the trader buried these objects, and mark his ingenuity as he continued to conceal and render secure a part, if not all, of his hoard.

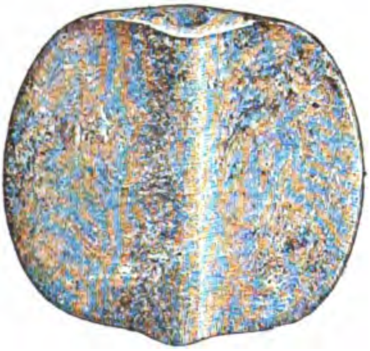

FIG. 3.

Selecting for the purpose a mound which the local tribes would be little likely to disturb, he proceeded hastily to excavate a cavity wherein he could spread them at length. He needed an implement for the purpose, and the ancient specimen before you answered most admirably. Observe how the attrition of the hand holding it has polished the patina of the one flange, while the other presents a roughened edge fractured and splintered through the overlying patina by the use to which he put it, and showing in the fracture the natural color of the stone. But, as he digs, his ingenuity finds display in separating the hoard into two parts, so that if one should be discovered the other part might escape detection. After clearing a space sufficiently long and wide to permit of his spreading the five at length, he sank in the centre a circular hole three feet in depth, the extreme length of his arm and implement, and here he deposited three of the objects one above the other, as they were found, filling up the hole with sand to the level above. All that remained was to spread out the five remaining specimens in the trough prepared for them, and heaping the sand above them by means of the same ancient tool, he finally thrust it into the end of the trench, and there it was my good fortune to find it with the rest, some four centuries, perhaps more, afterwards. It remains but to say that the mound proved entirely destitute of burials either original or intrusive, nor were any other objects of interest found. We worked diligently until our excavation had reached the point aimed at on the opposite edge of the summit plateau, but without result. I would say briefly that this whole district must in ancient times have supported a most abundant population. About two miles to the eastward upon the opposite shore of the Halifax river, a mound of shell débris extends for a distance of nearly ten miles along the shore. Its average elevation is about fifteen feet, and its width at base one hundred and fifty feet. It is clothed with matted vegetation upon the sides, and crowned with noble oaks and pines upon its ridge. Many other shell mounds and fields are found in the neighborhood, all witnessing an abundant population in the unknown past.

RECENT ARCHÆOLOGICAL DISCOVERIES IN VERMONT. By G. H. PERKINS, of Burlington, Vt.

[ABSTRACT.]

NOT far from Middlebury, Vt., some peculiar specimens have been found under conditions which make it probable that they are the oldest implements yet discovered in the state. The surface soil at this locality is a clay-loam about a foot in depth.

Under this is a yellow subsoil. An apple tree in the orchard of a farmer was uprooted and in clearing away the débris stone implements were discovered. This led to further investigation and by persevering efforts quite a collection of objects was obtained. These were in the yellow subsoil from 18 to 24 inches below the loam and from 30 to 36 inches below the surface sod. There was no evidence that the soil had been disturbed or that the objects found had been deposited in graves. The specimens are themselves peculiar, being unlike any others found in Vermont. The material is in most cases a brownish siliceous stone from which "turtle backs" and other large and rather rude objects had been flaked. Ground implements, as celts and other like objects also have been found and a short cylinder of hard-burned pottery. Another interesting locality which I visited last summer proved to be the site of a village. Old settlers near by say that fifty years ago many skeletons were found, but none can be found at present. However, from all the cultivated fields which border what is known as Bristol pond in Addison county, great numbers of implements have been obtained. These fields are many of them on hillocks and the soil is in some of them clay, in others sandy, and in either case there are few stones of any sort except angular fragments brought by the former occupants. These are of all sizes from masses weighing eight or ten pounds to small flakes, the latter predominating. Over considerable areas the ground is completely filled with flakes and the total amount of material which must have been transported from a considerable distance to this village is very great. Hammerstones are, or rather were, for the region has been thoroughly looked over, so abundant as to be gathered by the basketful, and I presume that first and last a small wagon load of these implements has been carried off from this locality. The flaked implements are peculiar in their large size and rudeness. The material is almost uniformly gray quartzite. It seems probable that the large flaked implements were used as hoes or spades. It is difficult to guess at any other use to which they may have been put. Well ground celts and axes, pestles, amulets, banner stones, etc., occur and also finely flaked knives and arrowpoints, but by far the greater number of specimens found are either rather rudely made "turtle backs," or triangular or lanceolate objects from three to six inches long and two to four inches wide. Fragments of pottery of very good quality have also been found and one bone object, the tyne of a deer's horn notched at the base as if for the attachment of a cord. It is not supposed that this village site was occupied in very remote times—there is no evidence of that—but yet it must have been before the occupation of the country by white men, since had there been a settlement there then, some tradition or account of it would remain until now.

TITLES OF OTHER PAPERS READ IN SECTION H.

A SCHEME OF ANTHROPOLOGY. By O. T. Mason, of Washington, D. C.

ON THE COMPARATIVE PHONOLOGY OF FOUR SIOUAN LANGUAGES. By J. Owen Dorsey, of Washington, D. C.

NOTICE OF A COLLECTION OF SIOUX WEAPONS AND ARTICLES OF DRESS. By G. H. Perkins, of Burlington, Vt.

EXHIBITION OF A COLLECTION OF CHIPPED-STONE IMPLEMENTS FROM THE PACIFIC COAST. By H. N. Rust, of Pasadena, Cal.

WHO BUILT THE MOUNDS? By P. R. Hoy, of Racine, Wis.

WHO MADE THE NATIVE-COPPER IMPLEMENTS OF NORTH AMERICA? By P. R. Hoy, of Racine, Wis.

MONUMENTAL AND ART REMAINS IN THE LAKE REGIONS OF OHIO, PENNSYLVANIA AND NEW YORK. By Wills De Hass, of Washington, D. C.

ARCHÆOLOGICAL EXPLORATION: PROGRESS OF DISCOVERY. By Wills De Hass, of Washington, D. C.

MOUNTAIN ANTIQUITIES. By Wills De Hass, of Washington, D. C.

GEOLOGICAL TESTIMONY TO THE ANTIQUITY OF MAN IN AMERICA. By Wills De Hass, of Washington, D. C.

"PAGAN" IROQUOIS FESTIVALS. By Erminnie A. Smith, of Jersey City, N. J.

BELIEFS AND SUPERSTITIONS OF THE IROQUOIS INDIANS.[1] By Erminnie A. Smith, of Jersey City, N. J.

A FEW DEDUCTIONS FROM A "DICTIONARY OF THE TUSCARORA DIALECT." By Erminnie A. Smith, of Jersey City, N. J.

HOME LIFE AMONG SOME OF THE NORTH AMERICAN INDIAN TRIBES. By Alice C. Fletcher, of Cambridge, Mass.

THE BLEACHING OF THE ARYANS. WAS IT DONE BY THE LAW OF REPETITION; THE ACCUMULATION OF EFFECTS? By Virginia K. Bowers, of Newport, Ky.

INFLUENCE OF CLIMATE OF CANADA ON EUROPEANS. By Wm. H. Hingston, of Montreal, Canada.

STONE IMPLEMENTS OF BOMOSEEN AND CASTLETON RIVER VALLEYS. By John McNab Currier, of Castleton, Vt.

A STONE-GRAVE IN ILLINOIS[2]. By Charles Rau, of Washington, D. C.

ON COPPER IMPLEMENTS AND ORNAMENTS FROM NORTH AMERICA.[3] By F. W. Putnam, of Cambridge, Mass.

THE CONTENTS OF EIGHTY-FOUR STONE-GRAVES AT BRENTWOOD, TENN.[4] By F. W. Putnam, of Cambridge, Mass.

ACCOUNT OF THREE MOUNDS EXPLORED IN OHIO AND TENNESSEE.[4] By F. W. Putnam, of Cambridge, Mass.

DISCOVERY OF THE REMAINS OF A LOG STRUCTURE BELONGING TO THE STONE-GRAVE PERIOD IN TENNESSEE.[4] By F. W. Putnam, of Cambridge, Mass.

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[1] Will be printed in full in Second Annual Report of the Bureau of Ethnology Smithsonian Institution.

[2] Published in the American Naturalist, 1883.

[3] Published in full with illustrations in 15th Report Peabody Museum of American Archæology and Ethnology, Cambridge, 1882.

[4] The substance of these three papers will be given in the 16th Report of the Peabody Museum of American Archæology and Ethnology, Cambridge, 1883.

SECTION I.

ECONOMIC SCIENCE AND STATISTICS.

PAPERS READ.

STATISTICS IN AGRICULTURE. By J. R. DODGE, of Washington, D. C.

[ABSTRACT.]

THE offices and utilities of statistics in agriculture are indicated in this paper. Among these are : — the effort to measure the extent of national resources, contrast the actual with the possible in production, to weigh the effect of over-production in reducing prices, to show the error of reliance on uncertain markets, and mark the correlation of the industries, and the progress of the sciences in their application to rural arts. It shows how the emigrating farmer is guided by the light of statistics in seeking a home, choosing a location suited to his new circumstances, deciding whether to sell or store his crops at harvest, and in successive seasons whether to enlarge or to restrict the area of a given crop.

It states that orderly and scientific presentation of present and future accumulations of results of feeding experiments, that reveal the mysteries of animal physiology, will soon bear an important part in solving the problem of successful meat production. In view of the fact that the increase of value of all feeding material is greater than the advance in price of the product, thus reducing the margin of profit, this has become a vital question — how to produce a larger percentage of meat from a given measure of nutrients in feeding. Data from present and future experiment stations will be useful in this work, especially in showing the loss in flesh-making by exposure, by irregularity in time or quantity of feeding, by excess of succulent feed at one season followed by a ration of peculiar harshness and dryness during another period. Such statistics may harmonize the discrepant views of chemists and feeders upon the value of ensilage and the utility of the silo, and show the physiological necessity for variety, and possibly teach how meat may be made in winter as well as in summer, on hay and grain as well as grass, and eventually teach how two pounds of meat may be made where one was made before, and on a proportionally less quantity of nutriment.

Statistics have also been useful in reducing the overweening egotism of the cotton interest, which once assumed the regency of production and the dictatorship of exchange. Now the cereals have assumed the lead in exportation, and the products of the plebeian swine equal in value half of the exports of the patrician crop. A revolution is in progress, aided by statistics, showing the superior economy in cotton production through its subordination to diversification and rotation in a general system of cropping, by which the cost of culture is reduced, and the net profit of production increased. The progress of the south is shown in agriculture, in social and commercial economies, and in manufacture of cotton. The prediction is made of the increase of this industry to a practical equality with the north and Europe in consumption of the raw product.

The effect of the coördination of the facts of sheep-husbandry upon that interest is given, showing that the results of breeding in increasing weight and density of fleece and capacity for flesh-taking have been followed by extension of flocks and increase of price; and that, on the contrary, the exhibit of sudden reduction of wool had swept the pastures as with a besom of destruction — as in 1868, when four millions of peltless carcases were rendered for their tallow. The causes are slight that lead to the reduction of numbers and the abandonment of this industry — so sensitive is it to fluctuations of price — and the necessity for a uniform tariff policy is emphasized by these facts. The increase of wool-growing, during twenty years, is a striking fact — from 75,000,000 pounds to 300,000,000, and from a value of $20,000,000 to $80,000,000. Then the United States produced scarcely half that was manufactured, now fully four-fifths, the imported fifth being largely of low-priced carpet wools.

The use of statistics in determining whether agriculture is progressive or retrogressive is referred to, and the conclusion reached generally favorable to progress. Wheat culture is exhibited at some length, as illustrative of several aspects of this progress. The remarkable fact in its history is a doubling of the area in fifteen years, while in thirty years the increase in production has been nearly four hundred per cent, an increase, not only keeping pace with the increase of the population, but furnishing more than twice the supply *per capita*, viz.: 4.33 bushels in 1829, 5.5 in 1859, 7.28 in 1869, and 9.2 bushels in 1879. The facts of distribution are shown: only twelve states and two territories produced a surplus; the majority of the states failed to produce their own bread, and the causes of their failure in the several sections are duly shown. Illinois produced the largest quantity in the census year, followed in order by Indiana, Ohio, Michigan and Minnesota. The latter state produced *per capita*, 44 bushels to each inhabitant, Oregon 42, California 34, Nebraska 31. The movement of wheat-growing westward is thus shown as a percentage of the whole crop:—

	1849.	1859.	1869.	1879.
Atlantic Coast	51.4	30.7	20	12.8
Central Belt	43.3	54.6	49	49.9
Trans-Mississippi	5.3	14.7	31	37.3
	100	100	100	100

The production of wheat in five years has been enormous — an average of 425 million bushels per annum, of which (with twenty millions drawn last year from the average reserve in hands of farmers) 233 millions were consumed in the United States, 145 millions exported, and between fifty and fifty-one millions used as seed. The utility of statistics in fixing prices and failing speculation is offered, and it is intimated that in the last year of partial failure, a saving to farmers of twenty million dollars was made by the publication of national and state crop statistics.

NOTES UPON SOME METHODS AND RESULTS OF COLLECTING STATISTICAL MATTER BY MAIL. By CHAS. W. SMILEY, of Washington, D. C.

[ABSTRACT.]

STATISTICS are frequently laughed at, usually pushed aside as "dry" and sometimes analyzed to the serious discomfort of the author. At other times they are extolled and made the basis of the most important action. I am quite sure that these various treatments are usually just. Accurate and truthful statistics are very scarce and, when found, very valuable. Figures often do misrepresent terribly notwithstanding the charitable maxim that "figures never tell lies."

Whoever has prepared statistics or tested them knows that the great and fruitful cause of bad statistics is in bad methods of collecting the data. The methods are not revealed on the face of the results and hence the insidiousness of this cause. Is it not therefore pertinent at the very opening of the Section of Statistics and Economic Science to consider the subject of methods?

Mailing circulars does not constitute the most effective method, but if rightly managed it is cheapest. It is especially economical in the government bureaus where postage costs nothing. The most effective method is by personal visitation of skilled agents. This, from its expense, is usually out of reach of private institutions and often out of reach of government officers. The method of personal visitation I could extol very highly from my own experience and from close examination of results of that kind of work performed by others, but the present purpose is to treat solely of the collection of material by mail. I will describe, as illustrative of the best methods, an extensive correspondence carried on in the office of the U. S. Fish Commission.

In July, 1879, it was proposed by Gen. F. A. Walker, superintendent of the Tenth Census and Professor Spencer F. Baird, Commissioner of Fish and Fisheries, to ascertain something of the extent and nature of the fish trade and consumption of fish throughout the entire U. S. In consequence there was prepared a three-paged circular with blanks for answers and containing 18 brief but comprehensive questions, such as:—

1. Do fish constitute an important article of diet in your town and in the adjacent country?

2. Where is the supply obtained?

3. Check on the following list the kinds commonly to be seen in the markets: (list given)

4. What is the average retail price per pound?

5. What kinds of fish are taken from your ponds and streams? Check on the following list: (list given)

8. Are salted and smoked fish sold?

9. What kinds? Check on the following list: (list given)

11. Are oysters brought to your place? How are they brought; in shell, in tubs, in cans? What is the usual price?

16. Answer same questions for clams and lobsters.

18. Is fish guano in its various forms used by your farmers?

With this was sent to every Postmaster in the U. S., a circular letter asking the assistance of himself or some one familiar with the facts and accompanied by a circular from the Postmaster General instructing Postmasters to furnish such information as could be given "without prejudice to the duties of their offices." An addressed return envelope accompanied each circular.

The result of this sending in general terms was as follows:

41,517 Postmasters were addressed, from whom inside of 200 days 16,996 replies were received or 41 per cent. At the expiration of 200 days it was deemed best to send again, to those who had not answered, precisely the matter as had been sent before. This was done with a very gratifying result. 24,521 Postmasters were addressed from whom inside of 200 days 13,233 replies were received. In the 680 days that have since elapsed, but 155 more replies have arrived. This is much less than one per cent. The net result of two sendings was to get 73 per cent. of the reports desired.

Sending.	Circulars sent out.	Returns within 200 days.	Per cent of the sending returned.	Per cent of desired reports received.
I	41,517	16,996	41	41
II	24,521	13,233	54	32
Total	66,038	30,229		73

I should have preferred a third sending at the expiration of 200 days from the second. I estimate that a third call on the remaining 11,133 officers might have produced about 6,000 more replies and brought the per cent of results up to 86 per cent of what was desired.

I have taken 200 days as the limit of returns. The number that arrive after that is very insignificant. But it will be valuable to know what results to expect earlier. Taking the total of 16,996 replies which came in the first 200 days, the percentage which came in each of the ten 20-day periods was as follows:

Twenty-day periods.	Number of returns.	Per cent of all.	Days that had elapsed from the time of sending.	Per cent of what was destined to come which had arrived each twenty-day period.
First	9680	57	20	57
Second	4711	27	40	84
Third	1313	8	60	92
Fourth	604	3½	80	95½
Fifth	314	2	100	97½
Sixth	165	1	120	98½
Seventh	72	½	140	99
Eighth	65	½	160	99½
Ninth	42	¼	180	99¾
Tenth	30	¼	200	100
	16,996	100	200	100

The return within 200 days of 97½ per cent of all that would arrive within 200 days shows that, when subsequent calls are to be made, 100 days is a very good limit to fix at which to send again. The receipts under the second call were even more precipitate than the first, being augmented slightly by the driblets from the first call. It was as follows:

Twenty-day periods.	Number of returns.	Per cent of all.	Days that had elapsed from the time of sending.	Per cent of what was destined to come which had arrived each twenty-day period.
First	7862	59½	20	59½
Second	2796	21	40	80½
Third	998	7½	60	88
Fourth	790	6	80	94
Fifth	338	3	100	97
Sixth	238	2	120	99
Seventh	112	1	140	100
Eighth	33		160	
Ninth	11		180	
Tenth	3		200	

The case under consideration was a semi-official call upon all the states and territories and the effect of the call upon the different sections was very different. While the total yield of returns was 73 per cent of the offices addressed, 95 per cent of the Dakota offices answered, and but 61 per cent of the Louisiana offices answered. The per cent of answers for each state or territory was as follows:

Dak.	95	Utah	86	Me.	81	Mont.	80
Ind. T.	89	Mass.	85	Nev.	81	Fla.	79
Wash.	88	Vt.	83	N. H.	81	Del.	66
Wyo.	88	Oreg.	83	N. Y.	70	Tenn.	65
Idaho	87	Ohio	76	W. Va.	69	Ky.	65
Cal.	78	Colo.	75	Md.	69	Ga.	65
Iowa	78	N. Mex.	73	Ariz.	68	Miss.	64
Neb.	78	Mo.	72	Ark.	68	S. C.	63
Tex.	78	Ind.	71	Pa.	67	Ala.	63
N. J.	77	N. C.	70	Wis.	80	Va.	62
Ill.	77	Mich.	82	Conn.	80	La.	61
R. I.	87	Kans.	81	Minn.	80		

It was to be expected that states would answer according to their degree of education and intelligence and this is no doubt an element, but some other element has entered in here to place five western territories ahead of the best states. My own opinion is that the semi-official endorsement of the Postmaster General had far greater influence upon the new sections of the country than upon the older; that new officers answered partly because of the official endorsement and that old ones have become somewhat accustomed to disregard such endorsements. But why Dakota exceeded Arizona 27 per cent and the Indian Territory exceeded New Mexico 16 per cent I am unable to suggest except the possibility of the loss of mails in transit, by fire, wrecking, robbing of mail coaches, etc.

A grouping of the geographical sections yields some interesting results:

New England.		Great Plain Region.		Pacific Coast.		The North West.	
R. I.	87	Dak.	95	Wash.	88	Mich.	82
Mass.	85	Ind. Ter.	89	Oreg.	83	Kans.	81
Vt.	83	Wyo.	88	Nev.	81	Minn.	80
Me.	81	Idaho	87	Cal.	78	Wis.	80
N. H.	81	Utah	86			Iowa	78
Conn.	80	Mont.	80			Nebr.	78
		N. Mex.	73			Ill.	77
		Ariz.	68			Ohio	76
		Colo.	65			Ind.	71
Average	82	Average	82	Average	81	Average	78

Middle States.		South West.		South East.	
N. J.	77	Texas	78	Fla.	79
N. Y.	70	Mo.	72	N. C.	70
Pa.	67	Ark.	68	Md.	69
Del.	66	Ky.	65	W. Va.	69
		Tenn.	65	Ga.	65
		Miss.	64	S. C.	63
		Ala.	63	Va.	62
		La.	61		
Average	69	Average	68	Average	67

RECAPITULATION.

	Offices Addressed.	Ans. received in 680 days.	Per cent which answered.
New England	3176	2616	82
Great Plain Region	1381	1137	82
Pacific Coast	1528	1236	81
The Northwest	13157	10239	78
Middle States	7074	4905	69
The Southwest	8622	5865	68
The Southeast	6579	4386	67
Total	41517	30384	73

For quantity, these results were entirely satisfactory. The quality of the replies was of all grades. A very large per cent, however, bore internal evidence of truthfulness. A wise discretion was needed in the compilation but with skilled compilers excellent results could be produced. A discussion of methods of compilation, eliminating errors, etc., would be interesting but take too much time in this paper.

Let it not be supposed that these excellent results can be obtained only by government machinery. The official nature of this correspondence was a great aid, but I have obtained even better results from college men upon matters relating to their colleges and from specialists concerning their specialties. I therefore feel free to protest against the careless and inefficient work of this sort so often done by both public and private enterprise. Patience and perseverance in wise methods may not be all the qualifications that are necessary for good statistical work, but these are indispensable. Some of the suggestions to be made for success in collecting material are the following:

I. Make the questions very clear, concise and as few as possible. (Better send twice than totally break down by too long requests.)

II. If but one class of persons can be addressed from each of whom an answer is especially desired, send to all a first issue, to all who do not answer in 100 days a second issue, and to all who do not answer in 100 days more a third issue.

III. If more than one class of persons can give facts, address all of each class and after about 75 days address the delinquents a second time.

IV. Do not vary the matter sent the second time. Let it be an exact duplicate and be sent just exactly as if it had not been previously issued.

V. Leave blank lines between the questions so that no other paper is needed for reply, and if it can be put on a postal furnish it, even more for the sake of uniformity than for inducing people to reply.

VI. Always enclose an addressed envelope or postal for reply and provide that there be no expense to the respondent for postage.

VII. Remember that more or less of your circulars will be misdirected, lost in transit, fall into wrong hands, arrive during the absence or sickness of your correspondents, be crowded aside for later answer or to get some needed information and then inadvertently overlooked, and so, do not lose faith in human nature but rather rejoice that amid so many contingencies you can get the material at all.

Exhibition of some Statistics of College men. By Charles W. Smiley, of Washington, D. C.

[ABSTRACT.]

These statistics relate to the graduates and non-graduates of Wesleyan University, especially the latter. They cover fifty years ending with 1881. The elaborate methods of collecting the data were described. Copies of eight tables were distributed to the members present. These are to be published in full in the Alumni Record of the college. The following summaries may be given to illustrate their scope.

I. Table showing the Number in each Class, the Living, Dead, etc.

Classes.	Number of Non-graduates.	Number believed to be living.	Not certain whether living.	Numb'r dead.	Died before ent'r'ng life work.	Died since ent'r'ng life work.	No. of graduat's and non-graduat's	Per cent. who were not graduates*
1832–41	159	59	10	90	14	76	334	
1842–51	199	106	16	77	18	59	452	
1852–61	178	123	10	45	11	34	457	
1862–71	175	130	8	42	21	21	435	
1872–81	224	199	0	25	17	8	548	
Totals.	935	617	39	279	81	198	2226	
Per Ct.	100	66	4 1-6	29 5-6	8 4-6	21 1-6		42

*This ran as high as 58 per cent in 1834 and 1867, and exceeded 50 per cent in eleven years; it ran as low as 21 per cent in 1875, and as low as 28 per cent in 1859.

II. Table showing Time of Leaving this College, subsequent places of Study, Degrees, etc.

Classes.	No. in the Classes.	Year of leaving this College.				No. who on leaving entered		No. who have attained Degrees.						
		Senior.	Junior.	Sophomore.	Freshmen.	Other Col'ges.	Profes. Sch'ls.	A.B. or A.M.	M.D.	LL.B. or B. C. L.	LL.D. or D. C. L.	D. D. or S. T. D.	Ph. D.	Miscellaneous.
1832–41	159	26	33	52	48	32	12	38	12	2	5	4	0	0
1842–51	199	17	57	63	62	46	23	47	16	6	2	3	0	1
1852–61	178	8	42	60	68	60	28	56	21	6	2	2	0	1
1862–71	175	14	42	55	64	40	15	33	15	8	1	1	1	4
1872–81	224	19	38	70	97	35	18	20	7	10	0	0	1	6
Total	935	84	212	300	339	213	96	194	71	32	10	10	2	12
Pr. Ct	100	9	23	32	36	23	10	21	8	3	1	1		1

III. TABLE OF FAMILY STATISTICS.

CLASSES.	MARRIED.					Not married.	CHILDREN REPORTED.				
	Once.	Twice.	Thrice.	Total.	Unkn'n.		In families	Alive.	Dead.	Total.	Av'ge pr family.
1832–41	82	14	2	98	35	26	77	230	68	298	4
1842–51	111	15	0	126	39	34	98	312	78	390	3 4-5
1852–61	120	7	0	127	18	33	103	279	62	341	3 1-5
1862–71	108	6	0	114	13	48	93	192	35	227	2 1-5
1872–81	90	2	0	92	8	124	53	89	8	97	1 4-5
	511	44	2	557	113	235	422	1.102	251	1,353	3

IV. TABLE OF NATIVITIES.

Years.	Me.	N.H.	Vt.	Mass	R. I.	Ct.	N.Y.	N.J.	Pa.	South.	West.	Foreign.	Unknown.
1832–41	2	5	9	23	4	23	31	2	6	15	0	2	37
1842–51	5	10	4	24	2	24	50	4	5	10	0	9	53
1852–61	11	16	7	22	1	25	41	8	4	9	5	7	22
1862–71	15	9	9	27	1	24	24	9	5	6	7	9	30
1872–81	9	8	12	35	2	24	52	15	10	9	11	10	28
Totals.	42	48	41	131	10	119	198	38	30	49	23	37	169

V. PRESENT OCCUPATION.

In Ministry, Methodist Episcopal	85 (.09)[1]	
Protestant Episcopal	13 (.01)	
Congregational	6 } (.01)	
Baptist	4 }	
Other sects	9 (.01)	
		117 (.12)
Lawyers		98 (11.)
Teachers		66 (.07)
Physicians		47 (0.5)
Dentists		2
Editors, authors and purely literary work		20 (0.2)
Manufacturers		34 (.01)
General business pursuits		124 (.13)
Clerks		25 (.03)
Druggists		13 (.01)
Official positions, State or Federal		6 (.01)
Farmers		49 (.05)
Students		7 (.01)
Artists		4 } (.01)
Army or Navy		3 }
Out of employ		12 (.01)
Unknown		29 (.03)
Total number living non-graduates		656 (.70)
Number of deceased non-graduates		279 (.30)
TOTAL		935 (1.00)

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[1] The small figures in parenthesis denote per cents; thus, 9 per cent of all non-graduates are in the Methodist ministry now, while 30 per cent are dead.

VI. OCCUPATION SINCE LEAVING COLLEGE.

Occupation.	Number engaged.	Years spent.	Average years in each occupation.
Bishops	2	33	17
Ministry, Methodist Episcopal	136	1706	13
Protestant Episcopal	20	369	18
Congregational	11	107	10
Baptist	6	74	12
Presbyterian	4	75	19
Other sects	9	69	8
College Presidents	7	46	7
College Professors	17	157	9
Seminary Instructors and Tutors	10	70	7
Teaching professionally	94	1157	12
Teaching temporarily	100	264	3
Judges	7	68	10
Lawyers	144	2312	16
Physicians	82	1176	14
Dentists	2	46	23
Journalists	47	426	9
Authors	3	32	11
Manufacturers	37	611	17
General business	194	2365	12
Scientific work	2	46	23
State Legislatures	33	62	2
Members of Congress	4	14	4
Customs, Revenue, and Postal offices	11	83	8
Other public offices	19	47	2
Farming	66	868	13
Superannuated	8	53	7
Regular Army and Navy	11	70	6
Civil Engineers	10	127	13
Artists	4	32	8
Gentlemen of leisure	2	62	31
Mexican War	2	4	2
Federal Army,[1] 1861–5	92	220	2
Confederate Army, 1861–5	6	16	3

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[1] Died during, or from injuries received in, the war, 22.

VII. COLLEGES ENTERED, BY 213 NON-GRADUATES OF WESLEYAN UNIVERSITY, AFTER LEAVING THIS COLLEGE.

The Colleges entered, and the number who went to each, were as follows:

Union	45
Yale	23
Amherst	14
Dartmouth	14
University of the City of New York	13
Brown	12
Dickinson	9
Boston	7
Harvard and Trinity, each 6	12
Syracuse, West Point, and University of Michigan each 5	15
Middlebury	4
Bowdoin and Princeton each 3	6
Augusta, Genesee, Hamilton, Hobart, Indiana, Asbury, Lafayette, Ohio Wesleyan, Madison, St. John's College, University of Pennsylvania, University of Virginia, and Williams, each 2	24
Alexandria, Baldwin, Columbia, Cornell, Emory, Franklin and Marshall, Illinois Wesleyan, Iowa Wesleyan, Kings, Northwestern, Norwich, Rutgers, University of Vermont, U. S. Naval Academy, and Willoughby, each 1	15
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SOME FACTS RELATIVE TO THE U. S. CENSUS OF 1880. By CALVIN S. MIXTER, of Washington, D. C.

[ABSTRACT.]

THE following table shows the population of the United States by age and sex, classified as Native White, Foreign White and Colored,[1] according to the United States Census of 1880.

AGES.	ALL CLASSES.			NATIVE WHITE.		FOREIGN WHITE.		COLORED.	
	Total.	Males.	Females	Males.	Females	Males.	Females	Males.	Females
All ages.	50,155,783	25,518,820	24,036,963	18,609,265	18,234,026	3,521,635	3,038,044	3,387,920	3,364,893
Under 1 year.	1,447,963	734,024	713,959	617,307	595,430	2,989	3,061	113,728	115,468
1 year.	1,256,956	638,032	618,924	534,030	515,491	5,020	5,066	98,982	98,367
2 yrs.	1,427,086	726,038	701,048	601.683	579,272	6,893	6,752	117,462	115,024
3 yrs.	1,381,274	697,209	684,065	577,733	563,881	7,797	7,801	111,679	112,383
4 yrs.	1,401,217	712,406	688,811	587,440	565,513	8,557	8,435	116,409	114,863
Under 5 yrs.	6,914,516	3,507,709	3,406,807	2,918.193	2,819,587	31,256	31,115	558.260	556,105
5 to 9 years	6,479,660	3,275,131	3,204,529	2,694,398	2,625,324	61,803	60,894	518,930	518,311
10 to 14 years	5,715,186	2,907,481	2,807,705	2,361,832	2,280,260	120,740	117,699	424,909	409,746
15 to 19 years	5,011,415	2,476,088	2,535,327	1,965,748	2,007,090	184,320	194,492	326,020	333,745
20 to 24 years	5,087,772	2,554.684	2,533.088	1,945,279	1,928,938	274,038	254,217	335,367	349,933
25 to 29 years	4,080,621	2,109,741	1,970,880	1,472,960	1,397,625	365,094	306,022	271,687	267,233
30 to 34 years	3.368,943	1,744,308	1,624,635	1,128,308	1,088,315	419,769	342,862	196,231	193,458
35 to 39 years	3,000,419	1,527,159	1,473,260	920,264	938,963	432,957	356,308	173,938	177,989
40 to 44 years	2,468,811	1.243,773	1,225,038	726,832	751,013	384,931	327,959	132,010	146,066
45 to 49 years	2,089,445	1,078,695	1,010,750	621,164	609,787	340.863	290,078	116,668	110,885
50 to 54 years	1,839,883	936,702	873,181	538,133	512,672	318,045	259,042	110,524	101,467
55 to 59 years	1,271.434	674,927	596,507	403,260	378,541	203,820	166,294	64,847	51,672
60 to 64 years	1,104,219	584,858	519,361	345,575	321,270	170,841	139,622	68.442	58,469
65 to 69 years	725,876	379,498	346,378	243.803	232,245	98,028	83,333	37,667	30,800
70 to 74 years	495,442	250,001	245,441	165,062	164,476	59,590	55,019	25,349	25.946
75 to 79 years	281,065	138,601	142,464	94,598	99,574	31,106	29,590	12,897	13,300
80 to 84 years	146,362	67,941	78,421	44,492	51,949	15,156	15,930	8,293	10,542
85 to 89 years	49,835	21,908	27.927	14,858	19,425	4,419	5,096	2,631	3,406
90 to 94 years	16,100	6.351	9,749	3.486	5,357	1,270	1,690	1,595	2,702
95 to 99 years	4,763	1,855	2,908	783	1,260	433	575	639	1,073
100 and over	4,016	1,409	2,607	237	355	156	207	1.016	2,045

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[1] The colored population includes 105,465 Chinese, 66,407 Indians (not in tribal relations), and 148 Japanese, whose ages have not yet been separately compiled.

TITLES OF OTHER PAPERS READ IN SECTION I.

ON INTERNATIONAL STANDARD TIME. By E. B. Elliott, of Washington, D. C.

SUGGESTIONS OF ELECTRICAL UNITS. By E. B. Elliott, of Washington, D. C.

POPULATION STATISTICS OF THE CHEROKEES. By E. B. Elliott, of Washington, D. C.

ON CERTAIN GOVERNMENT SECURITIES. By E. B. Elliott, of Washington, D. C.

UPON THE INVESTMENT OF LABOR AND CAPITAL IN FOREST CULTURE AS COMPARED WITH OTHER PRODUCTIVE INDUSTRIES. By Franklin B. Hough, of Lowville, N. Y.

THE JESSUP COLLECTION TO ILLUSTRATE AMERICAN FORESTRY, IN THE MUSEUM OF NATURAL HISTORY, CENTRAL PARK, NEW YORK. By Albert S. Bickmore, of New York, N. Y.

ON METHODS OF OBTAINING REGISTRATION OF VITAL AND OTHER STATISTICS OF THE OMAHA AND COGNATE TRIBES OF INDIANS. By J. Owen Dorsey, of Washington, D. C.

REFERENCES TO SOME OF THE ECONOMIC AND SCIENTIFIC PRINCIPLES IN TREE-GROWTH AND TREE-DESTRUCTION. By James Hyatt, of Stanfordville, N. Y.

STANDARD TIME FOR NORTH AMERICA. By Winslow Upton, of Washington, D. C.

EXPERIMENTAL PLANTATION OF EUCALYPTUS NEAR ROME. By Franklin B. Hough, of Lowville, N. Y.

COÖPERATION OF OBSERVATIONS FOR MAINTAINING ACCURATE TIME. By J. Rayner Edmands, of Cambridge, Mass.

EXECUTIVE PROCEEDINGS.

REPORT OF THE GENERAL SECRETARY.

WEDNESDAY, AUGUST 23, 1882.

THE thirty-first annual meeting of the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE was called to order in the William Molson Hall of McGill University, Montreal, shortly after ten o'clock, by the retiring President, Prof. GEORGE J. BRUSH of New Haven, who introduced to the members the President for the coming year, Dr. J. W. DAWSON of Montreal.

President DAWSON then took the chair and spoke as follows:—

Ladies and Gentlemen of the Association:

I need hardly say how highly I esteem the honor you have done me in my election as your President for the present meeting. The unanimous election of a body like this to such an office is, in my judgment, the greatest distinction to which any scientific man on this continent can aspire, and I value it accordingly.

I have no doubt, however, that in the present case, the choice was in some degree determined by the wish to do honor to Canada, and to give to this meeting a character as thoroughly international as possible.

But this view of the reasons for the election by no means detracts from its value. On the contrary, it places the presidency of this association on a broader basis than that of any other office by extending over all this wide continent and including all that belongs to the powers that reign both at Washington and Ottawa. Science from her serene height thus overlooks all national boundaries and comprehends this whole world in her scope of vision.

It becomes your duty, therefore, for the time being, to merge the character of citizens of the United States or of Canada in that of cosmopolitan men of science. This is what I propose to attempt in endeavoring to perform the duties to which you have called me; and you will, therefore, kindly regard me not as a Canadian, or as an American, in the narrower sense of that term, but as the President of a society which, in meeting here, assumes a continental and international character.

THE PRESIDENT then requested the Anglican BISHOP OF MONTREAL to invoke the divine blessing on the Association's meetings, after which ceremony the PRESIDENT called on Dr. T. STERRY HUNT, Chairman of the Local Committee, who spoke as follows:—

In welcoming in the name of the Local Committee the American Association for the Advancement of Science to our city on the occasion of this, its thirty-first meeting, a crowd of memories rise to mind, many of which will be shared alike by citizens of Montreal and by my colleagues of the Association. Twenty-five years have passed since this Association met here in August, 1857, under the presidency of Prof. James Hall, who honors us with his presence again to-day. Of the Local Committee of that time only four are now with us; Dr. Dawson, the President-elect of the Association, Dr. Hingston and Mr. Charles Garth, both members of the Executive Committee for this meeting, and myself. Those who were with us on that occasion will note great changes and great progress alike in the Association and in ourselves. Montreal was then a city of 60,000, and has to-day a population of 150,000 souls. The infant university, of which Dr. Dawson had just then assumed the direction, has under his wise care attained a vigorous and flourishing manhood, and the collections in natural science, gathered with few exceptions during his administration, now suffice to fill the great museum just completed and presented to the University by the generosity of a citizen of Montreal.

Not less remarkable is the growth of our Association, which has in the meantime increased more than fourfold, and now numbers nearly 2000 members. The history of this and its related associations demands a word in passing. Unlike national academies or royal societies these are popular bodies, inviting to their membership all who have an interest in the Advancement of Science, holding their annual meetings in different cities of their respective countries and so performing a kind of missionary work. The parent of all these is the German Association founded more than half a century since by the venerable Oken. Next in order of time comes the British Association, which held its fiftieth anniversary at York last year under the presidency of Sir John Lubbock, and to-day meets again at Southampton, with Dr. C. W. Siemens as president. Two of its honored past-presidents, W. B. Carpenter and Samuel Haughton, are with us, as our guests, to-day. Next in order of time comes our own Association, of which more anon; and last, but not least, the young French Association for the Advancement of Science, which was born with the new French republic, and met last year in Algeria, bearing back across the Mediterranean the torch of science to the soil of northern Africa, which ages before had been the cradle of arts and sciences. It was in May, 1881, that the French nation carried the war of science into Africa, well supported by allies from other lands, among whom was our distinguished Hungarian guest of to-day Dr. Szabó, of Budapest.

But to return to our own organization: the American Association, though it reckons its meetings only from the year 1849, has an earlier history. In 1842 there was formed a little band of workers under the name of the American Association of Geologists and Naturalists. To this body I, at that time a student in the University, was admitted on the occasion of its meeting at New Haven in May, 1845. The scope of this association was, however, not large enough. The claims of physics, of chemistry and of mathematics, all indispensable allies of geology and of natural history, soon made themselves heard in our meetings, and the success which had attended the German and British Associations already mentioned led to its reconstruction and enlargement with its present organization and title. This was effected at Philadelphia in 1848, and with the subsequent history of the Association you are more familiar. Thirty-four years have thus passed, and we are to-day holding our thirty-first meeting. In its earlier days, the Association occasionally tried the experiment of two meetings annually, but the plan of a spring gathering was soon abandoned. Afterwards, during the late civil war, came an interruption of five years, during which the deliberations of our Association were silenced by the clash of arms, until its meeting in Buffalo in 1866; since which time its gatherings have been held annually.

From an early period in its history it was felt that this Association should be in its widest sense North-American. Science knows no political boundaries, and the policy adopted by our organization was continental. This was due in no small degree to the importance of the work of the Geological Survey of Canada, which, begun in 1842, had early drawn the attention of naturalists to this country. To this field of service I had come from my native Connecticut as early as 1847, and Professor James Hall, though persistently rejecting the repeated overtures which were made to him to join the Canadian survey, had at intervals given it the benefit of his great knowledge of American geology and paleontology. Hence it came about that in response to an urgent invitation from the people of this city the American Association for the Advancement of Science first met in Montreal in August, 1857, Prof. James Hall being president. The late Sir William Logan, at that time my chief, and the director of the Geological Survey, was then chairman of the Local Committee, while I was secretary of the Special Committee on Invitations.

And now twenty-five years have come and gone, and true to its continental character the American Association once more comes to Montreal to hold its annual meeting, having elected for its presiding officer the honored president of our University, and for its general secretary a representative naturalist from the province of Ontario. It has returned to find Montreal grown to a great metropolis, busy with manufactures and commerce, its wharves lined with ocean-steamers and ships of all nations; while it boasts at the same time that the growth of letters and science has kept pace with its material progress, and invites the members of the Association to visit its churches and asylums, its schools and its museums. It can only tell you with feelings of disappointment that the Geological Survey, with its valuable library and its extensive museum, has now left Montreal, having been last year removed to Ottawa, the seat of the federal government; while we at the same time regret that the duties of its present director, Dr. Selwyn, have, by calling him to the provinces of the far northwest, prevented him from meeting with us on this occasion.

The plan and scope of our Association, like that of its European sisters, is eminently popular, since it invites to its membership all those who by their attendance and their contributions are willing to show their interest in our objects. It is at the same time eminently conservative in its organization. Within the wide circle of its members, or associates as they might be called, is a select body of fellows, to whom alone, by the constitution, is intrusted the government of the body. These are under the present revised constitution carefully selected from the members for their scientific attainments, and are alone eligible for office in the Association. Moreover, those who have had the honor of filling the chair of president of the Association are perpetual members of its governing body, the Standing Committee.

So much in my capacity of an original member, fellow and past-president of the Association, I have ventured to recall for the information alike of citizens and the younger members of the Association. To all its members who have now honored us with their presence I would say that the Local Committee, not unmindful of the importance of the American Association, and of the distinction conferred on this city in choosing it as a place of meeting, has spared no pains or care to do honor to its guests, and now bids you a hearty welcome.

The MAYOR OF MONTREAL was next introduced and welcomed the members of the Association to the city in the following words:—

Mr. President, Ladies and Gentlemen:

I feel highly honored at having the very pleasant duty to perform of welcoming those gentlemen from the different parts of America — from the Dominion of Canada and the United States — who have met here to discuss questions which interest the whole of humanity — a welcome, gentlemen, which the citizens of Montreal and the City Council have heartily voted to the different Associations of scientific men — the Forestry Congress, as well as this American Association for the Advancement of Science — who have met here and are now daily meeting to discuss questions of the utmost importance. I have every hope and confidence that your deliberations will result in great improvements in the different branches of Science. Mr. President, on behalf of the citizens of Montreal, I welcome all those who are attending the American Association for the Advancement of Science, the American Forestry Congress and the Society for the Promotion of Agricultural Science, and I am authorized to offer you the freedom of the city.

President DAWSON then read a letter from His Excellency THE GOVERNOR GENERAL, regretting his absence, and also announced that His Honor, Lieutenant-Governor ROBITAILLE had expected to be present to address a few words of welcome to the members, but had been unexpectedly called to a distant part of the Province.

Dr. THORBURN, of Ottawa, who was next called on, said that at a public meeting recently held in that city, steps had been taken to give a hearty welcome to those members of the Association who might visit the capital. Ottawa had many attractions and he felt sure that the members would be pleased with their visit and trusted that as many as could would favor his fellow-citizens with their presence on Saturday next.

The PRESIDENT then said:—It falls to me, gentlemen, to acknowledge on behalf of the Association, the welcome which you have given it. In doing so, I have the advantage of knowing from personal observation the zeal and energy which have been displayed by the members of the Local Committee, and the enthusiasm kindled by the occasion in the minds of the citizens generally. The occasion is undoubtedly one worthy of the interest it has excited, not only in this and neighboring cities but in Canada generally. By its selection for the meeting of this Association, Montreal becomes for the time the scientific capital of North America, from which will go forth decrees more potent than those of parliaments, and to which will be turned the eyes of all interested in the progress of Science and the Scientific Arts. You do well to esteem highly the position thus conferred and to sustain it in the future by the actions of your own local institutions.

The fact that we can congratulate ourselves on this occasion in the presence of so many and so eminent men of science from the other side of the Atlantic is largely due to the exertions of the Local Committee and to the liberality of the citizens of Montreal. I had the honor in 1856 to be the spokesman of a delegation which went to Albany to invite this Association to hold the meeting in Montreal in 1857, which has been referred to. That meeting was a most successful one, though, no doubt, far inferior in numbers to the present. It benefited this Association and the interests of science, and gave a stimulus to the cause of scientific education and research in Montreal, the effects of which still remain. May the present meeting be still more agreeable and useful. Of the leading men of science who took part in the meeting of 1857 many have passed away: Henry, Logan, Bache, Pierce, Munroe, Billings and Morgan are among these. But many of the men who then stood high in science are with us to-day, and you now welcome a large number of men who have since that time risen into eminence, and of still younger men whose names will yet be widely known. To such young and rising men you should, and I know do, extend a welcome as hearty as to those who are their seniors and are better known to you. You welcome here to-day a much larger body than that which you received in 1857. This is evidenced not only by the larger number of members, but by the increased number of sections. In 1857 there were two sections and a sub-section. Now there will be no less than nine sections meeting simultaneously. I have only to add, on behalf of the Association, that the ordinary meetings of the sections, and those evening-meeting addresses which will be advertised, are open to all who may desire to benefit by them; and that the membership of the Association is open to all suitable applicants on very easy terms. Such entertainments and excursions as have been provided by the Local Committee or by local institutions and private individuals, are of course limited by the invitations these may issue, but I have no doubt they will be made as general as the circumstances will permit.

The PERMANENT SECRETARY announced that the financial report of the year had been presented to the Standing Committee and would appear in the volume of Proceedings. Several subscriptions had come in for reprinting volumes of the Proceedings of the Association, among others one from Gen. William Lilly of $1,000 for reprinting volume twenty-six, and since he had come to Montreal he had received another contribution of $850 for a similar purpose from a member, who for the present wished to remain unknown. He also stated that one hundred and sixty papers had already been entered for this meeting and referred to the various sections.

Dr. T. STERRY HUNT, on behalf of the ART ASSOCIATION, invited the members to visit the Art exhibition.

The PERMANENT SECRETARY then stated that notices of the decease of the following members of the Association had been received since the last meeting:

THOMAS ALLEN, of St. Louis, Mo. Joined the Association at the 27th meeting. Died April 8, 1882.

ZACHARIAH ALLEN, of Providence, R. I. An original member. Died March 17, 1882.

JOHN G. BARNARD, of New York, N. Y. Joined at the 14th meeting. Died May 14, 1882.

GEORGE S. BLACKIE, of Nashville, Tenn. Joined at the 26th meeting.

ALBERT D. BRIGGS, of Springfield, Mass. Joined at the 13th meeting. Died Feb. 20, 1881.

LEVI S. BURBANK, of Woburn, Mass. Joined at the 18th meeting. Died Aug. 20, 1880, in his 52d year.

Mrs. MARY H. CAMPBELL, of Crawfordsville, Ind. Joined at the 22nd meeting. Died Feb. 27, 1882.

FREDERICK COLLINS, of Washington, D. C. Joined at the 28th meeting.

J. M. CRAMP, of Wolfville, N. S. Joined at the 11th meeting. Born July 25, 1796. Died Dec. 6, 1881.

CHARLES F. CROCKER, of Lawrence, Mass. Joined at the 22nd meeting. Died in July, 1881.

E. A. DALRYMPLE, of Baltimore, Md. Joined at the 11th meeting. Died Oct. 30, 1881.

CALEB G. FORSHEY, of New Orleans, La. Joined at the 21st meeting. Died in Aug., 1881.

W. W. GLAZIER, of Key West, Fla. Joined at the 29th meeting. Died Dec. 11, 1880.

J. GOLDMARK, of New York, N. Y. Joined at the 29th meeting. Died in April, 1882.

GEORGE W. HAWES, of Washington, D. C. Joined at the 23d meeting. Born Dec. 31, 1848. Died June 22, 1882.

THOMAS POTTS JAMES, of Cambridge, Mass. Joined at the 22nd meeting. Born Sept. 1, 1803. Died Feb. 22, 1882.

LEWIS H. MORGAN, of Rochester, N. Y. Joined at the 10th meeting. Born Nov. 21, 1818. Died Dec. 17, 1881.

WILLIAM H. MUSSEY, of Cincinnati, Ohio. Joined at the 30th meeting. Born Sept. 13, 1818. Died Aug. 1, 1882.

Charles H. Payn, of Saratoga Springs, N. Y. Joined at the 28th meeting. Died in 1881.

J. Duncan-Putnam, of Davenport, Iowa. Joined at the 27th meeting. Born Oct. 18, 1855. Died Dec. 10, 1881.

William Barton Rogers, of Boston, Mass. An original member. Born Dec. 7, 1804. Died May 30, 1882.

Elihu Root, of Amherst, Mass. Joined at the 25th meeting.

William Sheppard, of Drummondville, Quebec. Joined at the 11th meeting. Born in 1783. Died in 1867.

David P. Smith, of Springfield, Mass. Joined at the 29th meeting. Died Dec. 26, 1880.

Charles Spinzig, of St. Louis, Mo. Joined at the 27th meeting. Died Jan. 22, 1882.

A. Remsen Thomson, of New York, N. Y. An original member. Died in Oct., 1879.

William S. Vaux, of Philadelphia, Pa. An original member. Born May 19, 1811. Died May 5, 1882.

Gouverneur Kemble Warren, of Newport, R. I. Joined at the 12th meeting. Died Aug. 8, 1882, in his 64th year.

J. Craig Watson, of Ann Arbor, Mich. Joined at the 13th meeting. Born Jan. 28, 1838. Died Nov. 23, 1880.

Mrs. G. O. Welch, of Lynn, Mass. Joined at the 21st meeting. Died in June, 1882.

Charles M. Wheatley, of Phœnixville, Pa. An original member. Died May 6, 1882.

Lewis E. Whiting, of Saratoga Springs, N. Y. Joined at the 28th meeting. Born March 7, 1815. Died Aug. 2, 1882.

This list includes the names of five original members, two past-presidents, and the treasurer of the Association.

The **General Secretary** announced that the Standing Committee recommended for membership in the Association the members of the Local Committee in Montreal and one hundred and sixty-nine other persons whose names were read to the meeting. On motion, the **General Secretary** was instructed to cast an affirmative ballot for the election of the parties named, who were then declared duly elected members of the Association.

The **President** announced that the following named gentlemen from abroad, who were present as invited guests of the Association, had been nominated for membership by the Standing Committee, and they were duly elected by the Association

Dr. **Joseph Szabó**, Royal Counsellor, Professor of Geology and Mineralogy, Budapest, Hungary.

Charles Weld, Professor of Histology, University of Vienna.

Dr. **Woldemar Kowalevsky**, Professor of Geology, University of Moscow.

Dr. **Rudolph Kœnig**, of Paris.

Rev. **Samuel Haughton**, Trinity College, Dublin.
George Francis Fitz Gerald, Trinity College, Dublin.
W. S. **McCay**, Fellow of Trinity College, Dublin.
Alfred Hamilton Ormsby, Barrister-at-Law, Dublin.
Rev. Prof. **Wiltshire**, King's College, London.
Dr. **John Rae**, F. R. S., LL. D., Kensington, London.
William B. Carpenter, C. B., M. D., London.
Dr. J. W. **Gilbert**, Rothamsted, England.
John Cox, Warden of Cavendish College, Cambridge.
Ernest H. Cook, Bristol University School, Bristol, Eng.
J. S. **Phené**, LL. D., F. S. A., F. G. S., London.

The **President** also announced that the annual meeting of the British Association opened on the same day with this meeting.

The recommendation of the Standing Committee that the hours of meeting each day, except Saturday, should be from 10 A. M. to 1 P. M., and from 2.30 to 5 P. M., was adopted.

The General Session was then declared adjourned for the meetings of the Sections in their respective rooms for organization.

Section A. *Mathematics and Astronomy.*

In the absence of the Vice President, **Wm. Harkness** of Washington, the Section was called to order by the Secretary, H. T. **Eddy** of Cincinnati, and W. A. **Rogers** of Cambridge was elected temporary Chairman.

W. W. **Johnson** of Annapolis was elected to the Standing Committee, W. W. **Payne** of Northfield to serve on the Nominating Committee, and the following Sectional Committee was elected: G. W. **Hill** of Washington, **Leonard Waldo** of New Haven, C. A. **Young** of Princeton.

The Subcommittee, to act with the Vice President and Secretary in recommending to the Nominating Committee the Vice President and Secretary of the next meeting, was elected as follows: F. H. **Sherman**, Nashville, R. W. **Wilson**, Cambridge, C. H. **Rockwell**, Tarrytown.

The Section then adjourned until 2.30 P. M.

In the afternoon, in the absence of the Vice President, his address was read by J. R. **Eastman** of the U. S. Naval Observatory.

Section B. *Physics.*

Section called to order by Vice President T. C. **Mendenhall** of Columbus, **Chas.** S. **Hastings** of Baltimore, Secretary.

Fellow elected to Standing Committee: E. W. **Blake**, Providence.

Sectional Committee: C. K. **Wead**, Ann Arbor, A. E. **Dolbear**, College Hill, H. A. **Rowland**, Baltimore.

To Nominating Committee: T. F. **Jewell**, Newport.

Sectional Subcommittee: N. D. C. **Hodges**, Salem, J. B. **Murdock**, Annapolis, S. H. **Brackett**, St. Johnsbury.

Section adjourned to meet at 4 P. M., when Vice President **Mendenhall** delivered his address.

Section C. *Chemistry.*

Section called to order by Vice President H. C. Bolton of Hartford, lfred Springer of Cincinnati, Secretary.

Fellow elected to Standing Committee: S. A. Lattimore, Rochester.

Sectional Committee: G. C. Caldwell, Ithaca, E. W. Morley, Hud- on, R. C. Kedzie, Lansing.

To Nominating Committee: G. C. Caldwell, Ithaca.

Sectional Subcommittee: A. A. Julien, New York, P. Schweitzer, olumbia, A. R. Leeds, Hoboken.

Section adjourned to meet at 2.30 p. m., when Vice President Bolton elivered his address.

Section D. *Mechanical Science.*

Section called to order by Vice President W. P. Trowbridge of New aven. In the absence of the Secretary, C. B. Dudley, J. Burkitt Webb f Ithaca was elected to fill the vacancy. The other elections were de- rred until the following day.

Section adjourned to meet at 4 p. m., when Vice President Trowbridge elivered his address.

Section E. *Geology and Geography.*

This Section was called to order by Vice President E. T. Cox of San rancisco. In the absence of the Secretary, C. E. Dutton, the Section lected H. S. Williams of Ithaca to act in his place.

Fellow elected to Standing Committee: G. H. Cook, New Brunswick.

Sectional Committee: C. H. Hitchcock, Hanover, N. H. Winchell, inneapolis, E. T. Nelson, Delaware.

To Nominating Committee: J. W. Spencer, Paxton.

Sectional Subcommittee: P. W. Sheafer, Pottsville, Alex. Winchell, nn Arbor, William Lilly, Mauch Chunk.

Section adjourned to meet at 4 p. m., when Vice President Cox ad- ressed the Section, and an interesting discussion followed.

Section F. *Biology.*

Section called to order by Vice President Dall of Washington. In the bsence of the Secretary, C. S. Minot, William Osler of Montreal was lected to fill the vacancy.

Fellow elected to Standing Committee: A. J. Cook, Lansing.

Sectional Committee: J. A. Lintner, Albany, W. J. Beal, Lansing, H. . Bassett, Waterbury.

To Nominating Committee: L. F. Ward, Washington.

Sectional Subcommittee: Thos. Meehan, Germantown, B. D. Halsted, ew York, T. J. Burrill, Champaign.

Section adjourned to meet at 4 p. m., when Vice President Dall deliv- red his address.

Section G. *Histology and Microscopy.*

Section called to order by Vice President A. H. Tuttle of Columbus, obert Brown, jr., of New Haven, Secretary.

Fellow elected to Standing Committee: R. H. **Ward**, Troy.

Sectional Committee: A. B. **Hervey**, Taunton, **Louis Elsberg**, New York, R. **Hitchcock**, New York.

To Nominating Committee: J. D. **Hyatt**, Morrisania.

Sectional Subcommittee: G. O. **Mitchell**, Hanover, T. J. **Burrill**, Champaign, C. C. **Merriman**, Rochester.

Section adjourned to meet at 2.30 P. M., when Vice President **Tuttle** delivered his address.

SECTION H. *Anthropology.*

In the absence of the Vice President, **Daniel Wilson** of Toronto, the Secretary, **Otis T. Mason** of Washington, called the Section to order.

Fellow elected to Standing Committee: P. R. **Hoy**, Racine.

Sectional Committee: A. S. **Bickmore**, New York, **Garrick Mallery**, Washington, N. S. **Townshend**, Columbus.

To Nominating Committee: **Horatio Hale**, Clinton.

Sectional Subcommittee: J. O. **Dorsey**, Washington, **Geo. H. Perkins**, Burlington, **Joseph Anderson**, Waterbury.

· Section adjourned to meet at 4 P. M., when the address of Vice President **Wilson** was read by the Secretary, **Otis T. Mason**.

SECTION I. *Economic Science and Statistics.*

Section called to order by Vice President E. B. **Elliot** of Washington, F. B. **Hough** of Lowville, Secretary.

Fellow elected to Standing Committee: **Joseph Cummings**, Evanston.

Sectional Committee: J. R. **Dodge**, Washington, **Chas. W. Smiley**, Washington, E. B. **Whitman**, Cambridge.

To Nominating Committee: **Eli E. Tappan**, Gambier.

Section adjourned to 2.30 P. M., when Vice President **Elliott** addressed the Section.

In the evening a General Session of the Association was held in the Queen's Hall, for the purpose of hearing the address of the retiring President of the Association, Prof. **George J. Brush** of Yale College, New Haven. President **Dawson** introduced Professor **Brush**, who then delivered his address, after which the members were invited to repair to the assembly rooms adjoining, where a kind reception awaited them by the Local Committee represented by Dr. T. **Sterry Hunt**. Music was provided, and the rooms were handsomely decorated with flowering-plants, palms and ferns, while at one end of the main hall refreshment tables were placed, and several hours were passed in pleasant social intercourse.

THURSDAY, AUGUST 24.

The Association met in General Session at 10 A. M., in the William Molson Hall, President **Dawson** in the chair.

The **Permanent Secretary** announced that an invitation had been extended to the members of the Association, by the directors of the **Montreal Amateur Athletic Association** to visit their rooms and library.

he GENERAL SECRETARY read the list of names of sixty-one persons ommended for membership by the Standing Committee, making two dred and thirty in all. On motion, the Secretary was directed to cast ffirmative ballot, and they were declared elected.

he Association then adjourned to meet in Sections. [The papers read ore the several Sections are given in other parts of this volume.]

Section A, a magnificent series of astronomical plates, from draw- s by Mr. E. L. TROUVELOT, was placed on exhibition.

Section C, the Chairman announced that, in accordance with the lutions passed at the previous day's session, he had appointed a com- ee consisting of F. W. CLARKE, ARTHUR H. ELLIOTT and H. B. NASON, iscuss the feasibility of framing a list of chemical indices. He said r duty would be the discussion of one of the most important obstacles in the way of chemistry. There being no uniform system of chemi- indices, each chemist adopted a system of his own in recording the lts of his investigations. When an eminent chemist chanced to die, results of his life's experience and researches would follow him into grave, and humanity would be deprived, perhaps, of the inestimable k of a life devoted to this science.

Section D, DE VOLSON WOOD, of Hoboken, was elected to the nding Committee, and G. LANZA of Boston, to the Nominating Com- ee.

Section G, an invitation was read and accepted from the MONTREAL ROSCOPICAL CLUB for Monday evening, from 8 to 10 P. M., for the tical demonstration of various modes of illumination.

the evening the members were invited to the formal opening of the path Museum, when the fine new building was presented by Mr. ER REDPATH to the McGill University. After the ceremonies of pre- ation were over, Principal and Mrs. DAWSON held a reception in the eum halls, to which the members of the Association and many citizens been invited. During the evening Mr. Hovey's paper on "Cave nery" was given in the theatre, illustrated by lantern pictures.

FRIDAY, AUGUST 25.

he Association met in General Session at 10 A. M., President DAWSON he chair.

he GENERAL SECRETARY read the names of fifty-two persons recom- ded by the STANDING COMMITTEE for membership in the Association, of whom were duly elected. He also announced that the STANDING MITTEE had nominated Prof. W. A. ROGERS of Cambridge as Vice sident for Section A, and Prof. ALEX. WINCHELL of Ann Arbor for tion H, and on motion the nominations were confirmed.

he STANDING COMMITTEE recommended that a committee be appointed onfer with committees of foreign associations for the advancement of science, with reference to an international convention of scientific associations, the said committee to consist of T. **Sterry Hunt**, Montreal, **Alex. Agassiz**, of Cambridge, Mass., and **Simon Newcomb**, of Washington.

Also, that a committee be appointed to confer with a committee of the British Association on the annual records of science, said committee to consist of J. W. **Dawson**, Montreal, F. W. **Clarke**, Cincinnati, **Edw. S. Dana**, New Haven, H. W. **Martin**, Baltimore, and C. A. **White**, Washington. Both these recommendations were adopted, and the committees appointed.

The following resolution was then proposed and adopted. Moved by Major **Jed. Hotchkiss** of Virginia, seconded by Prof. C. H. **Hitchcock** of Hanover, N. H. :—

Recognizing the relations of the late Prof. **Wm. B. Rogers** to this Association as one of its founders, his eminent and long continued services in the interest of science, and his world-wide reputation as a scientist, be it resolved, that the **Standing Committee** of this Association be requested to appoint some member familiar with the life and labors of Professor **Rogers** to prepare and read at the next annual meeting of this Association a suitable memoir of him, to be subsequently published in the Proceedings.

The Association then resolved itself into a meeting of Section B to listen to a paper "Upon the electrical experiments to determine the location of the bullet in the body of the late President Garfield, and upon a successful form of Inductive Balance for the painless detection of metallic masses embedded in the human body," by **Alex. Graham Bell**; it being understood that the other Sections would suspend their meetings until after the reading of this paper. At the conclusion of this interesting communication the thanks of the members were unanimously tendered to Professor **Bell** for his efforts in the cause of suffering humanity.

In Section C, the Index Committee reported the following resolution which was adopted:

Resolved, That a committee of five be appointed to *devise* and *inaugurate* a plan for the proper indexing of the literature of the chemical elements. Said committee to consist of the Chairman of the Section and four others to be appointed by him. The committee shall have full power to secure the coöperation of volunteers and be required to report at the meeting of the chemical Section in 1883.

F. W. **Clarke**,
A. H. **Elliott**,
H. B. **Nason**.

In Section I, Dr. **Hough** being obliged to leave for home, J. R. **Dodge** of Washington was elected Secretary of the Section.

Friday afternoon a reception was given to the members by Mr. and J. H. R. MOLSON in their house and grounds at Piedmont from 4 to 7 ck.

consequence of the excursions to Quebec and Ottawa, there were no ings of the Association on Saturday. Those who joined the Quebec rsion left by boat on Friday night, reaching Quebec early on Saturnorning, when, after breakfast on board the steamer, they were coned by a Committee of citizens to visit the fortifications, the Citadel the University. After this they embarked on another steamer for an rsion around the harbor, during which an excellent lunch was served oard, and was followed by addresses. The party returned to Mon- on Saturday evening by the North Shore Railway.

e Ottawa party left by early train on Saturday morning and reached apital after a pleasant ride of four hours where they were met by ception committee and conveyed to the various points of interest, which luncheon was provided in the Drill Hall, accompanied by ades of welcome and suitable responses by the members. The party ned to Montreal about midnight much pleased with the generous ment they had received from the citizens of Ottawa. The excursion tawa was conducted by Dr. J. W. DAWSON, and that to Quebec by . STERRY HUNT.

MONDAY, AUGUST 28.

e Association convened in General Session at 10 A. M., the PRESIDENT chair.

e GENERAL SECRETARY read a list of seventeen persons recommended e STANDING COMMITTEE for membership in the Association, and they duly elected.

e special committee on the proposed memorial to CHARLES DARWIN, ted as follows:—

ereas a committee has been designated to coöperate with the sh Committee for collecting subscriptions for a memorial to CHARLES IN, of which American Committee Mr. ALEXANDER AGASSIZ is Treas- the STANDING COMMITTEE of the American Association for the Adment of Science, confident that all American Naturalists, and all ls of science, will be glad to join in the furtherance of this object, y commend it to the Association, and request that all members of ssociation and all others who desire to do honor to the memory oble character, a most sagacious investigator, and a single-hearted of knowledge for its own sake, should send any contributions may wish to make, directly to Mr. ALEXANDER AGASSIZ, at Came, Mass.

e Committee to memorialize the Legislature of New York for a new y of Niagara Falls asked for further time, which was granted.

The Committee to obtain permanent funds for the Association and to advise with the Treasurer and Permanent Secretary on financial matters, was discharged.

The Committee on the best methods of science teaching in the public schools reported that they had no report to present this year, but the STANDING COMMITTEE desired that it should be continued. The Association agreed to this.

Prof. GEORGE J. BRUSH, of New Haven, reported on behalf of the Committee appointed to coöperate with the Committee of the American Philological Association in relation to the proper restriction of the degree of Ph. D. He explained that the degree of Ph. D., which it was first intended should be given only by examination, had been given very largely throughout the country as an Honorary Degree, and the JOINT COMMITTEE resolved to address a circular to the various colleges, representing these facts and asking them to restrict the conferring of the degree. This was accordingly done, the circular being prepared and sent to some 415 colleges throughout the country. He asked that the Committee be now discharged as it had fulfilled the duty for which it was appointed. The Association agreed to this and the Committee was discharged.

The Committee on State Geological Surveys reported progress, and desired to be continued. As this was recommended by the STANDING COMMITTEE, it was agreed to by the Association.

The Committee on Standard Time reported progress and asked for further time to complete their work. The STANDING COMMITTEE recommended that the request be granted, and this was agreed to.

The PRESIDENT announced that Dr. Elliott, who was not present, was to have reported on behalf of the Committee on Weights, Measures and Coinage, and also for the Committee on Registrations of Deaths, Births and Marriages. The STANDING COMMITTEE has recommended that these committees be continued. The Association agreed to their continuation.

The PERMANENT SECRETARY made a number of formal announcements, and the Association then adjourned to meet immediately afterwards in the various sections.

Section D having finished the reading of papers assigned to it concluded its sessions in the afternoon.

In the afternoon a garden party was given to the members of the Association by Mr. and Mrs. L. J. SARGEANT at their residence, Weredale, at the western end of Dorchester street.

In the evening at 8 o'clock, a lecture was delivered before a large audience, by Prof. A. MELVILLE BELL, of Kingston, Ont., at the Queen's Hall, on the subject, "Visible Speech."

A number of the members interested in Microscopy attended the meeting of the MICROSCOPIC CLUB held in the rooms of the Natural History Society. The numerous microscopes belonging to the members were in

e. At 9.30 the gathering was called to order by Mr. JAMES FERRIER,
Dr. CARPENTER was introduced, and in the course of a very inter-
ting address spoke of and explained several microscopes which he had
ought with him from England, and also explained certain objects as
ewed under them.

There was also a large attendance at the ART ASSOCIATION conver-
zione and at the GYMNASTIC ENTERTAINMENT both held at the same time.

TUESDAY, AUGUST 29.

The Association met in General Session at 10 A. M., President DAWSON
the chair.

The GENERAL SECRETARY read the names of fifteen persons recom-
ended by the STANDING COMMITTEE for membership in the Association,
d on motion they were duly elected.

The STANDING COMMITTEE recommended that Prof. G. F. BARKER of
iladelphia be appointed to prepare a memorial of the late Prof. W. B.
GERS; also that the following be appointed a committee on indexing
emical literature: Profs. H. C. BOLTON, Hartford, IRA REMSEN, Balti-
re, F. W. CLARKE, Cincinnati, A. A. JULIEN, New York, A. R. LEEDS,
boken, both of which recommendations were adopted.

The GENERAL SECRETARY then announced that the time had arrived
en the important business of selecting a place for the next year's
eeting was to be settled and read the following invitations:

MINNEAPOLIS, MINN., AUGUST 15, 1882.

At a special meeting of the Minnesota Academy of Natural Sciences
ld this evening, the following resolution was carried:

Resolved, That the Minnesota Academy of Natural Sciences hereby cor-
lly renew the invitation tendered the American Association for the
vancement of Science in the year 1881, to meet in this city in 1882;
d hereby assure the Association in repeating for the coming year, 1883,
at invitation seconded by the city and its various institutions, that its
embers will receive at the hands of our people the same generous hos-
ality promised them at that time.

It was further moved and carried that Prof. N. H. WINCHELL, a member
this Academy, be instructed to present the above resolution to the As-
ciation.

A. F. ELLIOTT, *President.*
C. W. HALL, *Secretary.*

IOWA AGRICULTURAL COLLEGE,
AMES, AUG. 19, 1882.

THE NOMINATING COMMITTEE OF THE A. A. A. S.,

GENTLEMEN:—In behalf of the scientific men of this College and State,
would urge upon you the desirability of selecting a western place of
ecting for the Association in 1883. We will give you most cordial wel-

come. We hope that you will decide to meet in Minneapolis, where you will receive royal treatment.

Very truly,
C. E. Bessey,
Vice President of the Iowa Agricultural College,
and President of the Iowa Academy of Sciences.

The Secretary announced that the Association had also received several other invitations, which were of an informal character, and that the Standing Committee had decided to recommend the acceptance of the invitation from Minneapolis.

The President, Dr. Dawson, said that the Standing Committee had given the matter every consideration, and they had been unanimous in their verdict. Prof. N. H. Winchell had attended their meeting and his description of the city and the facilities it would offer had given the committee every satisfaction.

It was voted unanimously to hold the next annual meeting at Minneapolis, Wednesday, August 15, 1883.

The General Secretary then read the following list of members recommended by the Standing Committee to be elected Fellows of the Association.

Allen, Harrison, Philadelphia, Pa.
Alvord, Henry E., Easthampton, Mass.
Ashburner, Wm., San Francisco, Cal.
Atwater, W. O., Middletown, Ct.
Baker, Marcus, Washington, D. C.
Bartlett, J. R., Washington, D. C.
Blair, Henry W., Washington, D. C.
Borden, Spencer, Fall River, Mass.
Britton, N. L., New Dorp, N. Y.
Burrill, T. J., Champaign, Ill.
Call, R. Ellsworth, Des Moines, Ia.
Chamberlain, Wm. J., Columbus, O.
Chance, H. Martyn, Philadelphia, Pa.
Chandler, Seth C., jr., Cambridge, Mass.
Chester, Albert H., Clinton, N. Y.
Chittenden, Russell H., New Haven, Conn.
Claypole, Edw. W., Yellow Springs, O.
Collier, Peter, Washington, D. C.
Comstock, J. Henry, Ithaca, N. Y.
Dabney, Chas. W., jr., Raleigh, N. C.
Dolbear, A. E., College Hill, Mass.
DuBois, Aug. J., New Haven, Ct.
Dudley, Chas. B., Altoona, Pa.
Dwight, Wm. B., Poughkeepsie, N. Y.
Edwards, Wm. H., Coalburg, W. Va.

Flint, James M., Boston, Mass.
Frazier, B. W., Bethlehem, Pa.
Fuller, A. S., Ridgewood, N. J.
Gatschet, A. S., Washington, D. C.
Hale, Horatio, Clinton, Ont.
Hazen, W. B., Washington, D. C.
Hines, Chas. F., Carlisle, Pa.
Hodges, N. D. C., Salem, Mass.
James, Jos. F., Cincinnati, O.
Jewell, Theo. F., Newport, R. I.
Kendall, E. Otis, Philadelphia, Pa.
Kennan, George, Washington, D. C.
Langdon, F. W., Cincinnati, O.
Lanza, Gaetano, Boston, Mass.
Lazenby, Wm. R., Columbus, O.
Lilly, William, Mauch Chunk, Pa.
Love, Edw. G., New York, N. Y.
Macfarlane, James, Towanda, Pa.
Macloskie, George, Princeton, N. J.
McGee, W. J., Farley, Iowa.
McNair, A. R., Saratoga Springs, N. Y.
Mixter, Wm. G., New Haven, Ct.
Neale, Arthur T., New Brunswick, N. J
Nelson, A. B., Danville, Ky.
Peirce, C. S., Baltimore, Md.
Penhallow, D. P., Mountainville, N. Y.
Perkins, George H., Burlington, Vt.
Pilling, James C., Washington, D. C.
Platt, Franklin, Philadelphia, Pa.
Prentiss, D. Webster, Washington, D. C.
Reed, E. B , London, Ont.
Rockwell, A. P., Boston, Mass.
Ross, Waldo O., Boston, Mass.
Roland, Henry A., Baltimore, Md.
Russell, J. C., Salt Lake City, Utah.
Schanck, J. Stillwell, Princeton, N. J.
Seiler, Carl, Philadelphia, Pa.
Sestini, B., Woodstock, Md.
Sigsbee, Chas. B., Washington, D. C.
Smith, Edwin, Washington, D. C.
Snyder, Monroe B., Philadelphia, Pa.
Spencer, Jas. Wm., Windsor, N. S.
Stallo, J. B., Cincinnati, O.
Stevens, W. Leconte, New York, N. Y.
Stone, George H., Colorado Springs, Col.
Sturtevant, E. Lewis, South Framingham, Mass.
Swift, Lewis, Rochester, N. Y.

Taylor, Arthur F., Cleveland, O.
Thomas, Benj. F., Columbia, Mo.
Thomas, Cyrus, Carbondale, Ill.
True, Fred. W., Washington, D. C.
Trumbull, J. H., Hartford, Conn.
Van Dyck, Francis C., New Brunswick, N. J.
Walcott, Chas. D., Washington, D. C.
Wallace, William, Ansonia, Conn.
Warren, Cyrus M., Brookline, Mass.
Webster, H. E., Schenectady, N. Y.
Wharton, Joseph, Philadelphia, Pa.
Wheeler, Orlando B., Detroit, Mich.
White, I. C., Morgantown, W. Va.
Williams, H. S., Ithaca, N. Y.
Winsor, Justin, Cambridge, Mass.
Wright, Geo. F., Oberlin, Ohio.
Zentmayer, Joseph, Philadelphia, Pa.

These were balloted for and all duly elected.

The PERMANENT SECRETARY announced that the Standing Committee had decided to publish the papers read before the various Sections by title or abstract, unless a special recommendation should come from the Sections in relation to particular papers.

The Association then adjourned to meet in Sections.

Sections A, B, C, F and G finished the papers before them, and adjourned in the afternoon.

In the evening the members of the Association with a large number of citizens of Montreal met in the Queen's Hall to hear the lecture on THE TEMPERATURE OF THE DEEP SEA, by Dr. WILLIAM B. CARPENTER of London. President DAWSON was in the chair, and the officers of the Association with distinguished guests occupied the platform.

The PRESIDENT briefly introduced the lecturer, who on coming forward was greeted with applause.

Dr. CARPENTER, after some introductory remarks, said that the North Polar region was probably sea and covered with ice; while the South Polar region was probably land, or rather a group of islands covered with an ice-sheet extending out to sea and forming the great Antarctic barrier. If the whole area of the globe was divided into fifteen parts, it would be found that the land-surface covered only four, while the sea-surface covered eleven; or in other words the Land-surface was to the Sea-surface as four to eleven. But the proportion is very different when the mass of the water below the sea-level is compared with the mass of the land above it. If the whole land were levelled down to a uniform height, it would barely rise on an average a thousand feet above the sea-level. The average depth of the great ocean basins was however some 12,000 or 13,000 feet. Working out these figures, it would be found that the mass of the Land

s to the mass of the Sea as 1 to 36; or, in other words, the mass of the
a *beneath* the sea-level was 36 times as much as the mass of the Land
ove the sea-level. Until very recent times, the speaker said, much diffi-
lty had been experienced in ascertaining the temperature of this deep
a mass. Many observations had been made of bottom-temperature;
t these were mainly local, and in many cases fallacious on account of
e instruments employed. It has been found that at the enormous pres-
re to which they are subjected, the bulbs of the thermometers are com-
essed, and the mercury forced up into the tube; incorrect results being
us obtained. In 1868 he obtained from the British Admiralty the use
a vessel for carrying down dredging explorations to a greater depth
an had been previously attempted; and being provided with the best
ep-sea thermometers then made, he chanced, in using them, to stumble
on a most remarkable fact. A course had been shaped for the deepest
ter to be found near Great Britain; and a depth of more than 500
homs, or 3,000 feet, was obtained near the Faroe Islands. The
rmometers were let down; and to his great surprise it was found
t they fell to below 32 degrees. The doctrine among Physical Geog-
hers at that time, was that the universal deep sea-temperature was
out 39 degrees, which they considered to be the greatest density of
a water. At the same depth, within twenty miles of the same spot, a
nperature of 45 degrees, 13 degrees higher, was found. This strange
ult excited his interest, as he considered that it had a most important
ring on fundamental questions of Physical Geography and Geology.
brought his results home, and urged upon the Admiralty the impor-
ce of ascertaining what effect different pressures would have upon the
st-constructed thermometers. This was ascertained by the hydrostatic
ess; and it was found that at the pressure of three tons on the square
h (which was what their bulbs would have to sustain at a depth of
out 2,400 fathoms) the thermometers went up from 10 to 20 degrees,—
owing the fallacy of all past observations on deep-sea temperature.
lf a ton pressure was found to send the instrument up some 2½ degrees;
l from this he concluded that the temperature of 32 degrees which
y had found was in reality 29½°. The next thing was to devise means
which this pressure on the thermometer could be obviated. This was
ne, at the suggestion of the late Prof. W. A. Miller, by simply enclosing
bulb of the thermometer in another bulb with some fluid between, but
o including a vacuum; so that a slight reduction in the capacity of the
ter bulb did not affect the real thermometer bulb. In all further ob-
vations these instruments were used, and gave the true temperatures.
e result of these observations was, that the doctrine that 39 degrees
s the general temperature of the deep sea was shown to be a fallacy; and
proved to have been propounded in ignorance of the fact long previ-
sly determined by the careful experiments of Despretz, that *sea*-water
nlike fresh water) continues to contract in cooling, down to its freez-
;-point at or under 28° Fahr. He was so much struck with his experi-
ce in the Faroe Channel, that he soon afterwards obtained another

steamer, and took observations with the new thermometers; when the real temperature of the cold area was found to be 29½ degrees, as they had expected. The bottom temperature of the North Atlantic, on the other hand was uniformly 35° Fahr. The cause of this was, that while the Faroe channel is an extension of the Arctic basin, the coldest Polar water cannot find its way into the basin of the North Atlantic, being kept back by ridges extending across the channels between Greenland and Iceland, and between Iceland and the Faroes.—The observations were carried down through the Bay of Biscay in 1870, and into the Mediterranean Sea. From all these observations the important conclusion was drawn, that the bottom-temperature of the deep sea is not determined by the latitude, but by the freedom of communication with either of the great Polar areas, permitting a vast underflow of Polar water towards the Equator, with a corresponding slow movement of the upper stratum from the Equator toward the Poles.

The lecturer then referred briefly to the subject of the Gulf Stream. This subject, he said, possesses much interest to persons on either side of the Atlantic. An enthusiastic American had asserted that America, by cutting a channel through the Isthmus of Panama, could entirely divert the Gulf Stream, and turn Great Britain into a howling wilderness. (Laughter.) The speaker assured the audience that this was no joke of his own, but had actually been propounded. But he thought he could show that there was not much truth in such a statement; and proceeded to explain what (in his view) the Gulf Stream does, and what it does not. The temperature of Great Britain is affected by a surface-flow of warm water flowing in a northeasterly direction. This serves to keep open all the harbors of Norway the most part of the winter, and was certainly of the greatest importance. Now, all marine surveyors had come to the conclusion that the Gulf Stream dies out as a current in the middle of the Atlantic; and that the remaining flow is mere surface-drift produced by the prevalence of southwesterly winds, which carries it northeast. It is demonstrable that in this passage any stratum of only 100 fathoms thickness (and the spread-out Gulf Stream current cannot certainly be deeper than this) would soon take the temperature of the air above it, so as to lose its special heating power. But the Challenger temperature-sections have shown that the Gulf Stream carries into mid-Atlantic an enormous body of warm water; which, in virtue of the higher specific gravity it derives from concentration, there sinks down, displacing colder water and thus makes the mid-Atlantic a storehouse of heat. The question then arises, How does this water get further? The speaker explained that this warm water is carried northeast by the general oceanic circulation, which brings to the British and Norwegian shores a stratum of warm water 500 fathoms deep, deposited as it were for their use in the mid-Atlantic. If they had not this General Circulation, the Gulf Stream would never come near their shores. If they had not the Gulf Stream, the General Circulation would do much less towards their warmth.—The speaker illustrated his remarks by numerous interesting diagrams.

At the close of the lecture, on motion of Professor **James Hall**, a hearty ote of thanks was tendered to Dr. **Carpenter**.

Afterwards a reception was given to the members of the Association by Ir. and Mrs. M. H. **Gault**, in their beautiful grounds, which were bril iantly illuminated with Chinese lanterns, and handsomely decorated for he occasion. During the evening an admirable selection of music was endered by Gruenwald's orchestra.

An "at home" was also given at the same time by Hon. Justice and Mrs. **Iackay**.

Wednesday, August 30.

The Association at 10 A. M. met in General Session in Molson Hall, Presi lent **Dawson** in the chair.

The **General Secretary** read the names of ten persons recommended y the **Standing Committee** for membership, all of whom were elected. This makes three hundred and twenty-five new members elected since he opening of the meeting in Montreal.

The **Standing Committee** recommended the granting of the request from Section C, asking for a Committee to petition the Agricultural Depart nent of the United States or Canada to print the paper on "Sources of itrogen in the soil" by Messrs. **Lawes** and **Gilbert**, and the following Committee was appointed: **George B. Loring**, **William Saunders**, . V. **Riley**, E. B. **Elliott** and O. T. **Mason**.

The **Standing Committee** recommended the following amendment to **rticle vii** of the Constitution.

"The Standing Committee shall also have power to exclude from the ssociation any member or fellow on satisfactory evidence that said nember or fellow is an improper person to be connected with the Asso iation, or has in their estimation made improper use of his membership r fellowship."

The consideration of this proposed amendment will take place at the ext meeting.

Mr. **Elliott**, Vice President of the Section of Economic Science and tatistics, presented the following report with the sanction of the Stand g Committee:

"The Sectional Committee of the Section of Economic Science and tatistics, have considered the papers referred to them having reference o the Standard of Time, and recommend:

"First, That the Association should express its gratification at the pas age of the joint resolution by the Congress of the United States, author zing the President of the United States to call an international conference o fix on and recommend for universal adoption a common prime me idian, to be used in the reckoning of longitude and the regulation of ime throughout the world.

"Second, They further recommend the appointment by the Association f a Committee to confer in such a manner as it may seem best with the

Convention named in the resolution of Congress, and also to represent this Association, as its delegates, in any other convention that may be called to consider the subject of the standard of time.

"It is understood that the important legislation on the part of Congress is due in no small measure to the earnest and well directed efforts of the American Meteorological Society and of the American Society of Civil Engineers. A communication from Mr. Sandford Fleming, representing the American Society of Civil Engineers, has been received by this Sectional Committee, and will be referred to the Committee which may be appointed.

"The following Committee on a primary meridian and an international standard of time is respectfully suggested, viz.: F. A. P. Barnard, New York; C. A. Young, Princeton; T. Sterry Hunt, Montreal; J. K. Eastman, Washington; Charles Carpmael, Toronto, and E. B. Elliott, Washington.

The report was accepted and the designated committee appointed.

The Permanent Secretary then read the list of the officers proposed by the Nominating Committee for the next year, as follows:

President — C. A. Young of Princeton, N. J.

Vice Presidents — Section A — W. A. Rogers, Cambridge, Mass.; B — H. A. Rowland, Baltimore, Md.; C — Edward W. Morley, Cleveland, O.; D — De Volson Wood, Hoboken, N. J.; E — C. H. Hitchcock, Hanover, N. H.; F — W. J. Beale, Lansing, Mich.; G — J. D. Cox, Cincinnati, O.; H — O. T. Mason, Washington, D. C.; I — F. B. Hough, Lowville, N. Y.

General Secretary — J. R. Eastman of Washington, D. C.

Assistant General Secretary — Alfred Springer of Cincinnati, Ohio.

Secretaries of the Sections — Section A — W. W. Johnson, Annapolis, Md.; B — C. K. Wead, Ann Arbor, Mich.; C — J. W. Langley, Ann Arbor, Mich.; D — A. J. Dubois, New Haven, Conn.; E — Alexis A. Julien, New York, N. Y.; F — S. A. Forbes, Normal, Ill.; G — Carl Seiler, Philadelphia, Pa.; H — G. H. Perkins, Burlington, Vt.; I — Joseph Cummings, Evanston, Ill.

Treasurer — Wm. Lilly of Mauch Chunk, Pa.

The whole list was elected amidst considerable enthusiasm.

Dr. T. Sterry Hunt made a statement in reference to the International Geological Committee, of which Professors Hall, Selwyn, Lesley, and himself had been appointed representatives from North America. Several months ago a report was prepared by them. The work was not yet completed, and, on the recommendation of the Standing Committee, he moved that the Committee be continued. The motion was seconded by Professor Hall, and the Committee was continued.

The Permanent Secretary made a number of announcements, and the President gave some particulars regarding the excursions for the day. The Session then adjourned to meet in Sections.

Sections E, H, and I finished their work, and finally adjourned at the close of the morning session.

In the afternoon the members were taken on an excursion around the harbor to the Victoria Bridge and to the Grand Trunk workshop, returning soon after six o'clock, well pleased with their trip.

At 8.30 P. M. the concluding General Session was held in the William Molson Hall, when there was a large attendance of the members of the Association and the citizens of Montreal.

President DAWSON said they had now arrived at the closing exercises of the meeting, and that he would call upon the Permanent Secretary, Professor PUTNAM, to make some remarks with reference to his latest work, the programme of the Association.

In reply, the PERMANENT SECRETARY stated that this was the largest meeting the Association had ever held, with the exception of that at Boston two years ago, when sixty more names were entered on the register. During the present meeting 937 names were recorded on the Association register. Of these, 20 were invited guests and their ladies, from England, Ireland, Hungary, Austria, France and Russia; 579 members and 338 representatives of families of members were in attendance from the following places: Montreal, 90, other parts of the Dominion of Canada, 44; New York, 199; Massachusetts, 158; Ohio, 74; District of Columbia, 60; Pennsylvania, 43; New Jersey, 38; Connecticut, 30; Illinois, 22; Missouri, 22; Michigan, 18; Vermont, 16; Maryland, 15; Rhode Island, 14; Maine, 13; New Hampshire, 12; Indiana, 10; Florida, 5; South Carolina, 4; Virginia, 4; Colorado, 3; Minnesota, 3; Nebraska, 3; California, 2; Tennessee, 2; Mississippi, 2; North Carolina, 2; Louisiana, 2; Wisconsin, 2; and Alabama, Kentucky, West Virginia, Kansas and Iowa, one each; total, 937. Three hundred and twenty-five new members had been elected besides the members of the Local Committee who wished to accept membership in the Association. Two hundred and fifty-four papers had been sent in, which, with the exception of twelve, had been called up in the nine sections, that had been in session during the greater part of the week, and nine addresses and two lectures had been delivered.

The following resolutions were passed unanimously:

Prof. E. D. COPE, of Philadelphia, moved the following resolution:—

That the Local Committee of the Association have won our heartfelt esteem for the admirable efficiency with which they have planned and carried out the arrangements for our comfort. Especial mention should be made of the beautiful and complete "Handbook to the City of Montreal," prepared for the occasion by Mr. S. E. DAWSON, which has been a constant guide to us all; of the labors of the Railway Committee, Lodging Committee, the Excursion Committee, the Finance Committee, the Printing Committee, and the Reception Committee.

In moving this resolution, Professor COPE enumerated the different duties performed by the various committees, and expressed the hope that they had received some return for the great sacrifice which they had been called upon to make.

Dr. E. B. ELLIOTT, of Washington, moved the following:—

That the American Association for the Advancement of Science, deeply sensible of the kindness which they have received in Montreal, hereby declare their thanks to the hotels, the Diocesan Theological College and private citizens for hospitality, and to the ladies for the elegant garden parties and other courtesies extended to the members and their families.

In moving this resolution Dr. ELLIOTT said he wished that he felt able to move it in a fitting manner. His duties as Vice President of one of the Sections had occupied the most of his time, and he had not had much leisure to take advantage of many of these entertainments. He referred to the hospitality extended to the members of the Association in this city twenty-five years ago, which he had no doubt was prompted by their love of science, and said he felt that the newspapers of Montreal were educating the people to a high standard in this direction, and their example might be profitably followed even by some of the prominent newspapers of the United States. He concluded by referring to the very large accession of members which the Association had received at this meeting.

Professor Cox moved the following resolution:—

That the American Association hereby extends its heartfelt thanks to the Corporation and Faculty of the McGill University, of the Presbyterian Theological College, and the Society of Natural History, for commodious quarters during the meeting, and for the consideration with which every want has been anticipated and gratified.

In offering this resolution Professor Cox said he considered that it would be fully sustained by all the members. They had seldom been provided with more extensive, commodious or convenient rooms, and he was sure they would long remember the edifices of this University, its splendid library, museum and beautiful grounds.

Prof. WILLIAM SAUNDERS, of London, Ont., moved the following:—

That the thanks of the American Association are due and are most heartily tendered to the numerous railways of the United States and the Dominion, and to the several steamship companies, for their courtesy in affording members excellent facilities for attendance at this meeting. We especially acknowledge our obligations to the Grand Trunk Railway Company for the excursion to Lachine, and for the freedom of that monument of engineering skill, the Victoria Bridge. We also thank the Harbor Commissioners for the excursion of to-day.

Prof. Saunders in his remarks said that the subject of transportation was in one sense a dull one, and had nothing very transporting about it. The Companies had so carefully conducted their arrangements that the baggage as well as the persons of members had been well cared for.

rofessor **Leeds** moved the following resolution:—

hat the cordial thanks of this Association are due and tendered to the
egraph and Telephone Companies for their great liberality in extend-
the free use of their wires to the members and fellows during the
eting.

n offering this resolution, Prof. **Leeds** said he did so with a full sense of
great obligation they were under to these companies for the aid they
given. But as the Association for the Advancement of Science he
ught these companies in their turn owed them something, as it had
n the mother of electricity, and one of its members shared with Morse
honor of the discovery of the electric telegraph,—Joseph Henry.
pplause.) And since that time the Association had given much thought
care to the subject of electricity, as evinced by the valuable contri-
ions from Edison and other great electricians, and the recent valuable
ture by Prof. Alex. Graham Bell. He, therefore, thought they could
that the privileges granted by the companies were in one sense a due
cession to science.

rofessor **Putnam** remarked that for two months previous to the meet-
the telegraph company had given free transmission of messages to
nself and the Secretary of the Local Committee in Montreal.

r. T. **Sterry Hunt**, of Montreal, moved:

hat the members of the Association and their families will ever hold
grateful remembrance the generosity of the Richelieu & Ontario Navi-
ion Company and of the North Shore Railway for the delightful excur-
ns to Quebec, and the Mayor, Local Committee and citizens of that
ply interesting spot, for the harbor excursion and the bounteous colla-
n provided for the enjoyment of their guests.

he mover referred to the incidents of the trip and the pleasure en-
ed by all those who attended the visit to Dufferin Terrace, to the
ient Citadel, Laval University, the Harbor, etc. He considered that
those who attended the excursion would need have nothing said to
m to make them support the resolution. He called upon Dr. **Hings-**
, as Chairman of the Conveyance Committee, to add a few words of
nks to the railway and steamboat authorities.

r. **Hingston** made a few remarks, in the course of which he said
t the President, Dr. **Hunt** and himself were surviving members of
Local Committee who entertained the Association in this city
enty-five years ago. If Darwin or any of his disciples were present he
uld say it was another illustration of the survival of the fittest.
ughter.) He expressed their thanks to the railway authorities for the
diness with which they had responded to their appeals, and did every-
ng in their power to add to the comfort and convenience of the mem-
s of the Association.

he **President**, Dr. **Dawson**, remarked that the members of the Asso-
tion felt very much indebted to the citizens of Quebec for the aid they
given in making this meeting an agreeable one.

Dr. **JOHN RAE** moved the following resolution:—

That the thanks of the Association are most cordially tendered to the Canadian Pacific Railway for the special train to Ottawa, and to the Mayor, Local Committee and citizens of Ottawa, for the delightful view of the natural beauties, industries and public buildings of the place, for their elegant repast and for their kind words they were pleased to bestow upon the Association.

Dr. **RAE** said that all those who went to Ottawa and saw its beautiful buildings would agree with him that it was one of the most charming days they spent. He spoke of the beauty of the Parliament buildings, and the universal admiration with which they were regarded.

Prof. **JAMES HALL**, in supporting the motion, said that no one who had seen the library of Parliament at Ottawa could fail to express their admiration. It had no equal in the United States. The Museum at Ottawa was also a valuable institution, and it must always be the resort of scientific men. For this reason they should foster it. He, the speaker, had advocated the holding of the first meeting of the Association at Montreal twenty-five years ago, and had been largely instrumental in having it held here. He had many pleasant reminiscences of that meeting, and would always remember Montreal.

The Rev. Dr. **HOVEY** moved the following:

That the thanks of the American Association are due to the gentlemen of the local press, and the agents of papers in other cities, for the untiring pains which they have taken in reporting the proceedings of the Montreal meeting.

In moving this resolution, Dr. **HOVEY** said if it were not for the gentlemen of the press the deliberations of the members would not be so widely known as they are. He enumerated the various city papers to which they were indebted, as well as those of Boston, New York and Chicago, and also added that the proceedings of the Montreal meeting were, he considered, better reported than any previous one.

Dr. **HAUGHTON** of Dublin offered the following:

The American Association is especially honored at this Montreal meeting by the presence of so large a number of eminent visitors from abroad, thereby conferring an international character upon this assemblage on Canadian soil.

He said that a task had been assigned to him which no one but an Irishman could perform. He had been required to propose a vote of thanks to himself and then respond to it. (Laughter.) After returning thanks on behalf of the visitors for the courtesies extended to them by the Association, he said the only way in which he could express his opinion of the hospitality of which they had been the recipients, was by saying that if his mother had not been an Irishwoman he could have wished she had been a Canadian. [Applause.]

Prof. O. T. MASON, with a few humorous remarks, moved:

That a vote of thanks be returned to the Southeastern Railway Com-
any for the contemplated excursion to Newport and Lake Memphrema-
og.

Dr. ALEX. WINCHELL moved:

That the thanks of the Association be given to the officers of McGill
niversity, Mechanics' Institute, Art Association, Natural History So-
ety, the Montreal and Amateur Athletic Association, Numismatic
ociety, Young Men's Christian Association, and the Merchants' Exchange
r invitations extended to members of the Association.

Dr. WINCHELL, in moving it, spoke in high terms of the Art Gallery
d the Peter Redpath Museum, and the high educational value of such
ollections to the citizenship of the community at large.

After the passage of the several resolutions, President DAWSON
rought the proceedings to a close with some observations in regard to
e object of the gathering just brought to a termination. It was very
ratifying, he said, to know that this has been one of the most successful
eetings of the Association, not only in regard to attendance, but in re-
ect to the papers read and the practical benefits which might be ex-
ected to follow. He hoped all would carry away something which would
ake them wiser and better. He also hoped that they would carry away
ith them pleasant memories and a stimulus and encouragement which
ould lead them to greater achievements in the future.

The meeting then adjourned to meet at Minneapolis on the third Wed-
esday in August in next year.

ON THURSDAY MORNING

A party of nearly four hundred, composed of members and their ladies,
ailed themselves of the liberality of the SOUTHEASTERN RAILWAY and
ok part in the complimentary excursion to the little town of Newport
d the picturesque lake Memphremagog. The weather was propitious
d the party reached their destination at 12.30, where after partaking of
excellent dinner they embarked on the steamer "Lady of the Lake,"
d enjoyed a magnificent sail of about seventeen miles up, passing many
oints of interest and returning to Newport in time to take the 5.15 train
Montreal where the members finally separated.

WM. SAUNDERS,
General Secretary.

REPORT OF THE PERMANENT SECRETARY.

By holding a meeting for the second time beyond the limits of the United States, the Association has shown that its name of American is to be received in the widest sense.

A quarter of a century ago its eleventh meeting was held in Montreal, and again it was welcomed to the city for its thirty-first. In 1857 there were 351 names entered on the register of persons in attendance, and about 250 new members were elected; in 1882 there were 937 names registered and over 800 new members were elected. In 1857, 118 papers were read before two sections and one subsection, the regular presidential address and two memorial addresses were delivered; in 1882, 242 papers were presented before nine sections, and in addition to the presidential address there were seven by vice-presidents, also one special address and two evening lectures. In the volume of the proceedings relating to the eleventh meeting there are given the names of 946 members; in the present volume there are 1922. These few comparative statements show the growth of the Association in twenty-five years, and in this connection it must be remembered that during the civil war the meetings were suspended for five years.

Of the 255 papers entered on the list of titles received, one came after section D, to which it belonged, had adjourned; three were withdrawn by their authors, and eight were declined for various reasons. The paper by Mr. Meehan was given as an address before the biological section in accordance with the vote of the former section B at the Cincinnati meeting, and Mr. Hovey's paper on cave scenery was delivered in the theatre of the Redpath Museum on the evening of the formal opening of the Museum. Of the remaining 241 papers, 18 were referred to section A, 37 to B, 29 to C, 9 to D, 45 to E, 40 to F, 16 to G, 32 to H, and 15 to I.

The present volume contains the address delivered by Professor Hall, as retiring president of the Montreal meeting of 1857, now for the first time printed. The presidential address of Prof. Brush, and those of seven vice-presidents, the special address of Mr. Meehan, an abstract of Dr. Carpenter's lecture, and 124 papers either in full or by more or less extended abstracts as follows: Section A, 16; B, 13; C, 14; D, 7; E, 25; F, 25; G, 10; H, 10; I, 4. One hundred and seventeen other papers are mentioned by title simply, either from their authors failing to send abstracts for publications, or, in a few instances, in accordance with the votes of the standing committee that they should so appear. The delay in printing the volume has been due to a combination of causes for which neither the secretary nor the printers are responsible.

Of the 87 members elected fellows during the meeting, one has resigned his membership, one has declined, four have not yet answered the notices sent them, and 81 have accepted.

Of the new members elected, fifteen were invited foreign guests, twenty-two have declined, forty-eight have not yet been heard from, and 288 have paid in full, and their names are entered on the roll.

Seven members and fellows have become life-members since the report given in the Cincinnati volume.

For a statement relative to the 937 members and others registered at the meeting, I refer to the paragraph on page 635 of this volume.

Two thousand and fifteen copies of the Cincinnati volume of proceedings were received from the printers, and 1497 were sent to members entitled to receive them, 23 have been sold, and 177 distributed in accordance with votes of the Standing Committee. Of the preceding volumes, 77 have been sent to members entitled to them, 51 sold, and 19 have been distributed by vote of the Standing Committee. Four of the early volumes have been purchased and two have been received as a gift.

The Nashville volume was reprinted and stereotyped during the year, thanks to the liberality of General Lilly, and 253 copies were received from the printer. The St. Louis volume will be reprinted during the present year by the liberal gift of a member. After this is done there will be left on hand several hundred dollars which will be reserved for reprinting the second volume as soon as the required amount is secured.

Since the report given in the Cincinnati volume, under date of April 26, notices of the decease of 39 members and fellows have been received, among whom are three past-presidents, including the only Honorary Fellow of the Association; the former Treasurer, a former General Secretary, and four original members. Of these the names of thirty-two were read at Montreal and are recorded on pages 618–19 of this volume.

During the same period the names of 67 members and fellows have been taken from the list in consequence of resignations or from their being over two years in arrears in their assessments. In the same time 24 names have been restored to the list. Seven members elected at Cincinnati have accepted during the past year, in addition to the 819 mentioned in the last report.

The total number of members and fellows at the present date is 1920, a net gain of 221 since the list was printed in the Cincinnati volume.

For a statement of the receipts and expenses for the year covering the Cincinnati meeting and ending just previous to the Montreal meeting, I refer to the cash account on the following pages.

The invested funds of the Association are as follows:

General Fund, $84 67, int. to Aug. 1, '83, $4 23, . . .	$88 90	
Life Membership Fund, $1900 54, int. to Aug. 1, $73 00,	1973 54	
		$2062 45

The subscription fund for reprinting stands as follows:

June 10, 1881,	Gift of A. Schaffranck, ...	5 00	
Aug. 16, 1881,	Mrs. H. Herrman, ...	100 00	
	G. J. Brush, ...	20 00	
	William Bross, ...	20 00	
	William Lilly, ...	10 00	
	Thomas Meehan, ...	5 00	
	George Holley, ...	5 00	
	Robert Peters, ...	5 00	
	L. H. Trowbridge, ...	5 00	
	W. LeConte Stevens, ...	5 00	
	F. P. Dewey, ...	5 00	
	Alfred Springer, ...	5 00	
	J. D. Cox, ...	5 00	
	S. F. Peckham, ...	2 00	
	J. B. Killebrew, ...	2 00	
	F. W. Clarke, ...	2 00	
	W. H. Walmsley, ...	1 00	
	Several unknown contributors,	6 00	
April 10, 1882,	William Lilly, ...	1000 00	
July 3, 1882,	Geo. L. Vose, ...	1 00	
August 23, 1883,	A Member, ...	850 00	
Interest to Aug. 14, 1882,	. . . .	8 16	
Interest to Aug. 1, 1883,	. . . .	46 30	
			$2113 46
Cost of reprinting and stereotyping Nashville volume,		$857 94	
Balance on hand,	. . . .	1255 52	
			$2113 46

Respectfully submitted to the Association,

F. W. Putnam, *Permanent Secretary.*

Salem, Mass., July 27, 1883.

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1881-2.		
To Assessments previous to the 30th Meeting . .	$408 00	
" Cincinnati, " " . .	3,315 00	
" Montreal, 31st " . .	1,359 00	
		$5,082 00
Entrance fees Boston Meeting	15 00	
" " Cincinnati "	1,485 00	
" " Montreal "	330 00	
Fellowship fees	112 00	
		1,942 00
Sales of Publications and Binding	334 76	
Advertisements on covers	111 00	
Illustrations	3 00	
Postage 1.18; sale of old paper .55	1 73	
		450 49
Life Membership commutations (Intr. 6.00) .	256 00	
Reprinting fund, subscriptions (Intr. 8.16) .	212 16	
William Lilly, Patron	1,000 00	
		1,468 16
		$8,942 65

SALEM, MASS., AUGUST 15, 1882.

I have examined the above account and find

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE.

Cr.

1881–2.

By 2015 copies Proceedings, Vol. 30, Cincinnati Meeting:—		
Composition, stereotyping, and boxes for plates	$1,129 40	
Illustrations	22 49	
Paper and press-work	921 40	
Covers	20 00	
Binding 1,962 in paper 470.88; 40 in cloth 20.00	490 88	
" 15 in half-morocco 15.00; 25 covers 5.00	20 00	
Printing wrappers and wrapping	18 00	
Distribution by mail and express	229 30	
Extras of Addresses and Reports	38 19	
		$2,889 66
Vol. 29, Illustrations 2.40; Addresses 9.50	11 90	
Distribution, balance of postage and express	120 17	
		132 07
Expenses of Cincinnati Meeting		488 96
Publications bought, and binding		57 62
Extras of Addresses Montreal volume		15 41
General expenses:—		
Rent of office to July 1, 1882	100 00	
Janitor to Aug. 1, 1882	102 00	
Fuel 40.50, Express 46.40	86 90	
Minor expenses, stationery, office furniture, etc.	25 45	
Subscription to Postal Guide	1 50	
Postage and envelopes 286.31, P. O. box 8.00	294 31	
Printing circulars, blanks, cards, etc.	260 76	
Extra clerk hire	33 25	
		904 17
Balance paid Perm. Sec'y, due last acct.		773 03
Salary of Assistant Secretary	400 00	
" " Permanent "	1,000 00	1,400 00
Life Membership Fund 250.00, Intr. 6.00	256 00	
Reprinting Fund, including 5.00 received in 1880	365 62	
" and stereotyping Vol. 26 (250 copies)	851 54	1,473 16
Balance to new account		808.57
		$8,942 65

the same correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor*.

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OF

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FOR THE

ADVANCEMENT OF SCIENCE,

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T. Sterry Hunt of Montreal,
J. S. Newberry of New York,
N. H. Winchell of Minneapolis,
C. H. Hitchcock of Hanover,
J. P. Lesley of Philadelphia,
G. H. Cook of Brunswick,
E. A. Smith of Tuscaloosa,
Joseph Le Conte of San Francisco.

This Committee has power to add to its number.

10. *Committee on Interchange of Courtesies between the American and British Associations for the Advancement of Science.*

C. A. Young of Princeton,
J. P. Lesley of Philadelphia,
J. W. Dawson of Montreal,
T. Sterry Hunt of Montreal,
E. S. Morse of Salem,
T. C. Mendenhall of Columbus,
F. W. Putnam of Cambridge.

11. *Committee in relation to Duty on Scientific Books.*

H. A. Rowland of Baltimore,
C. A. Young of Princeton,
J. W. Powell of Washington,
J. D. Cox of Cincinnati,

E. S. Morse of Salem.

12. *Auditors.*

Henry Wheatland of Salem,
Thomas Meehan of Germantown.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION OF GEOLOGISTS AND NATURALISTS.

MEETING.	DATE.	PLACE.	CHAIRMAN.	SECRETARY.	ASSIST. SEC'Y.	TREASURER.
1st	April 2, 1840,	Philadelphia,	Edward Hitchcock,*	L. C. Beck,*		
2d	April 5, 1841,	Philadelphia,	Benjamin Silliman,*	L. C. Beck,*	B. Silliman, jr., C. B. Trego,	
3d	April 25, 1842,	Boston,	S. G. Morton,*	C. T. Jackson,	J. D. Whitney, M. B. Williams,	
4th	April 26, 1843,	Albany,	Henry D. Rogers,*	B. Silliman, jr.,		John Locke.*
5th	May 8, 1844,	Washington,	John Locke,*	B. Silliman, jr., O. P. Hubbard,		Douglas Houghton.*
6th	April 30, 1845,	New Haven,	Wm. B. Rogers,	B. Silliman, jr., J. Lawrence Smith,*		Douglas Houghton.*
7th	Sept. 2, 1846,	New York,	C. T. Jackson,*	B. Silliman, jr.,		E. C. Herrick.*
8th	Sept. 20, 1847,	Boston,	Wm. B. Rogers,†	Jeffries Wyman,*		B. Silliman, jr.

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*Deceased.

†Professor ROGERS, as chairman of this last meeting, called the first meeting of the new Association to order and presided until it was fully organized by the adoption of a constitution. As he was thus the first presiding officer of the new Association, it was directed at the Hartford meeting that his name be placed at the head of the Past Presidents of the American Association for the Advancement of Science.

MEETINGS AND OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE-PRESIDENT.	GENERAL SECRETARY.	PERMANENT SEC'Y.	TREASURER.
1st	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield,*		Walter R. Johnson,*		Jeffries Wyman.*
2d	Aug. 14, 1849,	Cambridge, Mass.,	Joseph Henry,*		E. N. Horsford, 1		A. L. Elwyn.
3d	Mar. 12, 1850,	Charleston, S. C.,	A. D. Bache,* 2		L. R. Gibbs, 3		St. J. Ravenel.* 4
4th	Aug. 19, 1850,	New Haven, Conn.,	A. D. Bache,*		E. C. Herrick,*		A. L. Elwyn.
5th	May 5, 1851,	Cincinnati, Ohio,	A. D. Bache,*		W. B. Rogers, 5 *	S. F. Baird,	S. F. Baird. 6
6th	Aug. 19, 1851,	Albany, N. Y.,	Louis Agassiz,*		W. B. Rogers,*	S. F. Baird,	A. L. Elwyn.
7th	July 28, 1853,	Cleveland, Ohio,	Benjamin Pierce,*		S. St. John, 7	S. F. Baird,	A. L. Elwyn.
8th	April 26 1854,	Washington, D.C.,	J. D. Dana,		J. Lawrence Smith,*	Joseph Lovering,	J. L. LeConte.* 8
9th	Aug. 15, 1855,	Providence, R. I.,	John Torrey,*		Wolcott Gibbs,	Joseph Lovering,	A. L. Elwyn.
10th	Aug. 20, 1856,	Albany, N. Y.,	James Hall,		B. A. Gould,	Joseph Lovering,	A. L. Elwyn.
11th	Aug. 12, 1857,	Montreal, Canada.	Alexis Caswell,* 9	Alexis Caswell,*	John L. LeConte,*	Joseph Lovering,	A. L. Elwyn.
12th	April 28, 1858,	Baltimore, Md.,	Alexis Caswell,* 10	John E. Holbrook,*†	W. M. Gillespie,* 11	Joseph Lovering,	A. L. Elwyn.
13th	Aug. 3, 1859,	Springfield, Mass.,	Stephen Alexander,*	Edward Hitchcock,*	William Chauvenet,*	Joseph Lovering,	A. L. Elwyn.
14th	Aug. 1, 1860,	Newport, R. I.,	Isaac Lea,	B. A. Gould,	Joseph LeConte,	Joseph Lovering,	A. L. Elwyn.
15th	Aug. 15, 1866,	Buffalo, N. Y.,	F. A. P. Barnard,	A. A. Gould,* 12	Elias Loomis, 13	Joseph Lovering,	A. L. Elwyn.†
16th	Aug. 21, 1867,	Burlington, Vt.,	J. S. Newberry,	Wolcott Gibbs,	C. S. Lyman,	Joseph Lovering,	A. L. Elwyn.†
17th	Aug. 5, 1868,	Chicago, Ill.,	B. A. Gould,	Charles Whittlesey,	Simon Newcomb, 14	Joseph Lovering,	A. L. Elwyn.†
18th	Aug. 18, 1869,	Salem, Mass.,	J. W. Foster,*	O. N. Rood,	O. C. Marsh,	F. W. Putnam, 15	A. L. Elwyn.†
19th	Aug. 17, 1870,	Troy, N. Y.,	T. S. Hunt, 16	T. S. Hunt,	F. W. Putnam, 17	Joseph Lovering,	A. L. Elwyn.†
20th	Aug. 16, 1871,	Indianapolis, Ind.,	Asa Gray,	G. F. Barker,	F. W. Putnam,	Joseph Lovering,	W. S. Vaux.*
21st	Aug. 15, 1872,	Dubuque, Iowa,	J. Lawrence Smith,*	Alex. Winchell,	E. S. Morse,	Joseph Lovering,	W. S. Vaux.*
22d	Aug. 20, 1873,	Portland, Me.,	Joseph Lovering,	A. H. Worthen,†	C. A. White,	F. W. Putnam,	W. S. Vaux.*
23d	Aug. 12, 1874,	Hartford, Conn.,	J. L. LeConte,*	C. S. Lyman,	A. C. Hamlin,	F. W. Putnam,	W. S. Vaux.†*

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1. In place of Jeffries Wyman, *not present.*
2. In place of Joseph Henry, *not present.*
3. In place of E. C. Herrick, *not present.*
4. In place of A. L. Elwyn, *not present.*
5. In place of E. C. Herrick, *not present.*
6. In place of A. L. Elwyn, *not present.*
7. In place of J. D. Dana, *not present.*
8. In place of A. L. Elwyn, *not present.*
9. In place of J. W. Bailey, *deceased.*
10. In place of Jeffries Wyman, *not present.*
11. In place of Wm. Chauvenet, *not present.*
12. In place of R. W. Gibbes, *not present.*
13. In place of W. P. Trowbridge, *not present.*
14. In place of A. P. Rockwell, *called home 1st day.*
15. In place of Joseph Lovering, *in Europe.*
16. In place of Wm. Chauvenet, *too ill to be present.*
17. In place of C. F. Hartt, *in Brazil.*

* Deceased. † Not present at the meeting.

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENT, SECTION A.	VICE PRESIDENT, SECTION B.	CHAIRMAN OF PERMANENT SUBSECTION OF CHEMISTRY.	CHAIRMAN OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	CHAIRMAN OF PERMANENT SUBSECTION OF MICROSCOPY.	CHAIRMAN OF PERMANENT SUBSECTION OF ENTOMOLOGY.	PERMANENT SECRETARY.	GENERAL SECRETARY.	SECRETARY OF SECTION A.	SECRETARY OF SECTION B.	SECRETARY OF PERMANENT SUBSECTION OF CHEMISTRY.	SECRETARY OF PERMANENT SUBSECTION OF ANTHROPOLOGY.	SECRETARY OF PERMANENT SUBSECTION OF MICROSCOPY.	SECRETARY OF PERMANENT SUBSECTION OF ENTOMOLOGY.	TREASURER.
24th	Aug. 11, 1875.	Detroit, Mich.,	J. E. Hilgard,	H. A. Newton,	J. W. Dawson,	S. W. Johnson,	L. H. Morgan,*	———	———	F. W. Putnam,	S. H. Scudder,	S. P. Langley, T. C. Mendenhall,	E. S. Morse,	F. W. Clarke,	F. W. Putnam,	———	———	W. S. Vaux.*
25th	Aug. 23, 1876.	Buffalo, N. Y.,	W. B. Rogers,*	C. A. Young,	E. S. Morse,	G. F. Barker,	L. H. Morgan,*	R. H. Ward,	———	F. W. Putnam,	T. C. Mendenhall,	A. W. Wright,	Albert H. Tuttle,	H. C. Bolton,	O. T. Mason,	E. W. Morley,	———	W. S. Vaux.*
26th	Aug. 29, 1877.	Nashville, Tenn.,	S. Newcomb,	R. H. Thurston,[1]	O. C. Marsh,	N. T. Lupton,	Daniel Wilson,†	R. H. Ward,	———	F. W. Putnam,	Aug. R. Grote,	H. C. Bolton,	Wm. H. Dall,	P. Schweitzer,	———[2]	T. O. Summers, jr.,	———	W. S. Vaux.†*
27th	Aug. 21, 1878.	St. Louis, Mo.,	O. C. Marsh,	R. H. Thurston,	Aug. R. Grote,	F. W. Clarke,	———[2]	R. H. Ward,[3]	———	F. W. Putnam,	H. C. Bolton,	F. E. Nipher,	George Little,	A. P. S. Stuart,	———[2]	G. J. Engelmann,	———	W. S. Vaux.†*
28th	Aug. 27, 1879.	Saratoga, N. Y.,	G. F. Barker,	S. P. Langley,	J. W. Powell,	F. W. Clarke,[4]	Daniel Wilson,	E. W. Morley,	———	F. W. Putnam,	H. C. Bolton,[5]	J. K. Rees,	W. H. Dall,[6]	W. R. Nichols,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.†*
29th	Aug. 25, 1880.	Boston, Mass.,	L. H. Morgan,*	Asaph Hall,	Alex. Agassiz,	J. M. Ordway,	J. W. Powell,	S. A. Lattimore,	———	F. W. Putnam,	J. K. Rees,	H. B. Mason,	C. V. Riley,	C. E. Munroe,	J. G. Henderson,	A. B. Hervey,	———	W. S. Vaux.*
30th	Aug. 17, 1881.	Cincinnati, Ohio,	G. J. Brush,	Wm. Harkness,[7]	E. T. Cox,[8]	G. C. Caldwell,[9]	G. Mallery,	A. B. Hervey,	J. G. Morris.	F. W. Putnam,	C. V. Riley,	E. T. Tappan,[10]	Wm. Saunders,	A. Springer,[12]	J. G. Henderson,	W. H. Seaman,[11]	B. P. Mann,	W. S. Vaux.†*

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[1] In the absence of E. C. Pickering.

[4] In the absence of Ira Remsen.

[7] In the absence of A. M. Mayer.

[10] In the absence of John Trowbridge.

* Deceased.

[2] The Subsection united with Sec. B.

[5] In the absence of George Little.

[8] In the absence of George Engelmann.

[11] In the absence of S. P. Sharples.

† Not present.

[3] In the absence of G. S. Blackie.

[6] In the absence of A. C. Wetherby.

[9] In the absence of W. R. Nichols.

[12] In place of H. W. Wiley, called away.

MEETINGS AND OFFICERS OF THE ASSOCIATION. (*Continued.*)

MEETING.	DATE.	PLACE.	PRESIDENT.	VICE PRESIDENTS.				
				Section A.	Section B.	Section C.	Section D.	Section E.
31st	Aug. 23, 1882.	Montreal, Can.	J. W. Dawson,	W. A. Rogers,[1]	T.C. Mendenhall,	H. C. Bolton,	W.P.Trowbridge,	E. T. Cox,
32nd	Aug. 15, 1883.	Minneapolis, M.	C. A. Young,	W. A. Rogers,	H.A.Rowland,	E. W. Morley,	De Volsen Wood,[7]	C.H.Hitchcock,

VICE PRESIDENTS.				Permanent Secretary.	General Secretary.	Assistant Gen. Secretary.	SECRETARIES OF SECTIONS.	
Section F.	Section G.	Section H.	Section I.				Section A.	Section B.
W. H. Dall,	A. H. Tuttle,	Alex. Winchell,[2]	E. B. Elliott,	F. W. Putnam,	Wm. Saunders,	J. R. Eastman,	H. T. Eddy,	C. S. Hastings,
W. J. Beal,	J. D. Cox,	O. T. Mason,	F. B. Hough,	F. W. Putnam,	J. R. Eastman,	Alfred Springer,	G. W. Hough,[8]	F. E. Nipher,[9]

SECRETARIES OF THE SECTIONS.							Treasurer.
Section C.	Section D.	Section E.	Section F.	Section G.	Section H.	Section I.	
Alfred Springer,	J. B. Webb,[3]	H. S. Williams,[4]	Wm. Osler,[5]	Robt. Brown, Jr.,	O. T. Mason,	F. B. Hough,[6]	——————*
J. W. Langley,[10]	J. B. Webb,[11]	A. A. Julien,	S. A. Forbes,	Carl Seiler,	G. H. Perkins,	Jos. Cummings,	William Lilly.

[1] In the absence of William Harkness.

[2] In the absence of Daniel Wilson.

[3] In the absence of C. B. Dudley.

[4] In the absence of C. E. Dutton.

[5] In the absence of C. S. Minot.

[6] F. B. Hough was called home and J. R. Dodge acted at the close of the meeting.

[7] Absent but place was not filled.

*Deceased.

[8] In the absence of W. W. Johnson.

[9] In the absence of C. K. Wead.

[10] W. M. McMurtrie, last three days of meeting.

[11] Pro tempore.

COMMONWEALTH OF MASSACHUSETTS.

THE YEAR ONE THOUSAND EIGHT HUNDRED AND SEVENTY-FOUR.

AN ACT

To Incorporate the "American Association for the Advancement of Science."

*it enacted by the Senate and House of Representatives, in General Court
assembled, and by the authority of the same, as follows:*

ECTION 1. Joseph Henry of Washington, Benjamin Pierce of Cam-
lge, James D. Dana of New Haven, James Hall of Albany, Alexis
well of Providence, Stephen Alexander of Princeton, Isaac Lea of
ladelphia, F. A. P. Barnard of New York, John S. Newberry of Cleve-
l, B. A. Gould of Cambridge, T. Sterry Hunt of Boston, Asa Gray of
nbridge, J. Lawrence Smith of Louisville, Joseph Lovering of Cam-
lge and John LeConte of Philadelphia, their associates, the officers
members of the Association, known as the "American Association
the Advancement of Science," and their successors, are hereby made
orporation by the name of the "American Association for the
ancement of Science," for the purpose of receiving, purchasing,
ling and conveying real and personal property, which it now is, or
eafter may be, possessed of, with all the powers and privileges, and
ject to the restrictions, duties and liabilities set forth in the general
s which now or hereafter may be in force and applicable to such cor-
ations.

ECTION 2. Said corporation may have and hold by purchase, grant,
or otherwise, real estate not exceeding one hundred thousand
ars in value, and personal estate of the value of two hundred and
thousand dollars.

ECTION 3. Any two of the corporators above named are hereby
horized to call the first meeting of the said corporation in the month
ugust next ensuing, by notice thereof "by mail," to each member of
said Association.

ECTION 4. This act shall take effect upon its passage.

House of Representatives, March 10, 1874.
Passed to be enacted,
John E. Sanford, *Speaker.*

Senate, March 17, 1874.
Passed to be enacted,
Geo. B. Loring, *President.*

March 19, 1874.
Approved,
W. B. Washburn.

ecretary's Department,
Boston, April 3, 1874.
A true copy, Attest:
David Pulsifer,
Deputy Secretary of the Commonwealth.

CONSTITUTION

OF THE

RICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

porated by Act of the General Court of the Commonwealth of Massachusetts.

OBJECTS.

ICLE 1. The objects of the Association are, by periodical and mi-
y meetings, to promote intercourse between those who are culti-
science in different parts of America, to give a stronger and more
al impulse and more systematic direction to scientific research, and
cure for the labors of scientific men increased facilities and a wider
ness.

MEMBERS, FELLOWS, PATRONS AND HONORARY FELLOWS.

. 2. The Association shall consist of Members, Fellows, Patrons,
onorary Fellows.

. 3. Any person may become a Member of the Association upon
mendation in writing by two members or fellows, nomination by
anding Committee, and election by a majority of the members and
s present in General Session.

. 4. Fellows shall be nominated by the Standing Committee from
f the members as are professionally engaged in science, or have by
abors aided in advancing science. The election of fellows shall be
lot and a majority vote of the members and fellows present in Gen-
ession.

. 5. Any person paying to the Association the sum of one thousand
s shall be classed as a Patron, and shall be entitled to all the
eges of a member and to all its publications.

. 6. Honorary Fellows of the Association, not exceeding three for
ection, may be elected; the nominations to be made by the Stand-
ommittee and approved by ballot in the respective sections before
on by ballot in General Session. Honorary Fellows shall be entitled
the privileges of fellows and shall be exempt from all fees and as-
ents, and entitled to all publications of the Association issued after
te of their election.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness, at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected. The Standing Committee shall have power to exclude from the Association any member or fellow, on satisfactory evidence that said member or fellow is an improper person to be connected with the Association, or has in the estimation of the Committee made improper use of his membership or fellowship.

ART. 8. No member or fellow shall take part in the organization of, or hold office in, more than one section at any one meeting.

OFFICERS.

ART. 9. The Officers of the Association shall be elected by ballot in General Session from the fellows, and shall consist of a President, a Vice President from each section, a Permanent Secretary, a General Secretary, an Assistant General Secretary, a Treasurer, and a Secretary of each Section; these, with the exception of the Permanent Secretary, shall be elected at each meeting for the following one, and, with the exception of the Treasurer and the Permanent Secretary, shall not be reëligible for the next two meetings. The term of office of Permanent Secretary shall be five years.

ART. 10. The President, or, in his absence, the senior Vice President present, shall preside at all General Sessions of the Association and at all meetings of the Standing Committee. It shall also be the duty of the President to give an address at a General Session of the Association at the meeting following that over which he presided.

ART. 11. The Vice Presidents shall be the chairmen of their respective Sections, and of their Sectional Committees, and it shall be part of their duty to give an address, each before his own section, at such time as the Standing Committee shall determine. The Vice Presidents may appoint temporary chairmen to preside over the sessions of their sections, but shall not delegate their other duties. The Vice Presidents shall have seniority in order of their continuous membership in the Association.

ART. 12. The General Secretary shall be the Secretary of all General Sessions of the Association, and shall keep a record of the business of these sessions. He shall receive the records from the Secretaries of the

ections, which, after examination, he shall transmit with his own records
the Permanent Secretary within two weeks after the adjournment of
e meeting. He shall receive proposals for membership and bring them
efore the Standing Committee.

ART. 13. The Assistant General Secretary shall be the Secretary of
e Standing Committee. He shall give to the Secretary of each Section
e titles of papers assigned to it by the Standing Committee.

ART. 14. The Permanent Secretary shall be the executive officer of the
ssociation under the direction of the Standing Committee. He shall
tend to all business not specially referred to committees nor otherwise
onstitutionally provided for. He shall keep an account of all business
at he has transacted for the Association, and make annually a general
port for publication in the annual volume of Proceedings. He shall
tend to the printing and distribution of the annual volume of Pro-
eedings, and all other printing ordered by the Association. He shall
sue a circular of information to members and fellows at least three
onths before each meeting, and shall, in connection with the Local
ommittee, make all necessary arrangements for the meetings of the
ssociation. He shall provide the Secretaries of the Association with
ch books and stationery as may be required for their records and busi-
ess, and shall provide members and fellows with such blank forms as
ay be required for facilitating the business of the Association. He shall
ollect all assessments and admission fees, and notify members and fellows
their election, and of any arrearages. He shall receive, and bring
efore the Standing Committee, the titles and abstracts of papers pro-
osed to be read before the Association. He shall keep an account of all
eceipts and expenditures of the Association, and report the same annually
the first meeting of the Standing Committee, and shall pay over to the
reasurer such unexpended funds as the Standing Committee may direct.
e shall receive and hold in trust for the Association all books, pamphlets
nd manuscripts belonging to the Association, and allow the use of the
ame under the provisions of the Constitution and the orders of the
tanding Committee. He shall receive all communications addressed to
e Association during the interval between meetings, and properly attend
the same. He shall at each meeting report the names of fellows and
embers who have died since the preceding meeting. He shall be allowed
salary which shall be determined by the Standing Committee, and may
mploy one or more clerks at such compensation as may be agreed upon
the Standing Committee.

ART. 15. The Treasurer shall invest the funds received by him in such securities as may be directed by the Standing Committee. He shall annually present to the Standing Committee an account of the funds in his charge. No expenditure of the principal in the hands of the Treasurer shall be made without a unanimous vote of the Standing Committee, and no expenditure of the income received by the Treasurer shall be made without a two-thirds vote of the Standing Committee.

ART. 16. The Secretaries of the Sections shall keep the records of their respective sections, and, at the close of the meeting, give the same, including the records of subsections, to the General Secretary. They shall also be the Secretaries of the Sectional Committees. The Secretaries shall have seniority in order of their continuous membership in the Association.

ART. 17. In case of a vacancy in the office of the President, one of the Vice Presidents shall be elected by the Standing Committee as the President of the meeting. Vacancies in the offices of Vice President, Permanent Secretary, General Secretary, Assistant General Secretary, and Treasurer, shall be filled by nomination of the Standing Committee and election by ballot in General Session. A vacancy in the office of Secretary of a Section shall be filled by nomination and election by ballot in the Section.

ART. 18. The Standing Committee shall consist of the past Presidents, and the Vice Presidents of the last meeting, together with the President, the Vice Presidents, the Permanent Secretary, the General Secretary, the Assistant General Secretary, the Secretaries of the Sections, and the Treasurer of the current meeting, with the addition of one fellow elected from each Section by ballot on the first day of its meeting. The members present at any regularly called meeting of the Committee, provided there are at least five, shall form a quorum for the transaction of business. The Standing Committee shall meet on the day preceding each annual meeting of the Association, and arrange the programme for the first day of the sessions. The time and place of this first meeting shall be designated by the Permanent Secretary. Unless otherwise agreed upon, regular meetings of the Committee shall be held in the committee room at 9 o'clock, A. M., on each day of the meeting of the Association. Special meetings of the Committee may be called at any time by the President. The Standing Committee shall be the board of supervision of the Association, and no business shall be transacted by the Association that has not first been referred to, or originated with, the Committee. The Committee shall receive and assign papers to the respective sections; examine and, if necessary, exclude papers; decide which papers, discussions and other

ceedings shall be published, and have the general direction of the
ilications of the Association; manage the financial affairs of the Asso-
tion; arrange the business and programmes for General Sessions;
gest subjects for discussion, investigation or reports; nominate mem-
s and fellows; and receive and act upon all invitations extended to the
sociation and report the same at a General Session of the Association.

ART. 19. The Nominating Committee shall consist of the Standing
mmittee, and one member or fellow elected by each of the Sec-
ns. It shall be the duty of this Committee to meet at the call of
President and nominate the general officers for the following meeting
the Association. It shall also be the duty of this Committee to rec-
mend the time and place for the next meeting. The Vice President
l Secretary of each Section shall be recommended to the Nominating
mmittee by a sub-committee consisting of the Vice President, Sec-
ary, and three members or fellows elected by the Section.

MEETINGS.

ART. 20. The Association shall hold a public meeting annually, for one
ek or longer, at such time and place as may be determined by vote of the
sociation, and the preliminary arrangements for each meeting shall be
de by the Local Committee, in conjunction with the Permanent Secre-
y and such other persons as the Standing Committee may designate.

ART. 21. General Sessions shall be held at 10 o'clock, A. M., unless
erwise ordered, on every day of the meeting, Sunday excepted, and
such other times as may be appointed by the Standing Committee.

SECTIONS AND SUBSECTIONS.

ART. 22. The Association shall be divided into nine Sections, namely:—
Mathematics and Astronomy; B, *Physics*; C, *Chemistry, including its*
plication to agriculture and the arts; D, *Mechanical Science*; E, *Geology*
d Geography; F, *Biology*; G, *Histology and Microscopy*; H, *Anthro-*
logy; I, *Economic Science and Statistics*. The Standing Committee
all have power to consolidate any two or more Sections temporarily, and
ch consolidated Sections shall be presided over by the senior Vice
esident and Secretary of the Sections comprising it.

ART. 23. Immediately on the organization of a Section there shall be
ree fellows elected by ballot after open nomination, who, with the Vice
esident and Secretary, shall form its Sectional Committee. The Sec-
nal Committees shall have power to fill vacancies in their own numbers.
eetings of the Sections shall not be held at the same time with a General
ssion.

ART. 24. The Sectional Committee of any Section may at its pleasure form one or more temporary Subsections, and may designate the officers thereof. The Secretary of a Subsection shall, at the close of the meeting, transmit his records to the Secretary of the Section.

ART. 25. A paper shall not be read in any Section nor Subsection until it has been received from the Standing Committee and placed on the programme of the day by the Sectional Committee.

SECTIONAL COMMITTEES.

ART. 26. The Sectional Committees shall arrange and direct the business of their respective Sections. They shall prepare the daily programmes and give them to the Permanent Secretary for printing at the earliest moment practicable. No titles of papers shall be entered on the daily programmes except such as have passed the Standing Committee. No change shall be made in the programme for the day in a Section without the consent of the Sectional Committee. The Sectional Committees may refuse to place the title of any paper on the programme; but every such title, with the abstract of the paper or the paper itself, must be returned to the Standing Committee with the reasons why it was refused.

ART. 27. The Sectional Committees shall examine all papers and abstracts referred to the sections, and they shall not place on the programme any paper inconsistent with the character of the Association; and to this end they have power to call for any paper, the character of which may not be sufficiently understood from the abstract submitted.

PAPERS AND COMMUNICATIONS.

ART. 28. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

ART. 29. If the author of any paper be not ready at the time assigned, the title may be dropped to the bottom of the list.

ART. 30. Whenever practicable, the proceedings and discussions at General Sessions, Sections and Subsections shall be reported by professional reporters, but such reports shall not appear in print as the official reports of the Association unless revised by the Secretaries.

PRINTED PROCEEDINGS.

RT. 31. The Permanent Secretary shall have the Proceedings of each
eting printed in an octavo volume as soon after the meeting as possible,
ginning one month after adjournment. Authors must prepare their
pers or abstracts ready for the press and forward them to the Perma-
nt Secretary within this interval, otherwise only the abstracts or titles
ken from the "title blanks" will appear in the printed volume. The
anding Committee shall have power to order the printing of any paper
abstract or title only. Whenever practicable, proofs shall be for-
rded to authors for revision. If any additions or substantial altera-
ns are made by the author of a paper after its submission to the Secre-
y, the same shall be distinctly indicated. Illustrations must be pro-
led for by the authors of the papers, or by a special appropriation from the
anding Committee. Immediately on publication of the volume, a copy
all be forwarded to every member and fellow of the Association who shall
ve paid the assessment for the meeting to which it relates, and it shall
o be offered for sale by the Permanent Secretary at such price as may
determined by the Standing Committee. The Standing Committee shall
o designate the institutions to which copies shall be distributed.

LOCAL COMMITTEE.

ART. 32. The Local Committee shall consist of persons interested in
e objects of the Association and residing at or near the place of the
oposed meeting. It is expected that the Local Committee, assisted by
e officers of the Association, will make all essential arrangements for
e meeting, and issue a circular giving necessary particulars, at least
e month before the meeting.

LIBRARY OF THE ASSOCIATION.

ART. 33. All books and pamphlets received by the Association shall be
the charge of the Permanent Secretary, who shall have a list of the
me printed and shall furnish a copy to any member or fellow on appli-
tion. Members and fellows who have paid their assessments in full
all be allowed to call for books and pamphlets, which shall be delivered
them at their expense, on their giving a receipt agreeing to make
od any loss or damage and to return the same free of expense to the
cretary at the time specified in the receipt given. All books and pam-
lets in circulation must be returned at each meeting. Not more than
e books, including volumes, parts of volumes, and pamphlets, shall be
ld at one time by any member or fellow. Any book may be withheld
m circulation by order of the Standing Committee.

ADMISSION FEE AND ASSESSMENTS.

ART. 34. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 35. The annual assessment for members and fellows shall be three dollars.

ART. 36. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research, unless otherwise directed by unanimous vote of the Standing Committee.

ART. 37. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ACCOUNTS.

ART. 38. The accounts of the Permanent Secretary and of the Treasurer shall be audited annually, by Auditors appointed by the Standing Committee.

ALTERATIONS OF THE CONSTITUTION.

ART. 39. No part of this Constitution shall be amended or annulled, without the concurrence of three-fourths of the members and fellows present in General Session, after notice given at a General Session of a preceding meeting of the Association.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

1. The retiring President introduces the President elect, who takes the chair.
2. Formalities of welcome of the Association as may be arranged by the Local Committee.
3. Report of the list of papers entered and their reference to the Sections.
4. Other reports.
5. Announcements of arrangements by the Local Committee.
6. Election of members.
7. Election of fellows.
8. Unenumerated business.
9. Adjournment to meet in Sections.

This order, so far as applicable, to be followed in subsequent General Sessions.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.[1]

PATRONS.[2]

THOMPSON, MRS. ELIZABETH, Stamford, Conn. (22).
LILLY, GEN. WILLIAM, Mauch Chunk, Carbon Co., Pa. (28).
HERRMAN, MRS. ESTHER, 59 West 56th St., New York, N. Y. (29).

MEMBERS.[3]

Abbot, S. L., M. D., 90 Mt. Vernon St., Boston, Mass. (29).
Abbott, E. K., M. D., Salinas, Monterey Co., Cal. (29).
Abert, Col. James W., Newport, Ky. (31).
Abert, S. Thayer, 1907 Pennsylvania Avenue, Washington, D. C. (30).
Acker, E. O'C., Ass't Eng'r, U. S. N., Norristown, Pa. (28).
Adams, Frank, 41 McGill College Avenue, Montreal, Can. (29).
Adams, John S., M. D., Oakland, Cal. (29).
Adams, Julius W., 155 Congress St., Brooklyn, N. Y. (29).
Adams, Capt. Robert C., Montreal, Can. (31).
Agard, Dr. A. H., 1259 Alice St., Oakland, Alameda Co., Cal. (28).
Agnew, Dr. Cornelius R., 266 Madison Ave., New York, N. Y. (32).
Ainslie, Charles N., Rochester, Minn. (32).
Allen, Edmund T., 404 Market St., St. Louis, Mo. (27).

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[1] The numbers in parentheses indicate the meeting at which the member was elected. The Constitution requires that the names of all members two or more years in arrears shall be omitted from the list, but their names will be restored on payment of arrearages. Members not in arrears are entitled to the annual volume of Proceedings bound in paper. *The payment of ten dollars at one time entitles a member to the subsequent volumes bound in cloth, or by the payment of twenty dollars, to the volumes bound in half morocco.*

[2] Persons contributing one thousand dollars or more to the Association are classed as Patrons, and are entitled to the privileges of members and to the publications. The names of Patrons are to remain permanently on the list.

[3] Any Member or Fellow may become a Life Member by the payment of fifty dollars. The money derived from Life Memberships is invested as a fund, the income of which is to be used only to aid in original research. Life Members are exempt from the annual assessment, and are entitled to the annual volume. The names of Life Members are printed in small capitals in the regular list of Members and Fellows.

Allen, Dr. G. M., 102 Clark St., Cincinnati, Ohio (30).
Allen, J. M., Hartford, Conn. (22).
Alling, J. C., Supt. City Schools, Albert Lea, Minn. (31).
Alling, Miss Mary R., Acra, Greene Co. N. Y. (29).
Allis, Solon M., Tucson, Arizona (29).
Allison, Henry, Fort Worth, Texas (32).
Ammen, Daniel, Rear Admiral U. S. Navy, Washington, D. C. (26).
Anderson, Newton M., Columbus, Ohio (30).
Andrews, L. W., Ph. D., Springfield, Mass. (31).
Angus, William, 214 St. James St., Montreal, Can. (31).
Ansley, Clark F., Swedona, Mercer Co., Ill. (32).
Appleton, Rev. Edw. W., D.D., Ashbourne P. O., Montgomery Co., Pa. (28).
Apthorp, Henry, Ashtabula, Ohio (32).
Archambault, U. E., P. O. Box 1944, Montreal, Can. (31).
Armstrong, Lucius H., St. Nicholas, Duval Co., Fla. (30).
Armstrong, Mrs. Lucius H., St. Nicholas, Duval Co., Fla. (30).
Arnold, Lauren B., Rochester, N. Y. (30).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Ashmead, Wm. H., Jacksonville, Fla. (29).
Atkinson, Jno. B., Earlington, Hopkins Co., Ky. (26).
Atwater, Richard M., Millville, N. J. (29).
Atwood, E. S., East Orange, N. J. (29).
Atwood, Oscar, Rutland, Vt. (31).
Auchincloss, Wm. S., 209 Church St., Philadelphia, Pa. (29).
Aughey, Prof. Samuel, Univ. of Nebraska, Lincoln, Neb. (29).
Austin, Miss Mary, Mattapan Station, Milton, Mass. (31).
Ayres, Prof. Brown, Univ. of Louisiana, New Orleans, La. (31).
Ayres, Dr. S. C., 64 W. 7th St., Cincinnati, Ohio (30).

Babbit, John, Fredericton, N. B. (29). [(32).
Babbitt, Miss Franc E., P. O. Box 75, Little Falls, Morrison Co., Minn.
Bailey, E. H. S., Bethlehem, Pa. (25).
Baker, A. L., 32 So. Charles St., Baltimore, Md. (30).
Baker, B. N., 32 So. Charles St., Baltimore, Md. (30).
Baker, Frank, M. D., 326 C St., Washington, D. C. (31).
Baker, Prof. I. O., Illinois Industrial Univ., Champaign, Ill. (30).
Baker, Prof. T. R., Millersville, Lancaster Co., Pa. (22).
Baker, Wm. E., 158 Tremont St., Boston, Mass. (29). [(31).
Baldwin, Miss Mary A., Summer Ave., Cor. Kearney St., Newark, N. J
Baldwin, Very Rev. Dean Maurice S., Montreal, Can. (31).
Balen, Abraham D., Plainfield, N. J. (31).
Ballard, H. H., Lenox, Mass. (31).
Ballou, Nahum E., M. D., Sandwich, Ill. (29).
Ballou, Wm. H., 105 Dearborn Avenue, Chicago, Ill. (29).
Banes, Charles H., 2416 Spring Garden St., Philadelphia, Pa. (31).
Baptie, George, Normal School, Ottawa, Ontario, Can. (29).

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Bardeen Francis L., M. D., Washington College, Chestertown, Md. (32).
Barker, Mrs. Martha M., care of H. R. Barker, 8 and 10 Central St., Lowell, Mass. (31).
Barnard, Edward E., care R. Poole, Cherry and Union Sts., Nashville, Tenn. (26).
Barnard, Jas. M., Hotel Vendôme, Boston, Mass. (18).
Barnes, Charles J., Chicago, Ill. (27).
Barrows, Nathan, M. D., Hartford, Conn. (29).
Bartlett, Arthur K., Battle Creek, Mich. (29).
Bartlett, Dr. Homer L., Flatbush, L. I. (28).
Bassett, Daniel A., Crawfordsville, Ind. (29).
Bassett, George W., M.D., Vandalia, Ill. (20).
BASSNETT, THOMAS, Box 936, Jacksonville, Fla. (8).
Bassnett, Mrs. Thomas, Box 936, Jacksonville, Fla. (24).
Bastian, Dr. David I., Clinton, Worcester Co., Mass. (25).
Bastin, Prof. E. S., Univ. of Chicago, Chicago, Ill. (29).
Batchelor, Charles, Electrician, Menlo Park, N. J. (28).
Batterson, J. G., Hartford, Conn. (23).
Bausch, Edward, Rochester, N. Y. (26).
Bayne, Prof. Herbert A., Royal Military College, Kingston, Ont. (29).
Beach, Charles M., Merchant, Hartford, Conn. (23).
Beach, J. Watson, Merchant, Hartford, Conn. (23).
Beach, William H., Beloit, Wis. (21).
Beadle, Delos W., St. Catharines, Ontario, Can. (31).
Beal, F. E. L., Agricultural College, Ames, Iowa (29).
Beall, Thomas, Lindsay, Ontario, Can. (31).
Beamer, Miles (30).
Bean, Thos. E., Box 441, Galena, Ill. (28).
Beard, Richard O., M. D., Minneapolis, Minn. (32).
Beaudry, J. Alphonse U., 97 St. James St., Montreal, Can. (31).
Beauregard, Gen. Gustave T., New Orleans, La. (30).
Bechdolt, Adolphus F., Mankato, Minn. (32).
Beckwith, Henry C., Passed Ass't Eng'r U. S. N., Coleman's Station, N. Y. (29).
Beebe, Wm. S., Brevet Major, U. S. Army, 25 Grace Court, Brooklyn, N. Y. (30).
Belknap, Chas., Lieut. U. S. N., U. S. S. Vandalia, care Navy Dept., Washington, D. C. (29).
Belknap, Morris B., Louisville, Ky. (29).
Belknap, Wm. B., Louisville, Ky. (29).
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Bell, John J., Exeter, N. H. (22).
Bell, Thomas D., McGill College, Montreal, Can. (31).
Bemis, Hon. A. S., President of the Common Council, Buffalo, N. Y. (25).
Bemrose, Joseph, 158 P. O., Montreal, Can. (31).
Benjamin, E. B., 6 Barclay St., New York, N. Y. (19).

Benjamin, Marcus, 43 East 67th St., New York, N. Y. (27).
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Best, W. F., St. John, N. B. (29).
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Bicknell, Hon. Thos. W., Boston, Mass. (29).
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Bill, Charles, Springfield, Mass. (17).
Bingham, Mrs. Martha A., Kansas City, Mo. (32).
Birge, Charles P., Keokuk, Iowa (29).
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Blackwell, Mrs. A. B., Somerville, N. J. (30).
Blaisdell, F. E., Poway, San Diego Co., Cal. (29).
Blake, Mrs. Clarence J., 19 St. James Avenue, Boston, Mass. (29).
Blake, Francis C., Penna. Lead Works, Mansfield Valley, Alleghany Co., Pa. (29).
Blake, Prof. John R., Davidson College, N. C. (29).
Blakslee, Prof. T. M., Harpersville, N. Y. (31).
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Blount, Henry F., Evansville, Vanderburg Co., Ind. (32).
Boardman, Miss E. D., 120 Beacon St., Boston, Mass. (29).
Boardman, Mrs. Wm. D., 38 Kenilworth St., Roxbury, Mass. (28).
Bobbitt, Mrs. John, Greensburg, Decatur Co., Ind. (30).
Boerner, Chas. G., Vevay, Ind. (29).
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Bond, Geo. W., Boston, Mass. (29).
Bookwalter, J. W., Springfield, Ohio (30).
Bourland, Dr. Addison M., Van Buren, Ark. (29).
Bowditch, Frederic C., Box 615, Brookline, Mass. (29).
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Bowker, W. H., 43 Chatham St., Boston, Mass. (30).
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Bowles, Miss Margaretta, The Institute, Columbia, Tenn. (26).
Boyd, William, Cambridge, Mass. (29).
Boynton, Miss S. P., Box 150, Lynn, Mass. (19).
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Brackett, Solomon H., St. Johnsbury, Vt. (29).
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Bradford, Royal B., Lt. Comd'r, U. S. N., 21 Brinley St., Newport, R. I. (31).
Bradley, C. W., cor. 3d and Walnut Sts., Cincinnati, Ohio (27).
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Brainerd, Thos. C., P. O. Box 720, Montreal, Can. (31).
Brandegee, T. S., Berlin, Conn. (29).
Branlette, Mrs. T. E., Louisville, Ky. (29).
Branstrup, Dr. W. T., Vincennes, Ind. (30).
Bray, Prof. C. D., College Hill, Mass. (29).
Breckinridge, S. M., Box 2794, St. Louis, Mo. (27).
Brewster, William, 61 Sparks St., Cambridge, Mass. (29).
Brice, Judge Albert G., New Orleans, La. (32).
Brissette, Milton H., Montreal, Can. (31).
Bronnson, W. H., Lt. Comd'r U. S. N., Yonkers, N. Y. (31).
Bronson, E. H., Ottawa, Ontario, Can. (31).
Brooks, Miss Margarette W., Salem, Mass. (29).
Broomall, Hon. John M., Media, Delaware Co., Pa. (23).
Brounell, Prof. Walter A., 723 Irving St., Syracuse, N. Y. (30).
Brown, Miss Anna, 528 West 7th St., Cincinnati, Ohio (31).
Brown, Arthur, C. E., Oakland, Alameda Co., Cal. (31).
Brown, Asa Warren, Newport, Ky. (30).
Brown, Mrs. Ashley, 115 Wilkinson St., Dayton, Ohio (25).
Brown, Prof. C. J., Randolph, N. Y. (31).
Brown, Fred. G., care of McKesson and Robbins, 93 Fulton St., New York, N. Y. (31).
Brown, Hon. John C., Vice President, Texas and Pacific R. R. Co., Pulaski, Tenn. (26).
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Buck, C. Elton, Wilmington, Del. (29).
Buck, Henry C., West Somerville, Mass. (29).
Buckingham, Chas. L., U. S. Patent Office, Washington, D. C. (28).
Buffum, Miss Fannie A., Linden, Mass. (29).
Bull, John, Room 30, Corcoran Building, Washington, D. C. (31).
Bulloch, Walter H., 99 W. Monro St., Chicago, Ill. (30).
Burdick, Edson A., 408 Spruce St., Le Droit Park, Washington, D. C (30).
Burke, Jos. C., Middletown, Conn. (29).
Burke, William, U. S. Patent Office, Washington, D. C. (28).
Burland, Jeffrey H., Montreal, Can. (31).
Burns, Prof. James A., Box 456, Atlanta, Ga. (32).
Burpee, J. P. C., St. John, N. B. (30).
Bush, Rev. Stephen, D. D., Waterford, N. Y. (19).
Butcher, Mrs. Z. D., Washington, D. C. (29).
Butler, A. W., Brookville, Franklin Co., Ind. (30).
Butler, Cyrus, 24 Cliff St., New York, N. Y. (29).

Butler, Nathan, 17 Johnston Block, Minneapolis, Minn. (32).
Butterfield, W. Webster, M. D., Indianapolis, Ind. (32).
Bynner, Edwin L., 13 Pemberton Square, Boston, Mass. (29).
Byrnes, R. M., M. D., cor. 5th and Race Sts., Cincinnati, Ohio (30).

Cabot, Sam'l, jr., 53 Kilby St., Boston, Mass. (29).
Calder, Edwin E., Box 17, Providence, R. I. (29).
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Caldwell, Dr. Frank, Cincinnati Hospital, Cincinnati, Ohio (30).
Calkins, Dr. Marshall, Springfield, Mass. (29).
Campbell, Rev. Prof. John, Presbyterian College, Montreal, Can. (31).
Canfield, Rev. Eli H., D.D., Arlington, Bennington Co., Vt. (25).
Capen, Miss Bessie T., Northampton, Mass. (23).
Cargill, H. L., St. Lawrence Hotel, Montreal, Can. (31).
Carman, Ezra A., Dept. of Agric., Washington, D. C. (30).
Carman, Lewis, Bangall, N. Y. (29).
Caron, C. K., Louisville, Ky. (30).
Carpenter, Geo. O., jr., care St. Louis Lead & Oil Co., St. Louis, Mo. (29).
Carpenter, Louis G., Lansing, Mich. (32).
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31).
Carrington, Col. Henry B., U. S. A., care Adj. Gen. U. S. A., Washington, D.C. (20).
Case, Mrs. Fidelia O., Kansas City, Mo. (32).
Cassino, S. E., Naturalists' Agency, 41 Arch St., Boston, Mass. (25).
Castle, Frederick A., M. D., 102 East 57th St., New York, N. Y. (29).
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Chace, Arnold B., Valley Falls, R. I. (29).
Chadbourn, Erlon R., Lewiston, Me. (29).
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Chamberlain, M., St. John, N. B. (32).
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Chapman, F. M., Danforth, Iroquois Co., Ill. (17).
Charbonnier, Prof. L. H., University of Georgia, Athens, Ga. (26).
Chase, Prof. F. A., Fisk University, Nashville, Tenn. (26).
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Chase, Prof. Pliny E., Haverford College, Haverford, Pa. (18).
Chase, R. Stuart, 16 Merrimack St., Haverhill, Mass. (18).
Chatfield, A. F., Albany, N. Y. (29).
Cheesman, Prof. Louis M., Trinity College, Hartford, Conn. (32).
Chenault, Prof. J. W., Louisville, Ky. (30).
Chester, Lt. Com'dr Colby M., U. S. N.,Coast Survey Office, Washington, D. C. (28).
Childe, John Healey, Boston, Mass. (31).
Cisco, J. G., Jackson, Tenn. (29).

rk, John S., 64 Pinckney St., Boston, Mass. (31).
rk, Simeon T., M.D., Lockport, Niagara Co., N. Y. (25).
rke, Miss Cora H., Jamaica Plain, Mass. (29).
rke, Prof. John M., Northampton, Mass. (31).
rke, Maurice D., 68 Ohio St., East Cambridge, Mass. (29).
rke, Robert, Cincinnati, Ohio (30).
veland, J. L., 547 W. 8th St., Cincinnati, Ohio (30).
venger, Dr. S. V., 189 37th St., Chicago, Ill. (29).
ud, Jno. W., Altoona, Pa. (28).
kley, George W., LL.D., Hempstead, L. I. (29).
b, Samuel C., 235 Boylston St., Boston, Mass. (29).
b, Rev. Sanford H., Saugerties, N. Y. (29).
, Henry W., M. D., Mandan, Dacotah (32).
lin, Amory, Phœnixville, Pa. (31).
inberry, W. L., Grand Rapids, Mich. (20).
burn, Richard T., Elizabeth, N. J. (31).
oy, C. E., M. E., School of Mines, Columbia College, New York, N. Y. (28).
e, Frederick, 1394 St. Catherine St., Montreal, Can. (31).
e, Edw. M., East Orange, N. J. (30).
ier, M. Dwight, St. Louis, Mo. (27).
in, Prof. Alonzo, Cornell College, Mount Vernon, Iowa (21).
nan, Henry, M.D., Swampscott, Mass. (25).
on, G. Woolworth, 182 William St., New York, N. Y. (22).
npton, Murat, Bradford, McKean Co., Pa. (32).
nstock, Dr. T. Griswold, 507 North 14th St., St. Louis, Mo. (29).
ger, Rev. Charles L., 1729 Eastern Avenue, Cincinnati, Ohio (30).
klin, W. A., Director Central Park Menagerie, New York, N. Y. (29).
ro, Miss Emma O., Keeseville, N. Y. (31).
stable, Casimir, Constableville, N. Y. (29).
k, Dr. Charles D., 133 Pacific St., Brooklyn, N. Y. (25).
lidge, Herbert, 48 Kilby St., Boston, Mass. (29).
n, Henry C., M. D., Alfred Centre, N. Y. (29).
per, Ed. M., 164 Walnut St., Cincinnati, Ohio (30).
es, Dr. Joseph S., New Orleans, La. (11).
tin, Rev. M. P., St. Jno. College, Fordham, N. Y. (30).
ing, Dr. B. E., Roxbury, Mass. (31).
lson, Miss Mary, Oxford, Butler Co., Ohio (30).
lter, Prof. John M., Crawfordsville, Ind. (32).
zins, Miss Phœbe W., LL.B., 2647 Washington Ave., St. Louis, Mo. (27).
ington, S. F., 68 W. 3d St., Cincinnati, Ohio (30).
an, Richard Wm., 416 Notre Dame St., Montreal, Can. (31).
dry, Thomas, M. D., Toronto, Can. (31).
, Hon. Joseph, Madisonville, Ohio (30).
ts, Robert H., 2329 South 6th St., Minneapolis, Minn. (32).
gin, Francis W., Washburn College, Topeka, Kan. (29).

Crane, C. R., 321 Garrard St., Covington, Ky. (30).
Crane, Henry L., 157 York St., Cincinnati, Ohio (30).
Crawford, John M., cor. 7th and Mound Sts., Cincinnati, Ohio (30).
Crawford, Prof. Morris B., Middletown, Conn. (30).
Crehore, John D., 1046 Prospect St., Cleveland, Ohio (24).
Crevier, I. A., M. D., Montreal, Can. (31).
Crocker, Miss Lucretia, 40 Rutland Square, Boston, Mass. (29).
Crosby, Francis W., 20 Second St., N. E., Washington, D. C. (31).
Crosier, Dr. Edward S., Louisville, Ky. (29).
Crow, Wayman, St. Louis, Mo. (27).
CROWELL, A. F., Woods Holl, Mass. (30).
Crump, Prof. M. H., Ogden College, Bowling Green, Ky. (29).
Cummings, John, Woburn, Mass. (18).
Currier, John McNab, M.D., Castleton, Vt. (28).
Curtis, Elizabeth, 16 Beethoven St., Egleston Square, Boston, Mass. (31)
Curtis, Dr. Lester, 1558 Wabash Avenue, Chicago, Ill. (29).
Curtiss, Abijah, Yonkers, N. Y. (31).
Cutler, Dr. Andrew S., Kankakee, Ill. (32).
Cutter, John D., Box 276, New York, N. Y. (28).
Cutter, W. E., Box 1037, Worcester, Mass. (29).

Dale, Ben B., 25 Wiggins Block, Cincinnati, Ohio (30).
Dale, T. Nelson, jr., Gibbs Ave., Newport, R. I. (27).
Dana, Judge Edmund L., Wilkesbarré, Pa. (32).
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Zinke, E. Gustav, M. D., Elm St., near 14th, Cincinnati, Ohio (30).
Zwick, G. A., 11th and Madison Sts., Covington, Ky. (30).

[1474 MEMBERS.]

NOTE.—The omission of an address in the foregoing list indicates that letters directed to that last printed were returned as uncalled for. Information of the present address of the members so indicated is requested by the PERMANENT SECRETARY.

HONORARY FELLOW.[1]

OGERS, PROF. WILLIAM B., 117 Marlborough St., Boston, Mass. (1). 1881.

FELLOWS.[2]

be, Prof. Cleveland, Washington, D. C. (16). 1874.
bott, Dr. Chas. C., Trenton, N. J. (29). 1883.
exander, John S., 1935 Arch St., Philadelphia, Pa. (20). 1874.
len, Dr. Harrison, 117 South 20th St., Philadelphia, Pa. (29). 1882.
len, Joel A., Mus. Comp. Zoology, Cambridge, Mass. (18). 1875.
ford, Benj., Brig. Gen., U. S. Army, 1207 Q St., N. W., Washington, D. C. (17). 1874.
ford, Henry E., C. E., Houghton Farm, Mountainville, Orange Co., N. Y. (29). 1882.
derson, Dr. Joseph, Waterbury, Conn. (29). 1883.
thony, Prof. Wm. A., Cornell Univ., Ithaca, N. Y. (28). 1880.
pleton, Prof. John H., Brown Univ., Providence, R. I. (22). 1874.
hburner, Wm., 1014 Pine St., San Francisco, Cal. (29). 1882.
kinson, Edward, 131 Devonshire St., Boston, Mass. (29). 1881.
water, Prof. W. O., Wesleyan Univ., Middletown, Conn. (29). 1882.
sten, Peter T., Ph. D., Rutgers College, Lock Box No. 2, New Brunswick, N. J. (26). 1879.
stin, E. P., 46 E. Newton St., Boston, Mass. (18). 1874.

iley, Prof. Loring W., University of Fredericton, N. B. (18). 1875.
iley, Prof. W. W., Brown University, Providence, R. I. (18). 1874.
ird, Prof. S. F., Sec'y Smithsonian Inst., Washington, D. C. (1). 1875.
ker, Marcus, Coast Survey Office, Washington, D. C. (30). 1882.
llou, Geo. F., Hartford, Conn. (29). 1881.
ndelier, Ad. F., Highland, Ill. (25). 1879.
RKER, PROF. G. F., Univ. of Penn., Philadelphia, Pa. (13). 1875.
rnard, F. A. P., President Columbia College, New York, N. Y. (7). 1874.
rnard, Prof. Wm. S., U. S. Dept. Agric., Washington, D. C. (24). 1880.
rtlett, Prof. Edwin J., Dartmouth College, Hanover, N. H. (28). 1883.
rtlett, J. R., Comd'r U. S. N, care of C. & G. Survey Office, Washington, D. C. (30). 1882.
ssett, Homer F., Waterbury, Conn. (23). 1874.
tchelder, Dr. J. H., Salem, Mass. (18). 1874.
tchelder, John M., 3 Divinity Avenue, Cambridge, Mass. (8). 1875.
al, Prof. Wm. James, Agricultural College, Lansing, Mich. (24). 1880.

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[1] See ARTICLE VI of the Constitution. [2] See ARTICLE IV of the Constitution.

, The number in parenthesis indicates the meeting at which the member joined Association; the date at the end of the line the time when made a Fellow.

When the name is given in small capitals, it designates that the Fellow is also a member, and is entitled to the Annual Volume of Proceedings.

Deceased.

Belknap, George E., Capt. U. S. N., care Fred. L. Wheeler & Co., Box 3522, Boston, Mass. (29). 1881.
Bell, Dr. Alex. Graham, Scott Circle, 1500 Rhode Island Ave., Washington, D. C. (26). 1879.
Bell, Samuel N., Manchester, N. H. (7). 1874.
Bessey, Prof. C. E., Agricultural College, Ames, Iowa (21). 1880.
Bethune, Rev. C. J. S., Trinity Coll. School, Pt. Hope, Canada (18). 1875.
Bickmore, Prof. Albert S., American Museum of Natural History, Central Park, New York, N. Y. (17). 1880.
Billings, John S., Surgeon U. S. A., Washington, D. C. (32). 1883.
Blackham, George E., M. D., Dunkirk, N. Y. (25). 1883. [1882.
Blair, Henry W., U. S. C. & G. Survey Office, Washington, D. C. (26).
Blake, Clarence J., M. D., Hotel Berkeley, Boston, Mass. (24). 1877.
Blake, Eli W., New Haven, Conn. (1). 1874.
Blake, Prof. Eli W., jr., Providence, R. I. (15). 1874.
Blake, Francis, Auburndale, Mass. (23). 1874.
BOLTON, DR. H. CARRINGTON, Trinity Coll., Hartford, Conn. (17). 1875.
Borden, Spencer, Fall River, Mass. (29). 1882.
Boss, Prof. Lewis, Director Dudley Observ., Albany, N. Y. (26). 1878.
Bouvé, Thos. T., Boston Soc. Nat. Hist., Boston, Mass. (1). 1875.
Bowditch, Prof. H. P., Harvard Med. School, Boston, Mass. (28). 1880.
Bowditch, Henry I., M.D., 113 Boylston St., Boston, Mass. (2). 1875.
Bowser, Prof. E. A., Rutgers College, New Brunswick, N. J. (28). 1881.
Brackett, Prof. C. F., College of New Jersey, Princeton, N. J. (19). 1875.
Breneman, A. A., Ithaca, N. Y. (20). 1880.
Brewer, Prof. Wm. H., New Haven, Conn. (20). 1875.
Britton, N. L., Columbia College, New York, N. Y. (29). 1882.
Broadhead, Garland Carr, Pleasant Hill, Cass Co., Mo. (27). 1879.
Brooks, Major Thomas B., Geol. State Surveys, Mich. and Wis., Newburgh, N. Y. (26). 1879.
Bross, Hon. Wm., Chicago, Ill. (7). 1874.
Broun, Prof. W. LeRoy, Vanderbilt Univ., Nashville, Tenn. (25). 1877.
Brown, Robert, jr., care of Yale College Observatory, New Haven, Conn (11). 1874.
Brown, Mrs. Robert, jr., New Haven, Conn. (17). 1874.
BRUSH, PROF. GEORGE J., Yale College, New Haven, Conn. (4). 1874.
Buckhout, W. A., State College, Centre Co., Pa. (20). 1881.
Burgess, Edward, Sec. Natural History Soc., Boston, Mass. (22). 1877.
Burnham, S. W., 52 Vincennes Ave., Chicago, Ill. (25). 1877.
Burr, Prof. William H., Troy, N. Y. (31). 1883.
Burrill, Prof. T. J., Illinois Industrial Univ., Champaign, Ill. (29). 1882

Caldwell, Prof. Geo. C., Cornell University, Ithaca, N. Y. (23). 1875.
Call, Prof. R. Ellsworth, David City, Neb. (29). 1882.
Canby, Wm. M., 1101 Delaware Avenue, Wilmington, Del. (17). 1878.
Carhart, Prof. Henry S., Evanston, Ill. (29). 1881.
Carmichael, Prof. Henry, Bowdoin College, Brunswick, Me. (21). 1875.

Carpenter, Lieut. W. L., U. S. A., Dunkirk, N. Y. (24). 1877.
Carr, Lucien, Peabody Mus. Arch. and Ethn., Cambridge, Mass. (25). 1877.
Case, Col. Theo. S., Ed. Western Review of Science, Kansas City, Mo. (27). 1883. [1882.
Chamberlain, Wm. I., Sec. Ohio State Bd. of Agric., Columbus, Ohio (30).
Chamberlin, T. C., Beloit College, Beloit, Wis. (21). 1877.
Chandler, Prof. C. F., School of Mines, Columbia Coll., 50th St. cor. 4th Ave., New York, N. Y. (19). 1875. [1882.
Chandler, Prof. Charles Henry, Ripon, Wis. (28). 1883.
Chandler, Seth C., jr., Harvard Observatory, Cambridge, Mass. (29).
Chandler, Prof. W. H., The Lehigh Univ., Bethlehem, Pa. (19). 1874.
Chanute, O., care Erie Railway, New York, N. Y. (17). 1877.
Chesbrough, E. S., Chicago, Ill. (2). 1877.
Chester, Prof. Albert H., Hamilton College, Clinton, N. Y. (29). 1882.
Chickering, Prof. J. W., jr., Deaf Mute College, Washington, D. C. (22). 1877.
Chittenden, Russell H., Ph. D., 95 Humphrey St., New Haven, Conn. (29). 1882.
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Mass. (31). 1883.
Clark, Alvan G., Cambridgeport, Mass. (28). 1880. [1875.
Clark, Prof. John E., Mathematics, Yale College, New Haven, Conn. (17).
Clarke, Prof. F. W., U. S. Geological Survey, Washington, D. C. (18). 1874.
Claypole, Edw. W., B. A., New Bloomfield, Perry Co., Pa. (30). 1882.
Coffin, Prof. John H. C., U. S. Navy, Washington, D. C. (1). 1874.
Coffin, Prof. Selden J., Ph.D., Lafayette College, Easton, Pa. (22). 1874.
Collett, Prof. John, Chief Indiana Bureau of Statistics of Geology, Indianapolis, Ind. (17). 1874.
Collier, Peter, Dept. of Agric., Washington, D. C. (29). 1882.
Colvin, Verplanck, Supt. N. Y. State Adirondack Survey, Albany, N. Y. (28). 1880.
Comstock, J. Henry, Cornell Univ., Ithaca, N. Y. (28). 1882.
Comstock, Milton L., Prof. Math., Knox Coll., Galesburg, Ill. (21). 1874.
Cook, Prof. A. J., Agricultural College, Lansing, Mich. (24). 1880.
Cook, Prof. George H., New Brunswick, N. J. (4). 1875.
Cooley, Prof. Le Roy C., Vassar College, Poughkeepsie, N. Y. (19). 1880.
Cope, Prof. Edward D., 2100 Pine St., Philadelphia, Pa. (17). 1875.
Cox, Prof. Edward T., New Harmony, Ind. (19). 1874.
Cox, Hon. Jacob D., 103 Broadway, Cincinnati, Ohio (30). 1881.
Coxe, Eckley B., Drifton, Luzerne Co., Pa. (23). 1879.
Crandall, Prof. A. R., Lexington, Ky. (29). 1883.
Crocker, Susan E., M.D., Lawrence, Mass. (21). 1874.
Crosby, Wm. O., Mass. Inst. Technology, Boston, Mass. (29). 1881.
Cross, Prof. Chas. R., Mass. Inst. Technology, Boston, Mass. (29). 1880.
Cummings, Rev. Joseph, D.D., Pres. Northwestern Univ., Evanston, Ill. (13). 1874.

Cutting, Hiram A., M.D., State Geologist, Lunenburgh, Vt. (17). 1874.

Dabney, Chas. W., jr., Ph. D., Agric. Experiment Station, Raleigh, N. C. (30). 1882.
Dall, Mrs. Caroline H., 31st St., Washington, D. C. (18). 1874.
Dall, Wm. H., Smithsonian Institution, Washington, D. C. (18). 1874.
Dana, Edward Salisbury, New Haven, Conn. (23). 1875.
Dana, Prof. James D., New Haven, Conn. (1). 1875.
Danforth, Edward, Dep't of Public Instruction, Elmira, N. Y. (11). 1874.
Davenport, B. F., M. D., 751 Tremont St., Boston, Mass. (29). 1883.
Davidson, Prof. Geo., Asst. Coast and Geodetic Survey, San Francisco, Cal. (29). 1881.
Dawson, Dr. J. W., Principal McGill Coll., Montreal, Canada (10). 1875.
Day, F. H., M. D., Wauwatosa, Wis. (20). 1874.
Dean, George W., P. O. Box 92, Fall River, Mass. (15). 1874.
Dimmock, George, Cambridge, Mass. (22). 1874.
Dinwiddie, Robert, 117 W. 43d St., New York, N. Y. (1). 1874.
Dodge, Charles R., Washington, D. C. (22). 1874.
Dolbear, A. Emerson, College Hill, Mass. (20). 1880.
Dorsey, Rev. James O., Bureau of Ethnology, Washington, D. C. (31) 1883.
Draper, Dan'l, Ph.D., Director N. Y. Meteorological Observatory, Central Park, 64th St., Fifth Avenue, New York, N. Y. (29). 1881.
Drown, Prof. Thos. M., Lafayette College, Easton, Pa. (29). 1881
Du Bois, Prof. Aug. J., New Haven, Conn. (30). 1882.
Dudley, Charles B., Altoona, Pa. (23). 1882.
Dudley, Wm. L., Miami Med. College, Cincinnati, Ohio (28). 1881.
Dunnington, Prof. F. P., University of Virginia, Va. (26). 1880.
Dutton, Capt. C. E., U. S. Ord. Dep't, Washington, D. C. (23). 1875.
Dwight, Prof. Wm. B., Vassar College, Poughkeepsie, N. Y. (30). 1882.
Dwyer, John, M. D., Hartford, Conn. (23). 1874.

Eads, Jas. B., 314 Chestnut Street, St. Louis, Mo. (27). 1879.
Eastman, Prof. J. R., U. S. Naval Observatory, Washington, D. C. (26). [1879.
Eaton, Prof. D. G., Packer Institute, Brooklyn, N. Y. (19). 1874.
Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25). 1883.
Eddy, Prof. H. T., University of Cincinnati, Cincinnati, Ohio (24). 1875.
Edison, Thos. A., Menlo Park, N. J. (27). 1878.
Edmands, J. Rayner, Observatory, Cambridge, Mass. (29). 1880.
Egleston, Prof. Thos., School of Mines, Columbia College, New York, N. Y. (27). 1879.
Eimbeck, Wm., U.S.C.S., Washington, D. C. (17). 1874.
Elliott, Arthur H., School of Mines, Columbia College, New York, N. Y. (23). 1880.
Elliott, Ezekiel B., Government Actuary, Treasury Dept., Washington, D. C. (10). 1874.
Elsberg, Louis, M.D., 614 Fifth Avenue, New York, N. Y. (23). 1874.

Emerson, Prof. Benjamin K., Amherst, Mass. (19). 1877.
Emerson, Prof. C. F., Dartmouth College, Hanover, N. H. (22). 1874.
Emerton, James H., Salem, Mass. (18). 1875.
Emmons, S. F., Box 2836, Denver, Col. (26). 1879.
Engelmann, Dr. George, St. Louis, Mo. (1). 1875.
Engelmann, Geo. J., M. D., 3003 Locust St., St. Louis, Mo. (25). 1878.
Ernst, Carl W., Daily Advertiser, Boston, Mass. (23). 1874.
Eustis, Prof. Henry L., Cambridge, Mass. (2). 1874.
Evans, Asher B., Principal Union School, Lockport, N. Y. (19). 1874.

Fairbanks, Henry, Ph. D., St. Johnsbury, Vt. (14). 1874. .
Fairchild, H. L., 102 E. 52d St., New York, N. Y. (28). 1883.
Farlow, Dr. W. G., 6 Park Square, Boston, Mass. (20). 1875.
Farmer, Moses G., Torpedo Station, Newport, R. I. (9). 1875.
Farquharson, Dr. Robert James, Des Moines, Iowa (24). 1880.
Ferguson, Maj. Thos. B., Ass't U. S. Fish Commissioner, Washington, D. C. (28). 1881.
Fernald, Prof. Charles H., State Agricultural College, Orono, Me. (22). 1881.
Fernald, Prof. M. C., State Agricultural College, Orono, Mo. (22). 1883.
Ferrel, Wm., U. S. Coast Survey Office, Washington, D. C. (11). 1875.
Ficklin, Prof. Joseph, Univ. of Missouri, Columbia, Mo. (20). 1878.
Fitch, Edward H., Jefferson, Ashtabula Co., Ohio (11). 1874.
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29). 1883.
Fletcher, James, Library of Parliament, Ottawa, Can. (31). 1883.
Fletcher, Dr. Robert, Surgeon General's Office, Washington, D. C. (29). 1881.
Flint, James M., Surgeon U. S. N., 42 Dartmouth St., Boston, Mass. (28). 1882.
Foote, Dr, A. E., 1223 Belmont Avenue, Philadelphia, Pa. (21). 1874.
Forbes, Prof. S. A., Normal, Ill. (27). 1879.
Frazer, Dr. Persifor, Ass't Geologist, 2d Geol. Survey of Pa., 917 Clinton St., Philadelphia, Pa. (24). 1879.
Frazier, Prof. B. W., The Lehigh University, Bethlehem, Pa. (24). 1882.
Freeman, H. C., Alto Pass, Union Co., Ill. (17). 1876.
Freeman, Spencer H., Adelkert College, Cleveland, Ohio (29). 1883.
French, Prof. Thomas, jr., Univ. of Cincinnati, Cincinnati, Ohio (30). 1883.
Frisby, Prof. Edgar, U. S. N. Observ., Washington, D. C. (28). 1880.
Fuller, Andrew S., Ridgewood, Bergen Co., N. J. (24). 1882.

Gage, Simon Henry, Ithaca, N. Y. (28). 1881.
Gardiner, Rev. Frederic, D.D., Middletown, Conn. (23). 1874.
Gardner, James T., Director N. Y. State Survey, Albany, N. Y. (25). 1879.
Garland, Rev. Dr. L. C., Chancellor Vanderbilt Univ., Nashville, Tenn. (25). 1877.

Garman, S., Museum Comp. Zoology, Cambridge, Mass. (20). 1874.
Gatschet, Albert S., Bureau of Ethnology, Washington, D. C. (30). 1882.
Genth, Prof. F. A., Univ. of Pennsylvania, Philadelphia, Pa. (24). 1875.
Gilbert, G. K., Box 240, Salt Lake City, Utah (18). 1874.
Gillman, Henry, 155 Adams Avenue East, Detroit, Mich. (24). 1875.
Gilman, Daniel C., President of the Johns Hopkins University, Baltimore, Md. (10). 1875.
Goessman, Prof. C. A., Mass. Agric. Coll., Amherst, Mass. (18). 1875.
Goldschmidt, S. A., Ph. D., Morse Building, 140 Nassau St., New York, N. Y. (24). 1880.
Gooch, Frank A., U. S. Geological Survey, Newport, R. I. (25). 1880.
Goodale, Prof. G. L., Botanic Gardens, Cambridge, Mass. (18). 1875.
Goode, Prof. G. Brown, Curator National Museum, Washington, D. C. (22). 1874.
Goodfellow, Edward, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (24). 1879.
Grant, Mrs. Mary J., Brookfield, Conn. (23). 1874.
Gray, Prof. Asa, Botanic Gardens, Cambridge, Mass. (1). 1875.
Gray, Elisha, Ph. D., Highland Park, Ill. (32). 1883.
Green, Traill, M. D., Easton, Pa. (1). 1874.
Grimes, J. Stanley, 406 Sycamore St., Milwaukee, Wis. (17). 1874.
Grote, Aug. R., New Brighton, Staten Island, N. Y. (22). 1875.
Gulley, Prof. Frank A., Agricultural College, Oktibbeha Co., Miss. (30). 1883.
Guyot, Prof. Arnold, Princeton, N. J. (1). 1875.

Hagen, Dr. Hermann A., Mus. Comp. Zool., Cambridge, Mass. (17). 1875.
Hague, Arnold, 10 West 29th St., New York, N. Y. (26). 1879.
Hale, Horatio, Clinton, Ontario, Can. (30). 1882.
Hall, Prof. Asaph, U. S. Naval Observ., Washington, D. C. (25). 1877.
Hall, Prof. C. W., Univ. of Minnesota, Minneapolis, Minn. (28). 1883.
Hall, Edwin H., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Hall, Prof. James, Albany, N. Y. (1). 1875.
Hamlin, Dr. A. C., Bangor, Me. (10). 1874.
HANAMAN, C. E., Troy, N. Y. (19). 1883.
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28). 1883.
Harger, Oscar, Yale College Museum, New Haven, Conn. (25). 1879.
HARKNESS, PROF. WM., U. S. N., U. S. Observatory, Washington, D. C. (26). 1878.
Harrington, Prof. Mark W., Ann Arbor, Mich. (22). 1875.
Harrison, Dr. B. F., Wallingford, Conn. (11). 1874.
Hasbrouck, Prof. I. E., Rutgers Coll., New Brunswick, N. J. (23). 1874.
Haskins, Rev. David Greene, Cambridge, Mass. (25). 1874.
Hastings, C. S., Johns Hopkins University, Baltimore, Md. (25). 1878.
Hazen, Brig. Gen. Wm. B., Chief Signal Office, U. S. A., Washington, D. C. (30). 1882.
Hedrick, B. S., Exam'r U. S. Pat. Office, Washington, D. C. (19). 1874.

Henderson, John G., Winchester, Ill. (25). 1879.
Henshaw, Henry W., Care Major J. W. Powell, 910 M St., Washington, D. C. (24). 1877.
Hervey, Rev. A. B., Taunton, Mass. (22). 1879.
Hilgard, Prof. E. W., Univ. of California, Berkeley, Cal. (11). 1874.
Hilgard, Prof. J. E., U. S. Coast Survey, Washington, D. C. (4). 1874.
Hill, Franklin C., P. O. Box 338, Princeton, N. J. (29). 1881.
Hill, George W., Nautical Almanac Office, Navy Department, Washington, D. C. (23). 1874.
Hill, Rev. Dr. Thomas, 738 Congress St., Portland, Me. (8). 1875.
Hill, Walter N., Chester, Pa. (29). 1881.
Himes, Prof. Charles F., Carlisle, Pa. (29). 1882.
Hinrichs, Prof. Gustavus, State University, Iowa City, Iowa (17). 1874.
Hitchcock, Prof. Charles H., Hanover, N. H. (11). 1874.
Hitchcock, Romyn, 53 Maiden Lane, New York, N. Y. (29). 1881.
Hodges, N. D. C., Salem, Mass. (29). 1882.
Hoffmann, Dr. Fred., 797 Sixth Ave., New York, N. Y. (28). 1881.
Holden, Prof. E. S., Director Washburn Observatory, Univ. of Wisconsin, Madison, Wis. (23). 1875.
Holman, Silas W., Mass. Inst. of Technology, Boston, Mass. (31). 1883.
Holmes, Dr. Oliver Wendell, 296 Beacon St., Boston, Mass. (29). 1881.
Holmes, Wm. H., Smithsonian Institution, Washington, D. C. (30). [1883.
Horsford, Prof. E. N., Cambridge, Mass. (1). 1876.
Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30). 1883.
Hough, Franklin B., Lowville, N. Y. (4). 1874.
Hough, G. W., Director Dearborn Observatory, Chicago, Ill. (15). 1874.
Hovey, Rev. Horace C., Minneapolis, Minn, (29). 1883.
Hoy, Philo R., M.D., Racine, Wis. (17). 1875.
Hunt, George, Providence, R. I. (9). 1874.
Hunt, Dr. T. Sterry, Montreal, Canada (1). 1874.
Huntington, Prof. J. H., Boston, Mass. (19). 1874.
Hyatt, Prof. Alpheus, Natural History Society, Boston, Mass. (18). 1875.
Hyatt, James, Stanfordville, Dutchess Co., N. Y. (10). 1874.
Hyde, Prof. E. W., Walnut Hills, Cincinnati, Ohio (25). 1881.

James, Jos. F., 177 Race St., Cincinnati, Ohio (30). 1882.
Jeffries, B. Joy, M. D., 15 Chestnut St., Boston, Mass. (29). 1881.
Jenks, Elisha T., Middleborough, Mass. (22). 1874.
Jenks, Prof. J. W. P., Middleborough, Mass. (2). 1874.
Jewell, Theo. F., Lt. Comdr. U.S.N., Torpedo Station, Newport, R. I. (25). 1882.
Jillson, Dr. B. C., Illinois Industrial Univ., Champaign, Ill. (14). 1881.
Johnson, Prof. S. W., Sheffield Sci. School, New Haven, Conn. (22). 1874.
Johnson, Prof. W. W., Annapolis, Md. (29). 1881.
Joy, Prof. Charles A., Stockbridge, Mass. (8). 1879.
Julien, A. A., School of Mines, Columbia Coll., New York, N. Y. (24). 1875.

Kedzie, Prof. Robert C., Lansing, Mich. (29). 1881.

Kendall, Prof. E. Otis, 3826 Locust St., Philadelphia, Pa. (29). 1882.
Kennan, George, 1318 Mass. Avenue, Washington, D. C. (30). 1882.
Kent, William, Pittsburgh, Pa. (26). 1881.
Kerr, Prof. W. C., Raleigh, N. C. (10). 1874.
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29). 1883.
Kinnicutt, Leonard P., Worcester, Mass. (28). 1883.
Kirkwood, Prof. Daniel, Bloomington, Ind. (7). 1874.
Kunz, G. F., care Tiffany and Co., New York, N. Y. (29). 1883.

Lambert, Rev. Thomas R., D.D., Charlestown, Mass. (18). 1874.
Langdon, Dr. F. W., 65 West 7th St., Cincinnati, Ohio (30). 1882.
Langley, Prof. J. W., Univ. of Michigan, Ann Arbor, Mich. (23). 1875.
Langley, Prof. S. P., Director of Observatory, Allegheny, Pa. (18). 1874.
Lanza, Prof. Gaetano, Mass. Inst. of Technology, Boston, Mass. (29) 1882.
Larkin, Edgar L., New Windsor, Mercer Co., Ill. (28). 1883.
Lattimore, Prof. S. A., Univ. of Rochester, Rochester, N. Y. (15). 1874.
Lawrence, George N., 45 E. 21st St., New York, N. Y. (7). 1877.
Lazenby, Prof. Wm. R., Ithaca, N. Y. (30). 1882.
Lea, Dr. Isaac, 1622 Locust St., Philadelphia, Pa. (1). 1875.
LeConte, Prof. Joseph, Univ. of Cal., Berkeley, Cal. (29). 1881.
Ledoux, Albert R., Ph. D., 17 Cedar St., New York, N. Y. (26). 1881.
Leeds, Prof. Albert R., Stevens Institute, Hoboken, N. J. (23). 1874.
Lesley, Prof. J. Peter, State Geol. of Pa., 1008 Clinton St., Philadelphia, Pa. (2). 1874.
Lewis, Prof. Henry C., E. Washington Lane, Germantown, Pa. (26). 1880.
LILLY, GEN. WM., Mauch Chunk, Carbon Co., Pa. (28). 1882. Patron.
Lindsley, Dr. J. Berrien, Nashville, Tenn. (1). 1874.
Lintner, J. A., N. Y. State Mus. of Nat. Hist., Albany, N. Y. (22). 1874.
Little, W. C., Albany, N. Y. (22). 1874.
Litton, Abram, Washington University, St. Louis, Mo. (28). 1879.
Lockwood, Rev. Samuel, Freehold, Monmouth Co., N. J. (18). 1875.
Loomis, Prof. Elias, New Haven, Conn. (1). 1874.
Lord, Prof. Nat. W., State Univ., Columbus, Ohio (29). 1881.
Loudon, Prof. James, Toronto, Can. (25). 1881.
Loughridge, Prof. R. H., Berkeley, Cal. (21). 1874.
Love, Edward G., School of Mines, Columbia College, New York, N. Y. (24). 1882.
Lovering, Prof. Joseph, Harvard University, Cambridge, Mass. (2). 1875.
Lull, Edward P., Captain U. S. N., Navy Yard, Boston, Mass. (28). 1880.
Lupton, Prof. N. T., Vanderbilt University, Nashville, Tenn. (17). 1874.
Lyle, David Alexander, Capt. of Ordnance U. S. A., National Armory, Springfield, Mass. (28). 1880.
Lyman, Prof. Chester S., New Haven, Conn. (4). 1875.
Lyman, Hon. Theodore, Brookline, Mass. (23). 1875.
Lyon, Dr. Henry, 34 Monument Sq., Charlestown, Mass. (18). 1874.

Mabery, Prof. C. F., Case School of Applied Science, Cleveland, Ohio (29). 1881.
Macfarlane, James, Towanda, Pa. (29). 1882.
Mackintosh, James B., School of Mines, Columbia College, New York, N. Y. (27). 1883.
Macloskie, Prof. George, College of New Jersey, Princeton, N. J. (25). 1882.
Mallery, Brevet Lieut. Col. Garrick, U. S. Army, Box 585, Washington, D. C. (26). 1879.
MANN, B. PICKMAN, U. S. Dep't of Agric., Washington, D. C. (22). 1874.
Marcy, Oliver, LL.D., Evanston, Ill. (10). 1874.
Mark, Edw. L., 48 Shepard St., Cambridge, Mass. (21). 1879.
Markoe, Geo. F. H., 61 Warren St., Roxbury, Mass. (29). 1881.
MARSH, PROF. O. C., Yale College, New Haven, Conn. (15). 1874.
Martin, Prof. Daniel S., Rutgers Female College, New York, N. Y. (23). 1879.
Martin, Prof. H. Newell, Johns Hopkins Univ., Baltimore, Md. (27). 1880.
Mason, Prof. Otis T., Columbian University, Washington, D. C. (25). 1877.
Mayor, Prof. A. M., So. Orange, N. J. (19). 1874.
McCauley, Capt. C. A. H., Ass't Q. M., U. S. A., 821 Michigan Ave., Chicago, Ill. (29). 1881.
McGee, W. J., U. S. Geol. Survey, Washington, D. C. (27). 1882.
McMurtrie, William, Dep't Agriculture, Washington, D. C. (22). 1874.
McNair, Lt. Commander Antoine R., U. S. N., Box 1071, Saratoga Springs, N. Y. (28). 1882.
McRae, Hamilton S., Supt. of Schools, Muncie, Ind. (20). 1874.
Meehan, Thomas, Germantown, Pa. (17). 1875.
Mees, Carl Leo, Columbus, Ohio (24). 1876.
Mendenhall, Prof. T. C., Columbus, Ohio (20). 1874.
Merriman, C. C., Rochester, N. Y. (29). 1880.
Michelson, A. A., Master U. S. N., Nautical Almanac Office, Washington, D. C. (26). 1879.
Minot, Charles Sedgwick, Roslindale P. O., Boston, Mass. (28). 1880.
Mitchell, Miss Maria, Vassar College, Poughkeepsie, N. Y. (4). 1874.
Mixter, Prof. Wm. G., New Haven, Conn. (30). 1882.
Moore, Prof. J. W., M. D., Lafayette College, Easton, Pa. (22). 1874.
Morley, Prof. Edward W., 749 Republic St., Cleveland, Ohio (18). 1876.
Morris, Rev. John G., Baltimore, Md. (12). 1874.
Morse, Prof. E. S., Salem, Mass. (18). 1874.
Morton, H., Stevens Institute Technology, Hoboken, N. J. (18). 1875.
Moses, Prof. Thos. F., Urbana Univ., Urbrna, Ohio (25). 1883.
Munroe, Prof. C. E., U. S. Naval Academy, Annapolis, Md. (22). 1874.
Murtfeldt, Miss Mary E., Kirkwood, Mo. (27). 1881.

Nason, Prof. H. B., Rensselaer Polytechnic Inst., Troy, N. Y. (13). 1874.
Neale, Arthur T., Ph. D., New Brunswick, N. J. (29). 1882.
Nelson, Prof. A. B., Centre College, Danville, Ky. (30). 1882.

Nelson, Prof. Edward T., Delaware, Delaware Co., Ohio (24). 1877.
Newberry, Prof. J. S., Cleveland, Ohio, and Columbia College, New York, N. Y. (5). 1875.
Newcomb, Prof. S., U. S. Naval Observ., Washington, D. C. (13). 1874.
Newton, Hubert A., New Haven, Conn. (6). 1874.
Nichols, E. L., Ph. D., Central Univ. Richmond, Ky. (28). 1881.
Nichols, Prof. W. R., Mass. Inst. Technology, Boston, Mass. (18). 1875.
Niles, Prof. W. H., Cambridge, Mass. (16). 1874.
Nipher, Prof. F. E., Washington University, St. Louis, Mo. (24). 1876.
Norton, Prof. W. A., New Haven, Conn. (6). 1875.
Norwood, Charles J., Ass't Kentucky Geological Survey, Russellville, Ky. (26). 1881.

Oliver, Prof. James E., Cornell University, Ithaca, N. Y. (7). 1875.
Oliver, Miss Mary E., Cascadilla Hotel, Ithaca, N. Y. (20). 1874.
Ordway, Prof. John M., Mass. Inst. Technology, Boston, Mass. (9). 1875.
Orton, Prof. Edward, President Ohio Agricultural and Mechanical College, Columbus, Ohio (19). 1875.
Osborn, Henry F., S. D., Garrisons, N. Y. (29). 1883.
Osborne, J. W., 212 Delaware Ave. N. E., Washington, D. C. (22). 1874.
Osler, Prof. William, McGill College, Montreal, Canada (28). 1881.
Owen, Dr. Richard, New Harmony, Ind. (20). 1874.

Packard, Dr. A. S., Jr., 115 Angell St., Providence, R. I. (16). 1875.
Paine, Cyrus F., Rochester, N. Y. (12). 1874.
Paine, Nathaniel, Worcester, Mass. (18). 1874.
Palfray, Hon. Charles W., Salem, Mass. (21). 1874.
Parke, John G., Lt. Col. Corps of Eng'rs, Bv't Maj. Gen. U. S. A., Office of Chief of Engineers, Washington, D. C. (29). 1881.
Parkhurst, H. M., Law Stenographer, 25 Chambers St., New York, N. Y. (23). 1874.
Payne, Prof. Wm. W., Carleton College, Northfield, Minn. (30). 1883.
Peckham, S. F., Providence, R. I. (18). 1875.
Pedrick, Wm. R., Lawrence, Mass. (22). 1875.
Peet, Rev. Stephen D., Clinton, Wis. (24). 1881.
Penhallow, Prof. D. P., McGill College, Montreal, Can. (30). 1882.
Perkins, Prof. George H., Burlington, Vt. (17). 1882.
Perkins, Maurice, Schenectady, N. Y. (15). 1875.
Peter, Dr. Robert, Ky. Geol. Survey, Lexington, Ky. (29). 1881.
Pettee, Prof. William H., Ann Arbor, Mich. (24). 1875.
Phillips, A. W., New Haven, Conn. (24). 1879.
Phippen, Geo. D., Salem, Mass. (18). 1874.
Pickering, Prof. E. C., Director of Observ., Cambridge, Mass. (18). 1875.
Pickering, William H., 84 Mount Vernon St., Boston, Mass. (29). 1883.
Pike, Dr. William H., University College, Toronto, Can. (29). 1881.
Pilling, James C., Box 585, Washington, D. C. (28). 1882.

Platt, Franklin, Ass't Geologist, 2nd Geol. Survey of Pa., 615 Walnut St., Philadelphia, Pa. (27). 1882.
Potter, Prof. William B., Washington University, St. Louis, Mo. (25). 1879. [1875.
Powell, Major J. W., U. S. Geologist, 910 M St., Washington, D. C. (23).
Prentiss, D. Webster, M. D., Washington, D. C. (29). 1882.
Prescott, Prof. Albert B., Ann Arbor, Mich. (23). 1875.
Prime, Frederick, jr., Allentown Iron Co., 230 So. 3d St., Philadelphia, Pa. (24). 1875.
Pritchett, Henry S., Observatory, Washington Univ., St. Louis, Mo. (29). 1881.
Procter, John R., Director Ky. Geol. Survey, Frankfort, Ky. (26). 1881.
Pulsifer, Wm. H., St. Louis, Mo. (26). 1879.
Pumpelly, Prof. Raphael, U. S. Geol. Survey, Newport, R. I. (17). 1875.
Putnam, F. W., Curator Peabody Museum Archæology and Ethnology, Cambridge, Mass. Address as Permanent Secretary A. A. A. S., Salem, Mass. (10). 1874.
Putnam, James J., M. D., 63 Marlborough St., Boston, Mass. (28). 1880.
Pynchon, Rev. T. R., Pres. Trinity College, Hartford, Conn. (23). 1875.

Quimby, Prof. E. T., Hanover, N. H. (22). 1874.
Quincy, Edmund, 88 Clinton St., Boston, Mass. (11). 1874.

Rauch, Dr. John H., Chicago, Ill. (11). 1875.
Raymond, Rossiter W., 17 Burling Slip, New York, N. Y. (15). 1875.
Redfield, J. H., care A. Whitney & Sons, Philadelphia, Pa. (1). 1874.
Reed, E. Baynes, London, Ontario, Can. (27). 1882.
Rees, Prof. John K., Columbia College, New York, N. Y. (26). 1878.
Remsen, Prof. Ira, Johns Hopkins University, Baltimore, Md. (22). 1875
Rice, John M., U. S. Naval Academy, Annapolis, Md. (25). 1881.
Rice, Prof. Wm. North, Middletown, Conn. (18). 1874.
Richards, Prof. Robert H., Mass. Inst. Tech., Boston, Mass. (22). 1875.
Richards, Mrs. Robert H., Mass. Inst. of Tech., Boston, Mass. (23). 1878.
Ricketts, Pierre de Peyster, Ph.D., 10 Cedar St., New York, N. Y. (26). 1880.
RILEY, PROF. C. V., U. S. Entomologist, 1700 13th St. N.W., Washington, D. C. (17). 1874.
Ritchie, E. S., 150 Tremont St., Boston, Mass. (10). 1877.
Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30). 1883.
Rockwell, Gen. Alfred P., Office Great Falls Manufacturing Co., Boston, Mass. (10). 1882.
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28). 1883.
Rockwood, Prof. Charles G., jr., Princeton, N. J. (20). 1874.
Rogers, Fairman, 202 West Rittenhouse Square, Philadelphia, Pa. (11). 1874.
Rogers, Prof. Rob't E., Jefferson Med. Coll., Philadelphia, Pa. (18). 1874.
Rogers, W. A., Ass't Prof. Harvard College Observ., Cambridge, Mass.

(15). 1875.

Rominger, Dr. Carl, Ann Arbor, Mich. (21). 1879.

Rood, Prof. O. N., Cor. E. 49th St. & Madison Ave., New York, N. Y. (14) 1875.

Roosevelt, Clinton, 11 Wall St., New York, N. Y. (11). 1874.

Rosenspitz, Dr. Alexander, Rabbi, Lock Box 430, Portland, Oregon, (26). 1883.

Ross, Waldo O., 189 Devonshire St., Boston, Mass. (29). 1882.

Rowland, Prof. Henry A., Baltimore, Md. (29). 1880.

Runkle, Prof. J. D., Mass. Inst. of Tech., Boston, Mass. (2). 1875.

Russell, I. C., U. S. Geological Survey, Salt Lake City, Utah (25). 1882.

Rutherford, Lewis M., 175 Second Ave., New York, N. Y. (13). 1875.

Safford, Dr. James M., Nashville, Tenn. (6). 1875.

Sampson, Comdr. W. T., U. S. N., Naval Observatory, Washington, D. C. (25). 1881.

Saunders, William, London, Ontario, Canada (17). 1874.

Schanck, Prof. J. Stillwell, Princeton, N. J. (4). 1882.

Schott, Charles A., U. S. Coast and Geodetic Survey Office, Washington, D. C. (8). 1874.

Schweitzer, Prof. Paul, State Univ. of Mo., Columbia, Mo. (24). 1877.

SCUDDER, SAMUEL H., Cambridge, Mass. (13). 1874.

Seaman, W. H., Microscopist, 1424 11th St. N. W., Washington, D. C. (23). 1874.

Seiler, Carl, M. D., Philadelphia, Pa. (29). 1882.

Sestini, Rev. B., Woodstock, Md. (29). 1882.

Sheafer, P. W., Pottsville, Pa. (4). 1879.

Sias, Solomon, M.D., Schoharie, Schoharie Co., N. Y. (10). 1874.

Sigsbee, Chas. D., Comd'r U. S. N., Navy Dep't, Washington, D. C. (28). 1882.

Sill, Hon. Elisha N., Cuyahoga Falls, Ohio (6). 1874.

Silliman, Prof. Benj., Yale College, New Haven, Conn. (1). 1874.

Silliman, Prof. Justus M., Lafayette College, Easton, Pa. (19). 1874.

Skinner, Joseph J., 131 Devonshire St., Boston, Mass. (23). 1880.

Smiley, Charles W., Smithsonian Institution, Washington, D. C. (28). 1883.

Smith, Prof. E. A., University of Alabama, Tuscaloosa, Ala. (20). 1877.

Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30). 1882.

Smith, Mrs. Erminnie A., 203 Pacific Ave., Jersey City, N. J. (25). 1880.

Smith, Prof. F. H., University of Virginia, Charlottesville, Va. (26). 1880.

SMITH, QUINTIUS C., M. D., Box 707, Austin, Texas (26). 1881.

Smith, Prof. S. I., Yale College, New Haven, Conn. (18). 1875.

Smock, Prof. John Conover, Box 282, New Brunswick, N. J. (23). 1879.

Snow, Prof. F. H., Lawrence, Kan. (29). 1881.

Snyder, Prof. Monroe B., High School Observatory, Philadelphia, Pa. (24). 1882.

Spencer, Prof. J. William, Univ. of Missouri, Columbia, Mo. (28). 1882.
Spencer, John W., Geol., Paxton, Sullivan Co., Ind. (20). 1874.
Springer, Dr. Alfred, Box 573, Cincinnati, Ohio (24). 1880.
Stallo, J. B., Masonic Temple, Cincinnati, Ohio (30). 1882.
Stanard, Benjamin A., Cleveland, Ohio (6). 1874.
Stearns, Henry P., M.D., Hartford, Conn. (23). 1874.
Stearns, R. E. C., Box 13, Berkeley, Cal. (18). 1874.
Steiner, Dr. Lewis H., Frederick City, Md. (7). 1874.
STEPHENS, W. HUDSON, Lowville, N. Y. (18). 1874.
Sternberg, George M., Surgeon U.S.A., Care Surgeon General, U.S.A., Washington, D. C. (24). 1880.
Stevens, W. LeConte, 170 Joralemon St., Brooklyn, N. Y. (29). 1882.
Stockwell, John N., 579 Case Avenue, Cleveland, Ohio (18). 1875.
Stone, George H., Colorado Springs, Col. (29). 1882.
Stone, Mrs. Leander, 3352 Indiana Avenue, Chicago, Ill. (22). 1874.
Stone, Ormond, Director Leander McCormick Observatory, Univ. of Va., Va. (24). 1876.
Storrs, Henry E., Jacksonville, Ill. (20). 1874.
Story, Wm. E., Johns Hopkins Univ., Baltimore, Md. (29). 1881.
Stuart, Prof. A. P. S., Lincoln, Nebraska (21). 1874.
Sturtevant, E. Lewis, M. D., Geneva, N. Y. (29). 1882.
Sutton, Dr. George, Aurora, Dearborn Co., Ind. (20). 1881.
Swallow, Prof. G. C., Columbia, Mo. (10). 1875.
Swift, Lewis, Ph. D., Rochester, N. Y. (29). 1882.

Tainter, Sumner, 1221 Connecticut Ave., Washington, D. C. (29). 1881.
Tappan, Prof. Eli T., Kenyon College, Gambier, Ohio (20). 1874.
Taylor, Arthur F., Ph. D., Case School of Applied Science, Cleveland, Ohio (29). 1882.
Taylor, Wm. B., Smithsonian Institution, Washington, D. C. (29). 1881.
Terry, Prof. N. M., U. S. Naval Academy, Annapolis, Md. (23). 1874.
Thomas, Benj. F., Ph. D., Univ. State of Missouri, Columbia, Mo. (29). 1882.
Thomas, Prof. Cyrus, Carbondale, Ill. (30). 1882.
Thurston, Prof. R. H.,Stevens Institute, Hoboken, N. J. (23). 1875.
Todd, Prof. David P., Director Lawrence Observatory, Amherst College, Amherst, Mass. (27). 1881.
Townshend, Prof. N. S., Columbus, Ohio (17). 1881.
Tracy, Sam'l M., Columbia, Boone Co., Mo. (27). 1881.
Trembley, J. B., M.D., Oakland, Alameda Co., Cal. (17). 1880.
Trowbridge, Prof. John, Harvard University, Cambridge, Mass. (25). 1876.
Trowbridge, Prof. W. P., New Haven, Conn. (10). 1874.
True, Fred W., U. S. National Museum, Washington, D. C. (28). 1882.
Trumbull, Dr. J. Hammond, Hartford, Conn. (29). 1882.
Tuttle, Prof. Albert H., Columbus, Ohio (17). 1874.

Uhler, Philip R., Baltimore, Md. (19). 1874.

Upham, Warren, Nashua, N. H. (25). 1880.
Upton, Winslow, Army Signal Office, Washington, D. C. (29). 1883.

Van der Weyde, P. H., M. D., Box 3619, New York, N. Y. (17). 1874.
Van Dyck, Prof. Francis Cuyler, New Brunswick, N. J. (28). 1882.
Van Vleck, Prof. John M., Middletown, Conn. (23). 1875.
Verrill, Prof. A. E., Yale College, New Haven, Conn. (16). 1875.
Vose, Prof. George L., Mass. Inst. Technology, Boston, Mass. (15). 1875.

Wadsworth, M. Edward, Ph. D., Assistant in Lithology, Museum of Comparative Zoology, Cambridge, Mass. (23). 1874.
Walcott, Charles D., U. S. Geol. Survey, Washington, D. C. (25). 1882.
Waldo, Leonard, S. D., New Haven, Conn. (28). 1880.
Walker, Prof. J. R., Bay Saint Louis, Hancock Co., Miss. (19). 1874.
Walker, Prof. Joseph B. (20). 1874.
Wallace, Wm., Ansonia, Conn. (28). 1882.
WALLER, E., School of Mines, Columbia College, New York, N. Y. (23). 1874.
Walling, H. F., Cambridge, Mass. (16). 1874.
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28). 1883.
Ward, Prof. Henry A., Rochester, N. Y. (13). 1875.
Ward, Lester F., U. S. Geological Survey, Washington, D. C. (26). 1879.
Ward, Dr. R. H., 53 Fourth St., Troy, N. Y. (17). 1874.
Warder, Prof. Robert B., Lafayette, Ind. (19). 1881.
WARNER, JAMES D., 199 Baltic St., Brooklyn, N. Y. (18). 1874.
Warren, Cyrus M., Brookline, Mass. (29). 1882.
Warren, Prof. S. Edward, Newton, Mass. (17). 1875.
Watson, Sereno, Botanic Gardens, Cambridge, Mass. (22). 1875.
Wead, Prof. Charles K., Univ. of Michigan, Ann Arbor, Mich. (23). 1880.
Webb, Prof. J. Burkitt, Ithaca, N. Y. (31). 1883.
Webster, Prof. H. E., Union College, Schenectady, N. Y. (23). 1882.
Webster, Prof. N. B., Principal Webster Inst., Norfolk, Va. (7). 1874.
Wells, Daniel H., Hartford, Conn. (18). 1875.
Westcott, O. S., Maywood, Cook Co., Ill. (21). 1874.
Wheatland, Dr. Henry, President Essex Inst., Salem, Mass. (1). 1874.
Wheeler, Capt. Geo. M., U. S. E., Lock Box 93, Washington, D. C. (25). 1879.
Wheeler, Orlando B., Ass't U. S. Lake Survey, Detroit, Mich. (24). 1882.
Wheildon, W. W., Concord, Mass. (13). 1874.
White, Prof. C. A., Le Droit Park, Washington, D. C. (17). 1875.
White, Prof. I. C., Univ. of W. Va., Morgantown, W. Va. (25). 1882.
Whitfield, R. P., American Museum Natural History, 77th St. & 8th Avenue, New York, N. Y. (18). 1874.
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31). 1883.
Whitman, Chas. O., care G. F. Leonard, Newton Highlands, Mass. (27). 1880.
Whitney, Solon F., Watertown, Mass. (20). 1874.

Whittlesey, Col. Charles, 1305 Euclid Ave., Cleveland, Ohio (1). 1875.
Wilber, G. M., Pine Plains, N. Y. (19). 1874.
Wilbur, A. B., Port Jervis, Orange Co., N. Y. (23). 1874.
Wilder, Prof. Burt G., Cornell University, Ithaca, N. Y. (22). 1875.
Wiley, Prof. Harvey W., Dept. of Agric., Washington, D. C. (21). 1874.
Williams, Charles H., M.D., 15 Arlington St., Boston, Mass. (22). 1874.
Williams, H. S., Cornell Univ., Ithaca, N. Y. (18). 1882.
Williams, Prof. Henry W., 15 Arlington St., Boston, Mass. (11). 1874.
Wilson, Prof. Daniel, President University College, 117 Bloor St., Toronto, Canada (25). 1876.
Winchell, Prof. Alex., Ann Arbor, Mich. (3). 1875.
Winchell, Prof. N. H., Univ. of Minnesota, Minneapolis, Minn. (19). 1874.
Winsor, Justin, Librarian Harvard Univ., Cambridge, Mass. (29). 1882.
Woerd, Chas. V., Am. Watch Co., Waltham, Mass. (29). 1881.
Wood, Prof. De Volson, Hoboken, N. J. (29). 1881.
Wormley, T. G., Univ. of Pennsylvania, Philadelphia, Pa. (20). 1878.
Worthen, A. H., Springfield, Ill. (5). 1874.
Wright, Prof. Albert A., Oberlin College, Oberlin, Ohio (24). 1880.
Wright, Prof. Arthur W., Yale College, New Haven, Conn. (14). 1874.
Wright, Rev. Geo. F., Oberlin College, Oberlin, Ohio (29). 1882.
Würtele, Rev. Louis C., Acton Vale, Province of Quebec, Can. (11). 1875.
Wyckoff, Wm. C., 44 Howard St., New York, N. Y. (20). 1874.
Wylie, Prof. Theoph. A., Indiana Univ., Bloomington, Ind. (20). 1874.

Youmans, Prof. Edward L., New York, N. Y. (6). 1874.
Young, C. A., Prof. of Astronomy, College of New Jersey, Princeton, N. J. (18). 1874.

Zentmayer, Joseph, 147 So. Fourth St., Philadelphia, Pa. (29). 1882.

[559 FELLOWS.]

TOTAL NUMBER OF MEMBERS OF THE ASSOCIATION, 2033.

DECEASED MEMBERS.

[Information respecting omissions in this list, and the date of birth and of decease of any of the former members, is requested by the Permanent Secretary.]

Abbe, George W., New York, N. Y. (23). Died Sept. 25, 1879.
Abert, J. J., Washington, D. C. (1). Born in 1785. Died January 27, 1863.
Adams, C. B., Amherst, Mass. (1). Born January 11, 1814. Died Jan'y
Adams, Edwin F., Charlestown, Mass. (18). [19, 1853.
Adams, Samuel, Jacksonville, Ill. (18). Born Dec. 19, 1806. Died April 29, 1877.
Agassiz, Louis, Cambridge, Mass. (1). Born May 28, 1807. Died Dec.
Ainsworth, J. G., Barry, Mass. (14). [14, 1873.
Alexander, Stephen, Princeton, N. J. (1). Born Sept. 1, 1806. Died June 25, 1883.
Allen, Thomas, St. Louis, Mo. (27). Died April 8, 1882.
Allen, Zachariah, Providence, R. I. (1). Died March 17, 1882.
Allston, R. F. W., Georgetown, S. C. (3). Born April 21, 1801. Died April 7, 1864.
Ames, M. P., Springfield, Mass. (1). Born in 1803. Died April 23, 1847.
Andrews, Ebenezer B., Lancaster, Ohio (7). Died Aug. 21, 1880, aged 59.
Anthony, Charles H., Albany, N. Y. (6). Died in 1874.
Appleton, Nathan, Boston, Mass. (1). Born Oct. 6, 1779. Died July 14
Armstrong, John W., Fredonia, N. Y. (24). [1861.
Atwater, Mrs. S. T., Chicago, Ill. (17). Born Aug. 8, 1812. Died April 11,
Aufrecht, Louis, Cincinnati, Ohio (30). [1878.

Bache, Alexander D., Washington, D. C. (1). Born July 19, 1806. Died Feb. 17, 1867.
Bache, Franklin, Philadelphia, Pa. (1). Born Oct. 25, 1792. Died March 19, 1864.
Bailey, Jacob W., West Point, N. Y. (1). Born April 29, 1811. Died Feb. 26, 1857.
Bardwell, F. W., Lawrence, Kan. (13). Died in 1878.
Barnard, John G., New York, N. Y. (14). Died May 14, 1882.
Barrett, Moses, Milwaukee, Wis. (21). Died in 1873.
Barry, Redmond, Melbourne, Australia (25).
Beck, C. F., Philadelphia, Pa. (1).
Beck, Lewis C., New Brunswick, N. J. (1). Born Oct. 4, 1798. Died April 21, 1853.
Beck, T. Romeyn, Albany, N. Y. (1). Born Aug. 11, 1791. Died Nov. 19, 1855.

Belfrage, G. W., Clifton, Texas (29). Died Dec. 7, 1882.
Belt, Thomas, London, Eng. (27). Died Sept. 8, 1878.
Benedict, George W., Burlington, Vt. (16). Born Jan. 11, 1796. Died in 1871.
Bicknell, Edwin, Boston, Mass. (18). Born in 1830. Died March 19,1877.
Binney, Amos, Boston, Mass. (1). Born Oct. 18, 1803. Died Feb. 18,
Binney, John, Boston, Mass. (3). [1847.
Blackie, Geo. S., Nashville, Tenn. (26).
Blake, Homer C., New York, N. Y. (28). Born Feb. 1, 1822.
Blanding, William, ———, R. I. (1).
Blatchford, Thomas W., Troy, N. Y. (6).
Blatchley, Miss S. L., New Haven, Conn. (19).
Boadle, John, Haddonfield, N. J. (20). Born in 1805. Died in July, 1878.
Bomford, George, Washington, D. C. (1). Born 1780. Died March 25, 1848.
Bowron, James, South Pittsburg, Tenn. (26). Died in Dec., 1877.
Bradley, Leverette, Jersey City, N. J. (15). Died in 1875.
Braithwaite, Jos., Chambly, C. W. (11).
Briggs, Albert D., Springfield, Mass. (13). Died Feb. 20, 1881.
Briggs, Robert, Philadelphia, Pa. (29). Born May 18, 1822. Died July 24, 1882.
Brigham, Charles H., Ann Arbor, Mich. (17). Born July 27, 1820. Died in Jan., 1879.
Brown, Andrew, Natchez, Miss. (1).
Brown, Horace, Salem, Mass. (27).
Burbank, L. S., Woburn, Mass. (18). [1859.
Burnap, G. W., Baltimore, Md. (12). Born Nov. 30, 1802. Died Sept. 8,
Burnett, Waldo I., Boston, Mass. (1). Died July 1, 1854, aged 27.
Butler, Thomas B., Norwalk, Conn. (10). Born Aug. 22, 1806. Died June 8, 1873.

Cairns, F. A., New York, N. Y. (27). Died in 1879.
Campbell, Mrs. Mary H., Crawfordsville, Ind. (22). Died Feb. 27, 1882.
Carpenter, Thornton, Camden, S. C. (7).
Carpenter, William M., New Orleans, La. (1).
Case, Leonard, Cleveland, Ohio (15). Born June 27, 1820. Died Jan. 5,
Case, William, Cleveland, Ohio (6). [1880.
Caswell, Alexis, Providence, R. I. (2). Born Jan. 29, 1799. Died Jan. 8, 1877.
Chadbourne, Paul Ansel, Amherst, Mass. (10). Born Oct. 21, 1823. Died Feb. 23, 1883.
Chapman, N., Philadelphia, Pa. (1). Born May 28, 1780. Died July 1, 1853.
Chase, Stephen, Hanover, N. H. (2). Born in 1813. Died Aug. 5, 1851.
Chauvenet, William, St. Louis, Mo. (1). Born May 24, 1819. Died Dec.
Cheney, Miss Margaret S., Jamaica Plain, Mass. (29). [13, 1870.
Clapp, Asahel, New Albany, Ind. (1). Born Oct. 5, 1792. Died Dec. 15, 1862.

Clark, Henry James, Cambridge, Mass. (13). Died July 1, 1873, aged 47.
Clark, Joseph, Cincinnati, Ohio (5).
Clarke, A. B., Holyoke, Mass. (13).
Cleaveland, C. H., Cincinnati, Ohio (9).
Cleveland, A. B., Cambridge, Mass. (2).
Coffin, James H., Easton, Pa. (1). Born Sept. 6, 1806. Died Feb. 6, 1873.
Cole, Thomas, Salem, Mass. (1). Born Dec. 24, 1779. Died June 24, 1852.
Coleman, Henry, Boston, Mass. (1).
Collins, Frederick, Washington, D. C. (28). Born Dec. 5, 1842. Died Oct. 27, 1881.
Conrad, Timothy Abbott, Philadelphia, Pa. (1). Born in August, 1803. Died August 9, 1877.
Cooke, Caleb, Salem, Mass. (18). Born Feb. 15, 1838. Died June 5, 1880.
Cooper, William, Hoboken, N. J. (9). Died in 1864.
Corning, Erastus, Albany, N. Y. (6). Born Dec. 14, 1794. Died April 9, 1872.
Couper, James Hamilton, Darien, Ga. (1). Born March 5, 1794. Died July 3, 1866.
Cramp, J. M., Wolfville, N. S. (11). Born July 25, 1796. Died Dec. 6, 1881.
Crocker, Charles F., Lawrence, Mass. (22). Died in July, 1881.
Crosby, Alpheus, Salem, Mass. (10). Born Oct. 13, 1810. Died Apr. 17, 1874.
Crosby, Thomas R., Hanover, N. H. (18). Born Oct. 22, 1816. Died March 1, 1872.
Croswell, Edwin, Albany, N. Y. (6). Born in May, 1797. Died June 13,
Curry, W. F., Geneva, N. Y. (11). [1871.
Curtis, Josiah, Washington, D. C. (18). Died Aug. 1, 1883.

Dalrymple, E. A., Baltimore, Md. (11). Died Oct. 30, 1881.
Dayton, Edwin A., Madrid, N. Y. (7). Born in 1827. Died June 24, 1873.
Dean, Amos, Albany, N. Y. (6). Born Jan. 16, 1803. Died Jan. 26, 1868.
Dearborn, George H. A. S., Roxbury, Mass. (1).
Dekay, James E., New York, N. Y. (1). Born in 1792. Died Nov. 21, 1851.
DeLaski, John, Carver's Harbor, Me. (18).
Dewey, Chester, Rochester, N. Y. (1). Born Oct. 25, 1781. Died Dec. 15,
Dexter, G. M., Boston, Mass. (11). [1867.
Dillingham, W. A. P., Augusta, Me. (17).
Doggett, Wm. E., Chicago, Ill. (17). Born Nov. 20, 1820. Died in 1876.
Doolittle, L., Lenoxville, C. E. (11). Died in 1862.
Dorr, E. P., Buffalo, N. Y. (25). Died March 28, 1881.
Draper, Henry, New York, N. Y. (28).
Ducatel, J. T., Baltimore, Md. (1).
Duffield, George, Detroit, Mich. (10). Born July 4, 1794. Died June 26, 1869.
Dumont, A. H., Newport, R. I. (14).
Duncan, Lucius C., New Orleans, La. (10). Died Aug. 9, 1855, aged 54.
Dunn, R. P., Providence, R. I. (14).

Easton, Norman, Fall River, Mass. (14). Died Dec. 21, 1872.
Eaton, James H., Beloit, Wis. (17). Died Jan. 5, 1877.
Ely, Charles Arthur, Elyria, Ohio (4).
Emerson, Geo. B., Boston, Mass. (1). Born Sept. 12, 1797. Died March 4, 1881.
Emmons, Ebenezer, Williamstown, Mass. (1). Born May 16, 1799. Died October 1, 1863.
Engstrom, A. B., Burlington, N. J. (1).
Everett, Edward, Boston, Mass. (2). Born April 11, 1794. Died Jan. 15, 1865.
Ewing, Thomas, Lancaster, Ohio (5). Born Dec. 28, 1789. Died Oct. 26, 1871.

Faries, R. J., Wauwatosa, Wis. (21). Died May 31, 1878.
Ferris, Isaac, New York, N. Y. (6). Born Oct. 9, 1798. Died June 16 1873.
Feuchtwanger, Lewis, New York, N. Y. (11). Died in 1876.
Fillmore, Millard, Buffalo, N. Y. (7). Born Jan. 7, 1800. Died March 8,
Fisher, Mark, Trenton, N. J. (10). [1874.
Fitch, Alexander, Hartford, Conn. (1). Born March 25, 1799. Died Jan. 20, 1859.
Fitch, O. H., Ashtabula, Ohio (7). Died Sept. 17, 1882, in his 80th year.
Forbush, E. B., Buffalo, N. Y. (15).
Force, Peter, Washington, D. C. (4). Born Nov. 26, 1790. Died Jan. 23, 1868.
Ford, A. C., Nashville, Tenn. (26).
Forshey, Caleb G., New Orleans, La. (21). Died in Aug., 1881.
Foster, John W., Chicago, Ill. (1). Born March 4, 1815. Died June 29, 1873.
Foucon, Felix, Madison, Wis. (18).
Fowle, Wm. B., Boston, Mass. (1). Born Oct. 17, 1795. Died Feb. 6,
Fox, Charles, Grosse Ile, Mich. (7). [1865.
Frazer, John F., Philadelphia, Pa. (1). Born July 8, 1812. Died Oct. 14,
French, J. W., West Point, N. Y. (11). [1872.

Garber, A. P., Columbia, Pa. (29). Died Aug. 26, 1881.
Gavit, John E., New York, N. Y. (1).
Gay, Martin, Boston, Mass. (1). Died Jan. 12, 1850, aged 46.
Gibbon, J. H., Charlotte, N. C. (3).
Gillespie, W. M., Schenectady, N. Y. (10). Born in 1816. Died Jan'y 1, 1868.
Gilmor, Robert, Baltimore, Md. (1).
Glazier, W. W., Key West, Fla. (29). Died Dec. 11, 1880.
Goldmark, J., New York, N. Y. (29). Died in April, 1882.
Gould, Augustus A., Boston, Mass. (11). Born April 23, 1805. Died Sept. 15, 1866.
Gould, B. A., Boston, Mass. (2). Born June 15, 1787. Died Oct. 24, 1859

Graham, James D., Washington, D. C. (1). Born in 1799. Died Dec. 28, 1865.
Gray, Alonzo, Brooklyn, N. Y. (13). Born in 1808. Died March 10, 1860.
Gray, James H., Springfield, Mass. (6).
Greene, Benjamin D., Boston, Mass. (1). Died Oct. 14, 1862, aged 68.
Greene, Everett W., Madison, N. J. (10). Died in 1864.
Greene, Samuel, Woonsocket, R. I. (9). Died in 1868.
Greer, James, Dayton, Ohio (20).
Griffith, Robert E., Philadelphia, Pa. (1).
Griswold, John A., Troy, N. Y. (19). Born in 1822. Died Oct. 31, 1872.
Guest, William E., Ogdensburg, N. Y. (6).

Hackley, Charles W., New York, N. Y. (4). Born March 9, 1809. Died January 10, 1861.
Hadley, George, Buffalo, N. Y. (6). Born June, 1813. Died Oct. 16, 1877.
Haldeman, S. S., Chickies, Pa. (1). Died Sept. 10, 1880, aged 68.
Hale, Enoch, Boston, Mass. (1). Born Jan. 29, 1790. Died Nov. 12, 1848.
Hance, Ebenezer, Fallsington P. O., Pa. (7). Died in 1876.
Hare, Robert, Philadelphia, Pa. (1). Born Jan. 17, 1781. Died May 15, 1858.
Harlan, Joseph G., Haverford, Pa. (8).
Harlan, Richard, Philadelphia, Pa. (1).
Harris, Thaddeus W., Cambridge, Mass. (1). Born Nov. 12, 1795. Died Jan. 16, 1856.
Harrison, A. M., Plymouth, Mass. (29).
Harrison, Jos., jr., Philadelphia, Pa. (12).
Hart, Simeon, Farmington, Conn. (1). Died April 20, 1853, aged 57.
Hartt, Charles F., Ithaca, N. Y. (18). Born in 1840. Died March 18, 1878.
Haven, Joseph, Chicago, Ill. (17). Born Jan. 4, 1816. Died May 23, 1874.
Hawes, George W., Washington, D. C. (23). Born Dec. 31, 1848. Died June 22, 1882.
Hayden, H. H., Baltimore, Md. (1).
Hayward, James, Boston, Mass. (1). Died July 27, 1866, aged 80.
Hempstead, G. S. B., Portsmouth, Ohio (29). Died July 9, 1883, in his 89th year.
Henry, Jos., Washington, D. C. (1). Born Dec. 17, 1797. Died May 13, 1878
Hickox, S. V. R., Chicago, Ill. (17). Died in 1872. [1875.
Hilgard, Theo. C., St. Louis, Mo. (17). Born Feb. 28, 1828. Died Mch. 5,
Hincks, William, Toronto, C. W. (11).
Hitchcock, Edward, Amherst, Mass. (1). Born May 24, 1793. Died Feb. 27, 1864.
Hodgson, W. B., Savannah, Ga. (10). Born 1815.
Holbrook, John E., Charleston, S. C. (1). Born Dec. 31, 1796. Died Sept. 8, 1871.
Hopkins, Albert, Williamstown, Mass. (19). Born July 14, 1807. Died May 25, 1872.
Hopkins, James G., Ogdensburg, N. Y. (10). Died in 1860.
Hopkins, T. O., Williamsville, N. Y. (10). Died in 1866.

Hopkins, Wm., Lima, N. Y. (5.) Died in March, 1867.
Horton, C. V. R., Chaumont, N. Y. (10). Died in 1862.
Horton, William, Craigville, N. Y. (1).
Hosford, Benj. F., Haverhill, Mass. (13). Died in 1864.
Houghton, Douglas, Detroit, Mich. (1). Born Sept. 21, 1809. Died Oct. 13, 1845.
Hovey, Edmund O., Crawfordsville, Ind. (20). Born July 15, 1801. Died March 10, 1877.
Howland, Theodore, Buffalo, N. Y. (15).
Hubbert, James, Richmond, Province of Quebec (16). Died in 1868.
Hunt, Edward B., Washington, D. C. (2). Born June 15, 1822. Died Oct. 2, 1863.
Hunt, Freeman, New York, N. Y. (11). Born March 21, 1804. Died March 2, 1858.

Ives, Moses B., Providence, R. I. (9). Died in 1857.
Ives, Thomas P., Providence, R. I. (10).

Jackson, Charles T., Boston, Mass. (1). Born June 21, 1805. Died Aug. 29, 1880.
James, Thomas Potts, Cambridge, Mass. (22). Born Sept. 1, 1803. Died Feb. 22, 1882.
Johnson, Walter R., Washington, D. C. (1). Born June 21, 1794. Died April 26, 1852.
Johnson, William Schuyler, Washington, D. C. (31). Born Sept. 20, 1859. Died Oct. 6, 1883.
Jones, Catesby A. R., Washington, D. C. (8).
Jones, James H., Boston, Mass. (28).

Kedzie, W. K., Oberlin, Ohio (25).
Keely, George W., Waterville, Me. (1). Died in 1878.
Keep, N. C., Boston, Mass. (13). Died in 1875.
Kennicott, Robert, West Northfield, Ill. (12). Born Nov. 13, 1835. Died in 1866.
King, Mitchell, Charleston, S. C. (3). Born June 8, 1783. Died in 1862.
Klippart, John H., Columbus, Ohio (17). Died October, 1878.
Knickerbocker, Charles, Chicago, Ill. (17). Died in 1873.
Knight, J. B., Philadelphia, Pa. (21). Died March 10, 1879.

Lacklan, R., Cincinnati, Ohio (11).
Lapham, Increase A., Milwaukee, Wis. (3). Born Mch. 7, 1811. Died
LaRoche, R., Philadelphia, Pa. (12). [Sept. 14, 1875.
Lasel, Edward, Williamstown, Mass. (1). Born Jan. 21, 1809. Died in 1852.
Lawford, Frederick, Montreal, Canada (11). Died in 1866.
Le Conte, John L., Philadelphia, Pa. (1). Died Nov. 15, 1883.
Lederer, Baron von, Washington, D. C. (1).
Lieber, Oscar M., Columbia, S. C. (8). Born Sept. 8, 1830. Died June
Lincklaen, Ledyard, Cazenovia, N. Y. (1). [27, 1862.

Linsley, James H., Stafford, Conn. (1). Born May 5, 1787. Died Dec. 26, 1843.
Lockwood, Moses B., Providence, R. I. (9). Died in 1872.
Logan, William E., Montreal, Canada (1). Born April 23, 1798. Died June 22, 1875.
Loosey, Charles F., New York, N. Y. (12).
Lothrop, Joshua R., Buffalo, N. Y. (15). [24, 1872.
Lyon, Sidney S., Jeffersonville, Ind. (20). Born Aug. 4, 1808. Died June

Maack, G. A., Cambridge, Mass. (18). [16, 1871.
Mahan, Dennis H., West Point, N. Y. (9). Born April 2, 1802. Died Sept.
Marsh, Dexter, Greenfield, Mass. (1).
Marsh, James E., Roxbury, Mass. (10). [27, 1859
Mather, William W., Columbus, Ohio (1). Born May 24, 1804. Died Feb
Maude, John B., St. Louis, Mo. (27). Died in April, 1879.
Maupin, S., Charlottesville, Va. (10).
M'Conihe, Isaac, Troy, N. Y. (5).
McFadden, Thomas, Westerville, Ohio. (30). Born Nov. 9, 1825. Died Nov. 9, 1883.
McMahon, Mathew, Albany, N. Y. (11).
Meade, George G., Philadelphia, Pa. (15). Born Dec. 30, 1815. Died Nov. 6, 1872.
Meek, F. B., Washington, D. C. (6). Born December 10, 1817. Died December 21, 1876.
Meigs, James Aitken, Philadelphia, Pa. (12). Born July 30, 1829. Died Nov. 9, 1879.
Minifie, William, Baltimore, Md. (12). Born in 1805. Died Oct. 24, 1880.
Mitchel, O. M., Cincinnati, Ohio (3). Born Aug. 28, 1810. Died Oct. 30, 1862.
Mitchell, William, Poughkeepsie, N. Y. (2). Died April 2, 1869, aged 76.
Mitchell, Wm. H., Florence, Ala. (17).
Morgan, Lewis H., Rochester, N. Y. (10). Born Nov. 21, 1818. Died Dec. 17, 1881.
Morris, John B., Nashville, Tenn. (26).
Morton, Samuel G., Philadelphia, Pa. (1). Born Jan. 26, 1799. Died May 15, 1851.
Mudge, Benjamin F., Manhattan, Kansas (25). Born Aug. 11, 1817. Died Nov. 21, 1879, aged 62.
Munroe, Nathan, Bradford, Mass. (6). Born May 16, 1804. Died July 8,
Munroe, William, Concord, Mass. (18). Died April 27, 1877. [1866.
Mussey, William H., Cincinnati, Ohio (30). Born Sept. 13, 1818. Died Aug. 1, 1882.

Newland, John, Saratoga Springs, N. Y. (28). Died Jan. 18, 1880.
Newton, E. H., Cambridge, N. Y. (1).
Nichols, Charles A., Providence, R. I. (17). Born Jan. 4, 1826. Died Oct. 20, 1877.

Nicholson, Thomas, New Orleans, La. (21).
Nicollett, Jean N., Washington, D. C. (1). Born July 24, 1786. Died Sept. 11, 1843.
Norton, John P., New Haven, Conn. (1). Born in July, 1822. Died Sept. 5, 1852.
Noyes, J. O., New Orleans, La. (21).
Nutt, Cyrus, Bloomington, Ind. (20). Died in 1875.

Oakes, Wm., Ipswich, Mass. (1). Born July 1, 1799. Died July 31, 1848.
Ogden, Robert W., New Orleans, La. (21). Died March 24, 1878.
Ogden, William B., High Bridge, Westchester Co., N. Y. (17). Born in 1805. Died Aug. 3, 1877.
Olmsted, Alexander F., New Haven, Conn. (4). Born Dec. 20, 1822. Died May 5, 1853.
Olmsted, Denison, New Haven, Conn. (1). Born June 18, 1791. Died May 13, 1859.
Olmsted, Denison, jr., New Haven, Conn. (1). Born Feb. 16, 1824. Died Aug. 15, 1846.
Otis, George Alexander, Washington, D. C. (10). Born Nov. 12, 1830. Died Feb. 23, 1881.

Painter, Jacob, Lima, Pa. (23). Died in 1876.
Painter, Minshall, Lima, Pa. (7).
Parker, Wilbur F., West Meriden, Conn. (23). Died in 1876.
Parkman, Samuel, Boston, Mass. (1). Died Dec. 15, 1854, aged 38.
Payn, Charles H., Saratoga Springs, N. Y. (28). Born May 16, 1814. Died Dec. 20, 1881.
Peirce, B. O., Beverly, Mass. (18). Died Nov. 12, 1883, aged 71 years.
Peirce, Benjamin, Cambridge, Mass. (1). Born April 4, 1809. Died Oct. 6, 1880.
Perkins, George R., Utica, N. Y. (1). Born May 3, 1812. Died Aug. 22, 1876.
Perkins, Henry C., Newburyport, Mass. (18). Born Nov. 13, 1804. Died Feb. 2, 1873.
Perry, John B., Cambridge, Mass. (16). Died Oct. 3, 1872, aged 52.
Perry, M. C., New York, N. Y. (10).
Piggot, A. Snowden, Baltimore, Md. (10).
Plumb, Ovid, Salisbury, Conn. (9). [1870.
Pope, Charles A., St. Louis, Mo. (12). Born May 15, 1818. Died July 6,
Porter, John A., New Haven, Conn. (14). Born March 15, 1822. Died Aug. 25, 1866.
Pourtalès, Louis François de, Cambridge, Mass. (1). Born in 1822. Died July 18, 1880.
Pruyn, John V. L., Albany, N. Y. (1). Born June 22, 1811. Died Nov. 21, 1877.
Pugh, Evan, Centre Co., Pa. (14).
Putnam, Mrs. F. W., Cambridge, Mass. (19). Born Dec. 29, 1838. Died March 10, 1879.

Putnam, J. Duncan, Davenport, Iowa. (27). Born Oct. 18, 1855. Died Dec. 10, 1881.

Read, Ezra, Terre Haute, Ind. (20). Died in 1877.
Redfield, William C., New York, N. Y. (1). Born March 26, 1789. Died Feb. 12, 1857.
Resor, Jacob, Cincinnati, Ohio (8). Died in 1871.
Robb, James, Fredericton, N. B. (4).
Robinson, Coleman T., Buffalo, N. Y. (15).
Rockwell, John A., Norwich, Conn. (10). Born August 27, 1803. Died February 10, 1861.
Rogers, Henry D., Glasgow, Scotland (1). Born 1809. Died May 29, 1866.
Rogers, James B., Philadelphia, Pa. (1). Born February 22, 1803. Died June 15, 1852.
Rogers, William Barton, Boston, Mass. (1). Born Dec. 7, 1804. Died May 30, 1882.
Root, Elihu, Amherst, Mass. (25). Born Sept. 14, 1845.

Sager, Abram, Ann Arbor, Mich. (6). Born December 22, 1810. Died August 6, 1877.
Schaeffer, Geo. C., Washington, D. C. (1). Died in 1873.
Schley, William, New York, N. Y. (28). Died in 1882.
Scott, Joseph, Dunham, C. E. (11). Died in 1865.
Senter, Harvey S., Aledo, Ill. (20). Died in 1875. [10, 1872.
Seward, William H., Auburn, N. Y. (1). Born May 16, 1801. Died Oct.
Sheppard, William, Drummondville, Province of Quebec, Can. (11). Born in 1783. Died in 1867.
Sherwin, Thomas, Dedham, Mass. (11). Born March 26, 1799. Died July 23, 1869.
Silliman, Benjamin, New Haven, Conn. (1). Born August 8, 1779. Died November 22, 1864.
Skinner, John B., Buffalo, N. Y. (15). Died in 1871.
Slack, J. H., Philadelphia, Pa. (12). [26, 1880.
Smith, David P., Springfield, Mass. (29). Born Oct. 1, 1830. Died Dec.
Smith, J. Lawrence, Louisville, Ky. (1). Born Dec. 16, 1818. Died Oct. 1883.
Smith, J. V., Cincinnati, Ohio (5).
Smith, James Y., Providence, R. I. (9). Born September 15, 1809. Died in 1876.
Smith, Lyndon A., Newark, N. J. (9). Born November 11, 1795. Died December 15, 1865.
Snell, Ebenezer S., Amherst, Mass. (2). Born October 7, 1801. Died in September, 1877. [14, 1866.
Sparks, Jared, Cambridge, Mass. (2). Born May 10, 1789. Died March
Spinzig, Charles, St. Louis, Mo. (27). Died Jan. 22, 1882.
Stimpson, William, Chicago, Ill. (12). Died May 26, 1872.
Stone, Samuel, Chicago, Ill. (17). Born Dec. 6, 1798. Died May 4, 1876.

St. John, Joseph S., Albany, N. Y. (28). Died Nov. 28, 1882.
Sullivant, Wm. S., Columbus, Ohio (7). Born Jan. 15, 1803. Died April 30, 1873.
Swain, James, Fort Dodge, Iowa (21). Born in 1816. Died in 1877.

Tallmadge, James, New York, N. Y. (1). Born Jan. 20, 1778. Died Oct. 3, 1853.
Taylor, Richard C., Philadelphia, Pa. (1). Born January 18, 1789. Died November 26, 1851.
Tenney, Sanborn, Williamstown, Mass. (17). Born in January, 1827. Died July 11, 1877.
Teschemacher, J. E., Boston, Mass. (1). Died Nov. 9, 1853, aged 63.
Thompson, A. Remsen, New York, N. Y. (1). Died in Oct., 1879.
Thompson, Alexander, Aurora, N. Y. (1).
Thompson, Zadock, Burlington, Vt. (1). Born May 23, 1796. Died Jan.
Thurber, Isaac, Providence, R. I. (9). [19, 1856.
Tillman, Samuel D., Jersey City, N. J. (15). Died in 1875.
Tobin, Thomas W., Louisville, Ky. (30). Died Aug. 4, 1883.
Tolderoy, James B., Fredericton, N. B. (11).
Torrey, John, New York, N. Y. (1). Born August 15, 1796. Died March 10, 1873.
Torrey, Joseph, Burlington, Vt. (2). Born Feb. 2, 1797. Died Nov. 26, 1867.
Totten, Joseph G., Washington, D. C. (1). Born August 23, 1788. Died April 22, 1864.
Townsend, Howard, Albany, N. Y. (10). Died in 1867.
Townsend, John K., Philadelphia, Pa. (1).
Townsend, Robert, Albany, N. Y. (9). Died in 1866.
Troost, Gerard, Nashville, Tenn. (1). Born March 15, 1776. Died Aug. 14, 1850.
Tuomey, Michael, Tuscaloosa, Ala. (1). Born September 29, 1805. Died March 20, 1857.
Tyler, Edward R., New Haven, Conn. (1). Died Sept. 28, 1848.

Vancleve, John W., Dayton, Ohio (1).
Vanuxem, Lardner, Bristol, Pa. (1).
Vaux, William S., Philadelphia, Pa. (1). Born May 19, 1811. Died May 5, 1882.

Wadsworth, James S., Genesee, N. Y. (2). Born October 30, 1807. Died May 8, 1864.
Wagner, Tobias, Philadelphia, Pa. (9).
Walker, Joseph, Oxford, N. Y. (10).
Walker, Sears C., Washington, D. C. (1). Born March 28, 1805. Died January 30, 1853.
Walker, Timothy, Cincinnati, Ohio (4). Born Dec. 1, 1802. Died Jan. 15, 1856.
Walsh, Benjamin D., Rock Island, Ill. (17).

Wanzer, Ira, Brookfield, Conn. (18). Born April 17, 1796. Died March 5, 1879.

Warren, Gouverneur Kemble, Newport, R. I. (12). Died Aug. 8, 1882, in his 64th year.

Warren, John C., Boston, Mass. (1). Born Aug. 1, 1778. Died May 4, 1856.

Watertown, Charles, Wakefield, Eng. (1).

Watkins, Samuel, Nashville, Tenn. (26).

Watson, J. Craig, Ann Arbor, Mich. (18). Born Jan. 28, 1838. Died Nov. 23, 1880.

Webster, Horace B., Albany, N. Y. (1). Died Dec. 8, 1843.

Webster, J. W., Cambridge, Mass. (1). Died Aug. 30, 1850, aged 57.

Webster, M. H., Albany, N. Y. (1).

Weed, Monroe, Wyoming, N. Y. (6). Died in 1867.

Welch, Mrs. G. O., Lynn, Mass. (21). Died in June, 1882.

Weyman, George W., Pittsburgh, Pa. (6). Born April, 1832. Died July 16, 1864.

Wheatley, Charles M., Phœnixville, Pa. (1). Died May 6, 1882.

Wheatland, Richard H., Salem, Mass. (13). Born July 6, 1830. Died Dec. 21, 1863.

Wheeler, Arthur W., Baltimore, Md. (29). Born in March, 1859. Died Jan. 6, 1881.

White, Samuel S., Philadelphia, Pa. (23). Died Dec. 30, 1879.

Whiting, Lewis E., Saratoga Springs, N. Y. (28). Born March 7, 1815. Died Aug. 2, 1882.

Whitman, Wm. E., Philadelphia, Pa. (23). Died in 1875.

Whitney, Asa, Philadelphia, Pa. (1). Born Dec. 1, 1791. Died June 4, 1874.

Whittlesey, Charles C., St. Louis, Mo. (11). Died in 1872.

Willard, Emma, Troy, N. Y. (15). Born Feb. 23, 1787. Died April 15, 1870.

Williamson, R. S., San Francisco, Cal. (12).

Wilson, W. C., Carlisle, Pa. (12).

Winlock, Joseph, Cambridge, Mass. (5). Born Feb. 6, 1826. Died June 11, 1875.

Woodbury, Levi, Portsmouth, N. H. (1). Born Dec. 22, 1789. Died Sept. 4, 1851.

Woodman, John S., Hanover, N. H. (11). Born in 1819. Died May 15, 1871.

Wright, John, Troy, N. Y. (1).

Wyman, Jeffries, Cambridge, Mass. (1). Born Aug. 11, 1814. Died Sept. 4, 1874.

Yarnall, M., Washington, D. C. (26). Born in 1817. Died Jan. 27, 1879.

Young, Ira, Hanover, N. H. (1). Died Sept. 14, 1858, aged 57.

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BY

J. W. DAWSON, LL. D.,

THE RETIRING PRESIDENT OF THE ASSOCIATION.

ON SOME UNSOLVED PROBLEMS IN GEOLOGY.

Ladies and Gentlemen of the American Association for the Advancement of Science:—

My predecessor in office remarked in the opening of his address that two courses are open to the retiring president of this Association in preparing the annual presidential discourse. He may either take up some topic relating to his own specialty, or he may deal with various or general matters relating to science and its progress. A geologist, however, is not necessarily tied up to one or the other alternative. His subject covers the whole history of the earth in time. At the beginning it allies itself with astronomy and physics and celestial chemistry. At the end it runs into human history and is mixed up with archæology and anthropology. Throughout its whole course it has to deal with questions of meteorology, geography and biology. In short, there is no department of physical or biological science, with which geology is not allied, or at least on which the geologist may not presume to trespass. When, therefore, I announce as my subject on the present occasion some of the unsolved problems of this universal science, you need not be surprised if I should be somewhat discursive.

Perhaps I shall begin at the utmost limits of my subject by remarking that in matters of natural and physical science, we are met at the outset with the scarcely solved question as to our own place in the nature which we study, and the bearing of this on the difficulties we encounter. The organism of man is decidedly a part of nature. We place ourselves, in this aspect, in the sub-kingdom vertebrata and class mammalia, and recognize the fact that man is the terminal link in a chain of being, extending throughout geological time. But the organism is not all of man, and when we regard man as a scientific animal, we raise a new question. If the human mind is a part of nature then it is subject to natural law, and nature includes mind as well as matter. On the other hand, without being absolute idealists we may hold that mind is more potent than matter, and nearer to the real essence of things. Our science is in any case necessarily dualistic, being the product of the reaction of mind on nature, and must be largely subjective and anthropomorphic. Hence, no doubt, arise much of the controversy of science and much of the unsolved difficulty. We recognize this when we divide science into that which is experimental or depends on apparatus, and that which is observational and classificatory — distinctions these which relate not so much to the objects of science as to our methods of pursuing them. This view also opens up to us the thought that the domain of science is practically boundless, for who can set limits to the action of mind on the universe or of the universe on mind. It follows that science must be limited on all sides by unsolved mysteries; and it will not serve any good purpose to meet these with clever guesses. If we so treat the enigmas of the sphinx nature, we shall surely be devoured. Nor, on the other hand, must we collapse into absolute despair and resign ourselves to the confession of inevitable ignorance. It becomes us rather boldly to confront the unsolved questions of nature, and to wrestle with their difficulties till we master such as we can, and cheerfully leave those we cannot overcome to be grappled with by our successors.

Fortunately, as a geologist, I do not need to invite your attention to those transcendental questions which relate to the ultimate constitution of matter, the nature of the ethereal medium filling space, the absolute difference or identity of chemical elements, the cause of gravitation, the conservation and dissipation of energy, the nature of life, or the primary origin of bioplasmic matter. I may take the much more humble role of an inquirer into the unsolved or partially solved problems which meet us in considering that short and imperfect record which geology studies in the rocky layers of the earth's crust, and which leads no farther back than to the time when a solid rind had already formed on the earth and was already covered with an ocean. This record of geology covers but a small part of the history of the earth and of the system to which it belongs, nor does it enter at all into the more recondite problems involved; still it forms, I believe, some necessary preparation at least to the comprehension of these.

What do we know of the oldest and most primitive rocks? At this moment the question may be answered in many and discordant ways; yet the leading elements of the answer may be given very simply. The oldest rock formation known to geologists is the Lower Laurentian, the fundamental gneiss, the Lewisian formation of Scotland, the Ottawa gneiss of Canada. This formation, of enormous thickness, corresponds to what the older geologists called the fundamental granite, a name not to be scouted, for gneiss is only a stratified granite. Perhaps the main fact in relation to this old rock is that it is a gneiss, that is, a rock at once bedded and crystalline, and having for its dominant ingredient the mineral orthoclase, a compound of silica, alumina and potash, in which are embedded, as in a paste, grains and crystals of quartz and hornblende. We know very well from its texture and composition that it cannot be a product of mere heat, and being a bedded rock we infer that it was laid down layer by layer in the manner of aqueous deposits. On the other hand, its chemical composition is quite different from that of the muds, sands and gravels usually deposited from water. Their special characters are caused by the fact that they have resulted from the slow decay of rocks like these gneisses, under the operation of carbonic acid and water, whereby the alkaline matter and the more soluble part of the silica have been washed away, leaving a residue mainly silicious and aluminous. Such more modern rocks tell of dry land subjected to atmospheric decay and rain-wash. If they have any direct relation to the old gneisses they are their grandchildren, not their parents. On the contrary, the oldest gneisses show no pebbles or sand or limestone — nothing to indicate that there was then any land undergoing atmospheric waste, or shores with sand and gravel. For all that we know to the contrary, these old gneisses may have been deposited in a shoreless sea, holding in solution or suspension merely what it could derive from a submerged crust recently cooled from a state of fusion, still thin, and exuding here and there through its fissures heated waters and volcanic products.

It is scarcely necessary to say that I have no confidence in the supposition of unlike composition of the earth's mass on different sides on which Dana has partly based his theory of the origin of continents. The most probable conception seems to be that of Lyell, namely, a molten mass, uniform except in so far as denser material might exist toward its centre, and a crust at first approximately even and homogeneous, and subsequently thrown into great bendings upward and downward. This question has recently been ably discussed by Mr. Crosby in the London Geological Magazine.[1]

In short, the fundamental gneiss of the Lower Laurentian may have been the first rock ever formed; and in any case it is a rock formed under conditions which have not since recurred except locally. It constitutes the first and best example of those chemico-physical, aqueous or aqueo-igneous rocks, so characteristic of the earliest period of the earth's history. Viewed in this way the Lower Laurentian gneiss is probably the oldest kind of rock we shall ever know — the limit to our backward progress, beyond which there remains nothing to the geologist except physical hypotheses respecting a cooling incandescent globe. For the chemical conditions of these primitive rocks, and what is known as to their probable origin, I must refer you to my friend Dr. Sterry Hunt, to whom we owe so much of what is known of the older crystalline rocks[2] as well as of their literature, and the questions which they raise. My purpose here is to sketch the remarkable difference which we meet as we ascend into the Middle and Upper Laurentian.

In the next succeeding formation, the true Lower Laurentian of Logan, the Grenville series of Canada, we meet with a great and significant change. It is true we have still a predominance of gneisses which may have been formed in the same manner with those below them; but we find these now associated with great beds of limestone and dolomite, which must have been formed by the separation of calcium and magnesium carbonates from the sea water, either by chemical precipitation or by the agency of living beings. We have also quartzite, quartzose gneisses, and even pebble beds, which inform us of sand banks and shores. Nay, more, we have beds containing graphite which must be the residue of plants, and iron ores which tell of the deoxidation of iron

[1] June, 1883. [2] Hunt, Essays on Chemical Geology.

oxide by organic matters. In short, here we have evidence of new factors in world-building, of land and ocean, of atmospheric decay of rocks, of deoxidizing processes carried on by vegetable life on the land and in the waters, of limestone-building in the sea. To afford material for such rocks, the old Ottawa gneiss must have been lifted up into continents and mountain masses. Under the slow but sure action of the carbonic dioxide dissolved in rain-water, its felspar had crumbled down in the course of ages. Its potash, soda, lime, magnesia and part of its silica had been washed into the sea, there to enter into new combinations and to form new deposits. The crumbling residue of fine clay and sand had been also washed down into the borders of the ocean, and had been there deposited in beds[3]. Thus the earth had entered into a new phase, which continues onward through the geological ages; and I place in your hands one key for unlocking the mystery of the world when I affirm that this great change took place, this new era of modern causes was inaugurated in the midst of the Laurentian period.

Was not this time a fit period for the first appearance of life? Should we not expect it to appear, independently of the evidence we have of the fact? I do not propose to enter here into that evidence, more especially in the case of the one well characterized Laurentian fossil, *Eozoon Canadense*. I have already amply illustrated it elsewhere. I would merely say here that we should bear in mind that in this later half of the Lower Laurentian, or if we so choose to style it, Middle Laurentian period, we have the conditions required for life in the sea and on the land; and since in other periods we know that life was always present when its conditions were present, it is not unreasonable to look for the first traces of life in this formation, in which we find for the first time the completion of those physical arrangements which make life, in such forms of it as exist on our planet, possible.

This is also a proper place to say something of the doctrine of what is termed metamorphism. The Laurentian rocks are undoubtedly greatly changed from their original state, more especially in the matters of crystallization and the formation of disseminated minerals, by the action of heat and heated water. Sandstones have thus passed into quartzites, clays into slates and schists, limestones into marbles. So far, metamorphism is not a doubtful question; but when theories of metamorphism go so far as to suppose an actual change of one element for another, they go beyond the bounds of chemical credibility; yet such theories of metamorphism are often boldly advanced and made the basis of important conclusions. Dr. Hunt has happily given the name "metasomatosis" to this imaginary and impossible kind of metamorphism, which may be regarded as an extreme kind of évolution, akin to some of those forms of that theory employed with reference to life, but more easily detected and exposed. I would have it to be understood that, in speaking of the metamorphism of the older crystalline rocks, it is not to this metasomatosis that I refer, and that I hold that rocks which have been produced out of the materials decomposed by atmospheric erosion can never by any process of metamorphism be restored to the precise condition of the Laurentian rocks. Thus there is in the older formations a genealogy of rocks, which, in the absence of fossils, may be used with some confidence, but which does not apply to the more modern deposits. Still nothing in geology absolutely perishes, or is altogether discontinued; and it is probable that, down to the present day, the causes which produced the old Laurentian gneiss may still operate in limited localities. Then, however, they were general not exceptional. It is further to be observed that the term gneiss is sometimes of wide and even loose application. Beside the typical orthoclase and hornblendic gneiss of the Laurentian, there are micaceous, quartzose, garnetiferous and many other kinds of gneiss; and even gneissose rocks, which hold labradorite or anorthite instead of orthoclase, are sometimes, though not accurately, included in the term.

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[3] Dr. Hunt has now in preparation for the press an important paper on this subject, read before the National Academy of Sciences.

The Grenville series, or Middle Laurentian is succeeded by what Logan in Canada called the Upper Laurentian, and which other geologists have called the Norite or Norian series. Here we still have our old friends the gneisses, but somewhat peculiar in type, and associated with them are great beds, rich in lime-felspar, the so-called labradorite and anorthite rocks. The precise origin of these is uncertain, but this much seems clear, namely, that they originated in circumstances in which the great limestones deposited in the Lower or Middle Laurentian were beginning to be employed in the manufacture, probably by aqueo-igneous agencies, of lime-felspars. This proves the Norian rocks to be much younger than the Laurentian, and that, as Logan supposed, considerable earth movements had occurred between the two, implying lapse of time.

Next we have the Huronian of Logan, a series much less crystalline and more fragmentary, and affording more evidence of land elevation and atmospheric and aqueous erosion than any of the others. It has great conglomerates, some of them made up of rounded pebbles of Laurentian rocks, and others of quartz pebbles, which must have been the remains of rocks subjected to very perfect erosion. The pure quartz-rocks tell the same tale, while limestones and slates speak also of chemical separation of the materials of older rocks. The Huronian evidently implies movements in the previous Laurentian, and changes in its texture so great, that the former may be regarded as a comparatively modern rock, though vastly older than any part of the Palæozoic series.

Still later than the Huronian, is the great Micaceous series, called by Hunt the Mont Alban or White mountain group, and the Taconian or Lower Taconic of Emmons, which recalls in some measure the conditions of the Huronian. The precise relations of these to the later formations and to certain doubtful deposits around Lake Superior, can scarcely be said to be settled, though it would seem that they are all older than the fossiliferous Cambrian rocks, which practically constitute the base of the Palæozoic. I have, I may say, satisfied myself, in regions which I have studied, of the existence and order of these rocks as successive formations, though I would not dogmatize as to the precise relations of those last mentioned, or as to the precise age of some disputed formations which may either be of the age of the older Eozoic formations or may be peculiar kinds of Palæozoic rocks modified by metamorphism. Probably neither of the extreme views now agitated is absolutely correct.

After what has been said, you will perhaps not be astonished that a great geological battle rages over the old crystalline rocks. By some geologists they are almost entirely explained away or referred to igneous action or to the alteration of ordinary sediments. Under the treatment of another school, they grow to great series of Pre-Cambrian rocks, constituting vast systems of formations, distinguishable from each other, not by fossils, but by differences of mineral character. I have already indicated the manner in which I believe the dispute will ultimately be settled, and the President of the Geological Section will treat it more fully in his opening address.

After the solitary appearance of Eozoon in the Laurentian, and of a few uncertain forms in the Huronian and Taconian, we find ourselves, in the Cambrian, in the presence of a nearly complete invertebrate fauna of protozoa, polyps, echinoderms, mollusks and crustacea, and this not confined to one locality merely, but apparently extended simultaneously throughout the ocean. This sudden incoming of animal life, along with the subsequent introduction of successive groups of invertebrates, and finally of vertebrate animals, furnishes one of the greatest of the unsolved problems of geology, which geologists were wont to settle by the supposition of successive creations. In an address delivered at the Detroit meeting of the Association in 1875, I endeavored to set forth the facts as to this succession, and the general principles involved in it, and to show the insufficiency of the theories of evolution suggested by biologists to give any substantial aid to the geologist in these questions. In looking again at the points there set forth, I find they have not been invalidated by subsequent discoveries, and that we are still nearly in the same position with respect to these great questions that we were in at that time,— a singular proof of the impotency of that deductive method of reasoning which has become fashionable among naturalists of late. Yet the discussions of recent years have thrown some additional light on these matters, and none more so than the mild disclaimers with which my friend Dr. Asa Gray and other moderate and scientific evolutionists have met the extreme views of such men as Romanes, Hæckel, Lubbock and Grant Allen. It may be useful to note some of these, as shedding a little light on this dark corner of our unsolved problems.

It has been urged on the side of rational evolution that this hypothesis does not profess to give an explanation of the absolute origin of life on our planet, or even of the original organization of a single cell or of a simple mass of protoplasm, living or dead. All experimental attempts to produce by synthesis the complex albuminous substances or to obtain the living from the non-living, have so far been fruitless, and indeed we cannot imagine any process by which such changes could be effected. That they have been effected we know, but the process employed by their maker is still as mysterious to us as it probably was to him who wrote the words:—"And God said let the waters swarm with swarmers." How vast is the gap in our knowledge and our practical power implied in this admission, which must however be made by every mind not absolutely blinded by a superstitious belief in those forms of words which too often pass current as philosophy.

But if we are content to start with a number of organisms ready made—a somewhat humiliating start however—we still have to ask —How do these vary so as to give new species? It is a singular illusion in this matter, of men who profess to be believers in natural law, that variation may be boundless, aimless and fortuitous, and that it is by spontaneous selection from varieties thus produced that development arises. But surely the supposition of mere chance and magic is unworthy of science. Varieties must have causes, and these causes and their effects must be regulated by some law or laws. Now it is easy to see that they cannot be caused by a mere innate tendency in the organism itself. Every organism is so nicely equilibrated that it has no such spontaneous tendency, except within the limits set by its growth and the law of its periodical changes. There may, however, be equilibrium more or less stable. I believe all attempts hitherto made have failed to account for the fixity of certain, nay of very many, types throughout geological time, but the mere consideration that one may be in a more stable state of equilibrium than another, so far explains it. A rocking stone has no more spontaneous tendency to move than an ordinary boulder, but it may be made to move with a touch. So it probably is with organisms. But if so, then the causes of variation are external, as in many cases we actually know them to be, and they must depend on instability or change in surroundings, and this so arranged as not to be too extreme in amount and to operate in some determinate direction. Observe how remarkable the unity of the adjustments involved in such a supposition! how superior they must be to our rude and always more or less unsuccessful attempts to produce and carry forward varieties and races in definite directions! This cannot be chance. If it exists it must depend on plans deeply laid in the nature of things, else it would be most monstrous magic and causeless miracle. Still more certain is this conclusion when we consider the vast and orderly succession made known to us by geology, and which must have been regulated by fixed laws, only a few of which are as yet known to us.

Beyond these general considerations, we have others of a more special character, based on palæontological facts, which show how imperfect are our attempts as yet to reach the true causes of the introduction of genera and species.

One is the remarkable fixity of the leading types of living beings in geological time. If, instead of framing, like Hæckel, fanciful phylogenies, we take the trouble with Barrande and Gaudry, to trace the forms of life through the period of their existence, each along its own line, we shall be greatly struck with this, and especially with the continuous existence of many low types of life through vicissitudes of physical conditions of the most stupendous character, and over a lapse of time scarcely conceivable. What is still more remarkable is that this holds in groups which, within certain limits, are perhaps the most variable of all. In the present world, no creatures are individually more variable than the protozoa; as, for example, the foraminifera and the sponges. Yet these groups are fundamentally the same, from the beginning of the Palæozoic until now, and modern species seem scarcely at all to differ from specimens procured from rocks at least half-way back to the beginning of our geological record. If we suppose that the present sponges and foraminifera are the descendants of those of the Silurian period, we can affirm that in all that vast lapse of time, they have on the whole made little greater change than that which may be observed in variable forms at present. The same remark applies to other low animal forms. In forms somewhat higher and less variable, this is equally noteworthy. The pattern of the venation of the wings of cockroaches, and the structure and form of land snails, gally-worms and decapod crustaceans were all settled in the Carboniferous age, in a way that still remains. So were the foliage and the fructification of club-mosses and ferns. If, at any time, members of these groups branched off, so as to lay the foundation of new species, this must have been a very rare and exceptional occurrence and one demanding even some suspension of the ordinary laws of nature.

Certain recent utterances of eminent scientific men in England

and France, are most instructive with reference to the difficulties which encompass this subject. Huxley, at present the leader of English evolutionists, in his "Rede Lecture"[4] delivered at Cambridge, England, holds that there are only two "possible alternative hypotheses" as to the origin of species—(1) that of "construction" or the mechanical putting together of the materials and parts of each new species separately; and (2) that of "evolution," or that one form of life "proceeded from another" by the "establishment of small successive differences." After comparing these modes, much to the disadvantage of the first, he concludes with the statement that "this was his case for evolution, which he rested wholly on arguments of the kind he had adduced"—these arguments being the threadbare false analogy of ordinary reproduction and the transformation of species, and the mere succession of forms more or less similar in geological time, neither of them having any bearing whatever on the origin of any species or on the cause of the observed succession. With reference to the two alternatives, while it is true that no certain evidence has yet been obtained either by experiment observation or sound induction as to the mode of origin of any species, enough is known to show that there are numerous possible methods, grouped usually under the heads of absolute creation, mediate creation, critical evolution and gradual evolution. It is also true that almost the only thing we certainly know in the matter is that the differences characteristic of classes, orders, genera and species must have arisen, not in one or two, but in many ways. An instructive commentary on the capacity of our age to deal with these great questions is afforded by the fact that this little piece of clever mental gymnastic should have been practised in a university lecture and in presence of an educated audience. It is also deserving of notice that though the lecturer takes the development of the *Nautili* and their allies as his principal illustration, he evidently attaches no weight to the argument in the opposite sense deduced by Barrande, the man of all others most profoundly acquainted with these animals, from the Palæozoic cephalopods.[5]

Another example is afforded by a lecture recently delivered at the Royal Institution in London by Prof. Flower.[6] The subject is, "The Whales, past and present, and their probable origin." The latter point, as is well known, Gaudry had candidly given up. "We have questioned," he says, "these strange and gigantic sovereigns of the Tertiary oceans as to their ancestors,—they leave us without reply." Flower is bold enough to face this problem, and he does so in a fair and vigorous way, though limiting himself to the supposition of slow and gradual change. He gives up at once, as every anatomist must, the idea of an origin from fishes or reptiles. He thinks the ancestors of the whales must have been quadrupedal mammals. He is obliged for good reasons to reject the seals and the otters, and turns to the ungulates, though here also the difficulties are formidable. Finally, he has recourse to an imaginary ancestor, supposed to have haunted marshes and rivers of the Mesozoic age, and to have been intermediate between a hippopotamus and a dolphin, and omnivorous in diet. As this animal is altogether unknown to geology or zoology, and not much less difficult to account for than the whales themselves, he very properly adds: "Please to recollect, however, that this is a mere speculation." He trusts, however, that such speculations are "not without their use;" but this will depend upon whether or not they lead men's minds from the path of legitimate science into the quicksands of baseless conjecture.

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[4] Report in "Nature," June 21, corrected by the author.

[5] Important facts bearing on this point have also been recently discovered by Hyatt.

[6] Reported in "Nature."

Gaudry, in his recent work, "Enchainements du Monde Animal,"[7] though a strong advocate of evolution, is obliged in his final resumé to say: "Il ne laisse point percer le mystère qui entoure le developpement primitif des grandes classes du monde animal. Nul homme ne sait comment ont été formés les premiers individus de foraminifères, de polypes, d'étoiles de mer, de crinoides, etc. Les fossiles primaires ne nous ont pas encore fourni de preuves positives du passage des animaux d'une classe à ceux d'une autre classe."

Prof. Williamson, of Manchester, in an address delivered in February last before the Royal Institution of Great Britain, after showing that the conifers, ferns and lycopods of the Palæozoic have no known ancestry, uses the significant words: "The time has not yet arrived for the appointment of a botanical king-at-arms and constructor of pedigrees."

Another caution which a palæontologist has occasion to give with regard to theories of life, has reference to the tendency of

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[7] Paris, 1883.

biologists to infer that animals and plants were introduced under embryonic forms, and at first in few and imperfect species. Facts do not substantiate this. The first appearance of leading types of life is rarely embryonic. On the contrary, they often appear in highly perfect and specialized forms, often however of composite type and expressing characters afterwards so separated as to belong to higher groups. The trilobites of the Cambrian are some of them of few segments and so far embryonic, but the greater part are many-segmented and very complex. The batrachians of the Carboniferous present many characters higher than those of their modern successors and now appropriated to the true reptiles. The reptiles of the Permian and Trias usurped some of the prerogatives of the mammals. The ferns, lycopods and equisetums of the Devonian and Carboniferous were, to say the least, not inferior to their modern representatives. The shell-bearing cephalopods of the Palæozoic would seem to have possessed structures now special to a higher group, that of the cuttle-fishes. The bald and contemptuous negation of these facts by Hæckel and other biologists does not tend to give geologists much confidence in their dicta.

Again, we are now prepared to say that the struggle for existence, however plausible as a theory, when put before us in connection with the productiveness of animals and the few survivors of their multitudinous progeny, has not been the determining cause of the introduction of new species. The periods of rapid introduction of new forms of marine life were not periods of struggle but of expansion—those periods in which the submergence of continents afforded new and large space for their extension and comfortable subsistence. In like manner, it was continental emergence that afforded the opportunity for the introduction of land animals and plants. Further, in connection with this, it is now an established conclusion that the great aggressive faunas and floras of the continents have originated in the north, some of them within the arctic circle, and this in periods of exceptional warmth, when the perpetual summer sunshine of the arctic regions coëxisted with a warm temperature. The testimony of the rocks thus is that not struggle but expansion furnished the requisite conditions for new forms of life, and that the periods of struggle were characterized by depauperation and extinction.

But we are sometimes told that organisms are merely mechanical, and that the discussions respecting their origin have no significance any more than if they related to rocks or crystals, because they relate merely to the organism considered as a machine, and not to that which may be supposed to be more important, namely, the great determining power of mind and will. That this is a mere evasion by which we really gain nothing, will appear from a characteristic extract of an article by an eminent biologist in the new edition of the Encyclopedia Britannica, a publication which, I am sorry to say, instead of its proper role as a repertory of facts, has become a strong partisan, stating extreme and unproved speculations as if they were conclusions of science. The statement referred to is as follows: "A mass of living protoplasm is simply a molecular machine of great complexity, the total results of the working of which or its vital phenomena depend on the one hand on its construction and on the other on the energy supplied to it; and to speak of vitality as anything but the name for a series of operations is as if one should talk of the horologity of a clock." It would I think scarcely be possible to put into the same number of words a greater amount of unscientific assumption and unproved statement than in this sentence. Is "living protoplasm" different in any way from dead protoplasm, and if so, what causes the difference? What is a "machine?" Can we conceive of a self-produced or uncaused machine, or one not intended to work out some definite results? The results of the machine in question are said to be "vital phenomena;" certainly most wonderful results, and greater than those of any machine man has yet been able to construct. But why "vital?" If there is no such thing as life, surely they are merely physical results. Can mechanical causes produce other than physical effects? To Aristotle, life was "the cause of form in organisms." Is not this quite as likely to be true as the converse proposition? If the vital phenomena depend on the "construction" of the machine, and the "energy supplied to it," whence this construction and whence this energy? The illustration of the clock does not help us to answer this question. The construction of the clock depends on its maker, and its energy is derived from the hand that winds it up. If we can think of a clock which no one has made and which no one winds, a clock constructed by chance, set in harmony with the universe by chance, wound up periodically by chance, we shall then have an idea parallel to that of an organism living yet without any vital energy or creative law, but in such a case we should certainly have to assume some antecedent cause, whether we call it "horologity" or by some other name. Perhaps the term evolution would serve as well as any other, were it not that common sense teaches that nothing can be spontaneously evolved out of that in which it did not previously exist.

There is one other unsolved problem in the study of life by the geologist to which it is still necessary to advert. This is the inability of palæontology to fill up the gaps in the chain of being. In this respect we are constantly taunted with the imperfection of the record; but facts show that the record is much more complete than is generally supposed. Over long periods of time and many lines of being we have a nearly continuous chain, and if this does not show the tendency desired, the fault is as likely to be in the theory as in the record. On the other hand, the abrupt and simultaneous appearance of new types in many specific and generic forms and over wide and separate areas at one and the same time, is too often repeated to be accidental. Hence palæontologists in endeavoring to establish evolution, have been obliged to assume periods of exceptional activity in the introduction of species alternating with others of stagnation, a doctrine differing very little from that of special creation as held by the older geologists.

The attempt has lately been made to account for these breaks by the assumption that the geological record relates only to periods of submergence and gives no information as to those of elevation. This is manifestly untrue. In so far as marine life is concerned, the periods of submergence are those in which new forms abound for very obvious reasons already hinted; but the periods of new forms of land and fresh-water life are those of elevation, and these have their own records and monuments, often very rich and ample, as for example the swamps of the Carboniferous, the transition from the Cretaceous subsidence to the Laramie elevation, the Tertiary lake-basins of the west, the Terraces and raised beaches of the Pleistocene. Had I time to refer in detail to the breaks in the continuity of life which cannot be explained by the imperfection of the record, I could show at least that nature in this case does advance *per saltum*—by leaps, rather than by a slow continuous process. Many able reasoners, as Le-Conte in this country, and Mivart and Collard in England, hold this view.

Here, as elsewhere, a vast amount of steady conscientious work is required to enable us to solve the problems of the history of life. But if so, the more the hope for the patient student and investigator. I know nothing more chilling to research or unfavorable to progress than the promulgation of a dogmatic decision that there is nothing to be learned but a merely fortuitous and uncaused succession, amenable to no law, and only to be covered, in order to hide its shapeless and uncertain proportions, by the mantle of bold and gratuitous hypothesis.

So soon as we find evidence of continents and oceans we raise the question, Have these continents existed from the first in their present position and form, or have the land and water changed places in the course of geological time? In reality both statements are true in a certain limited sense. On the one hand, any geological map whatever suffices to show that the general outline of the existing land began to be formed in the first and oldest crumplings of the crust. On the other hand, the greater part of the surface of the land consists of marine sediments which must have been derived from land that has perished in the process, while all the continental surfaces, except perhaps, some high peaks and ridges, have been many times submerged. Both of these apparently contradictory statements are true; and without assuming both it is impossible to explain the existing contours and reliefs of the surface.

In the case of North America, the form of the old nucleus of Laurentian rock in the north already marks out that of the finished continent, and the successive later formations have been laid upon the edges of this, like the successive loads of earth dumped over an embankment. But in order to give the great thickness of the Palæozoic sediments, the land must have been again and again submerged and for long periods of time. Thus, in one sense, the continents have been fixed; in another, they have been constantly fluctuating. Hall and Dana have well illustrated these points in so far as eastern North America is concerned. Prof. Hull of the Geological Survey of Ireland has recently had the boldness to reduce the fluctuations of land and water as evidenced in the British Islands to the form of a series of maps intended to show the physical geography of each successive period. The attempt is probably premature, and has been met with much adverse criticism; but there can be no doubt that it has an element of truth. When we attempt to calculate what could have been supplied from the old Eozoic nucleus by decay and aqueous erosion, and when we take into account the greater local thickness of sediments towards the present sea-basins, we can scarcely avoid the conclusion that extensive areas once occupied by high land are now under the sea. But to ascertain the precise areas and position of these perished lands may now be impossible.

In point of fact we are obliged to believe in the contemporaneous existence in all geological periods, except perhaps the very oldest, of three sorts of areas on the surface of the earth: (1) Oceanic areas of deep sea, which must always have occupied the bed of the present ocean or parts of it; (2) Continental plateaus sometimes existing as low flats or as higher table-lands and sometimes submerged; (3) Areas of plication or folding, more especially along the borders of the oceans, forming elevated lands rarely submerged and constantly affording the material of sedimentary accumulations.

Every geologist knows the contention which has been occasioned by the attempts to correlate the earlier Palæozoic deposits of the Atlantic margin of North America with those forming at the same time on the interior plateau, and with those of intervening lines of plication and igneous disturbance. Stratigraphy, lithology and fossils, are all more or less at fault in dealing with these questions, and while the general nature of the problem is understood by many geologists, its solution in particular cases is still a source of apparently endless debate.

The causes and mode of operation of the great movements of the earth's crust which have produced mountains, plains and table-lands, are still involved in some mystery. One patent cause is the unequal settling of the crust toward the centre; but it is not so generally understood as it should be, that the greater settlement of the ocean-bed has necessitated its pressure against the sides of the continents in the same manner that a huge ice-floe crushes a ship or a pier. The geological map of North America shows this at a glance, and impresses us with the fact that large portions of the earth's crust have not only been folded but bodily pushed back for great distances. On looking at the extreme north, we see that the great Laurentian mass of central Newfoundland has acted as a protecting pier to the space immediately west of it, and has caused the gulf of St. Lawrence to remain an undisturbed area since Palæozoic times. Immediately to the south of this, Nova Scotia and New Brunswick are folded back. Still farther south, as Guyot has shown, the old sediments have been crushed in sharp folds against the Adirondack mass, which has sheltered the table-land of the Catskills and of the great lakes. South of this again the rocks of Pennsylvania and Maryland have been driven back in a great curve to the west. Nothing, I think, can more forcibly show the enormous pressure to which the edges of the continents have been exposed, and at the same time the great sinking of the ocean-beds. Complex and difficult to calculate though these movements of plication are, they are more intelligible than the apparently regular pulsations of the flat continental areas, whereby they have alternately been below and above the waters, and which must have depended on somewhat regularly recurring causes, connected either with the secular cooling of the earth or with the gradual retardation of its rotation or with both. Throughout these changes, each successive elevation exposed the rocks for long ages to the decomposing influence of the atmosphere. Each submergence swept away and deposited as sediment the material accumulated by decay. Every change of elevation was accompanied with changes of climate, and with modifications of the habitats of animals and plants. Were it possible to restore accurately the physical geography of the earth in all these respects, for each geological period, the data for the solution of many difficult questions would be furnished.

It is an unfortunate circumstance that conclusions in geology arrived at by the most careful observation and induction do not remain undisturbed, but require constant vigilance to prevent them from being overthrown. Sometimes, of course, this arises from new discoveries throwing new light on old facts; but when this occurs it rarely works the complete subversion of previously received views. The more usual case is that some over zealous specialist suddenly discovers what seems to him to overturn all previous beliefs, and rushes into print with a new and plausible theory which at once carries with him a host of half informed people, but the insufficiency of which is speedily made manifest.

Had I written this address a few years ago, I might have referred to the mode of formation of coal as one of the things most surely settled and understood. The labors of many eminent geologists, microscopists and chemists in the old and the new worlds had shown that coal nearly always rests upon old soil surfaces penetrated with roots, and that coal-beds have in their roofs erect trees, the remains of the last forests that grew upon them. Logan and the writer have illustrated this in the case of the series of more than sixty successive coal beds exposed at the South Joggins, and have shown unequivocal evidence of land surfaces at the time of the deposition of the coal. Microscopical examination has proved that these coals are composed of the materials of the same trees whose roots are found in the underclays, and their stems and leaves in the roof shales; that much of the material of the coal has been subjected to subaërial decay at the time of its accumulation; and that in this, ordinary coal differs from bituminous shale, earthy bitumen and some kinds of cannel, which have been formed under water; that the matter remaining as coal consists almost entirely of epidermal tissues, which being suberose in character are highly carbonaceous, very durable and impermeable by water,[8] and are, hence, the best fitted for the production of pure coal; and finally that the vegetation and the climatal and geographical features of the coal period were eminently fitted to produce in the vast swamps of that period, precisely the effects observed. All these points and many others have been thoroughly worked out for both European and American coal-fields, and seemed to leave no doubt on the subject. But several years ago certain microscopists observed on slices of coal layers filled with spore-cases, a not unusual circumstance, since these were shed in vast abundance by the trees of the coal forests, and because they contain suberose matter of the same character with epidermal tissues generally. Immediately we were informed that all coal consists of spores, and this being at once accepted by the unthinking, the results of the labors of many years are thrown aside in favor of this crude and partial theory. A little later, a German microscopist has thought proper to describe coal as made up of minute algæ, and tries to reconcile this view with the appearances, devising at the same time a new and formidable nomenclature of generic and specific names, which would seem largely to represent mere fragments of tissues. Still later, some local facts in a French coal-field have induced an eminent botanist of that country to revive the drift theory of coal. in opposition to that of growth *in situ*. A year or two ago, when my friend Professor Williamson of Manchester, informed me that

[8] Acadian Geology, third edition, supplement, p. 68.

he was preparing a large series of slices of coal with the view of revising the whole subject, I was inclined to say that after what had been done by Lyell, Goeppert, Logan, Hunt, Newberry and myself, this was scarcely necessary; but in view of what I have just stated, it may be that all he can do will be required to rescue from total ruin the results of our labors even should he make no new discoveries.

An illustration of a different character is afforded by the controversy now raging with respect to the so-called fucoids of the ancient rocks. At one time the group of fucoids or algæ constituted a general place of refuge for all sorts of unintelligible forms and markings; graptolites, worm-trails, crustacean tracks, shrinkage-cracks, and above all rill-markings forming a heterogeneous group of fucoidal remains distinguished by generic and specific names. To these were also added some true land plants, badly preserved or exhibiting structures not well understood by botanists. Such a group was sure to be eventually dismembered. The writer has himself done something toward this,[9] but Professor Nathorst has done still more,[10] and now some intelligible explanation can be given of many of these forms. Quite recently, however, the Count de Saporta, in an elaborate illustrated memoir[11] has come to the defence of the fucoids, more especially against the destructive experiments of Nathorst, and would carry back into the vegetable kingdom many things which would seem to be mere trails of animals. While writing this address, I have received from Professor Crié of Rennes, a paper in which he not only supports the algal nature of *Rusichnites*, *Arthrichnites* and many other supposed fucoids, but claims for the vegetable kingdom even *Receptaculites* and *Archæocyathus*. It is not to be denied that some of the facts which he cites respecting the structure of the *Siphoniæ* and of certain modern encrusting *Algæ* are very suggestive, though I cannot agree with his conclusions. My own experience has convinced me that, while non-botanical geologists are prone to mistake all kinds of markings for plants, even good botanists, when not familiar with the chemical and mechanical conditions of fossilization, and with the present phenomena of tidal shores, are quite as easily misled, though they are very prone on the other hand to regard land plants of some complexity, when badly preserved, as mere algæ. In these circumstances it is very difficult to secure any consensus, and the truth is only to be found by careful observation of competent men. One trouble is that these usually obscure markings have been despised by the greater number of palæontologists, and probably would not now be so much in controversy were it not for the use made of them in illustrating supposed phylogenies of plants.

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[9] Footprints and Impressions on Carboniferous Rocks, Am. Jour. of Science, 1873.

[10] Royal Swedish Academy, Stockholm, 1881.

[11] Apropos des Algues fossiles, Paris, 1883.

It would be wrong to close this address without some reference to that which is the veritable *pons asinorum* of the science, the great and much debated glacial period. I trust that you will not suppose that in the end of an hour's address, I am about to discuss this vexed question. Time would fail me even to name the hosts of recent authors who have contended in this arena. I can hope only to point out a few landmarks which may aid the geological adventurer in traversing the slippery and treacherous surface of the hypothetical ice-sheets of pleistocene times, and in avoiding the yawning crevasses by which they are traversed.

No conclusions of geology seem more certain than that great changes of climate have occurred in the course of geological time, and the evidence of this in that comparatively modern period which immediately preceded the human age is so striking that it has come to be known as preëminently the ice age; while in the preceding tertiary periods, temperate conditions seem to have prevailed even to the pole. Of the many theories as to these changes which have been proposed, two seem at present to divide the suffrages of geologists, either alone or combined with each other. These are (1)the theory of the precession of the equinoxes in connection with the varying eccentricity of the earth's orbit, advocated more especially by Croll; and (2) the different distribution of land and water as affecting the reception and radiation of heat and the ocean currents, a theory ably propounded by Lyell, and subsequently extensively adopted either alone or with the previous one. One of these views may be called the astronomical, the other the geographical. I confess that I am inclined to accept the second or Lyellian theory for such reasons as the following: (1) Great elevations and depressions of land have occurred in and since the Pleistocene, while the alleged astronomical changes are not certain, more especially in regard to their probable effect on the earth; (2) When the rival theories are tested by the present phenomena of the southern polar region and the North Atlantic, there seem to be geographical causes adequate to account for all except extreme and unproved glacial conditions; (3) The astronomical cause would suppose regularly recurring glacial periods of which there is no evidence, and it would give to the latest glacial age an antiquity which seems at variance with all other facts; (4) In those more northern regions where glacial phenomena are most pronounced, the theory of floating sheets of ice, with local glaciers descending to the sea, seems to meet all the conditions of the case, and these would be obtained, in the North Atlantic at least, by very moderate changes of level, causing, for example, the equatorial current to flow into the Pacific, instead of running northward as a gulf stream; (5) The geographical theory allows the supposition not merely of vicissitudes of climate quickly following each other in unison with the movements of the surface, but allows also of that near local approximation of regions wholly covered with ice and snow and others comparatively temperate which we see at present in the north.

If, however, we are to adopt the geographical theory, we must avoid extreme views, and this leads to the inquiry as to the evidence to be found for any such universal and extreme glaciation as is demanded by some geologists.

The only large continental area in the northern hemisphere supposed to be entirely ice- and snow-clad is Greenland, and this so far as it goes is certainly a local case, for the ice and snow of Greenland extend to the south as far as 60° N. latitude, while both in Norway and in the interior of North America the climate in that latitude permits the growth of cereals. Further, Grinnel Land, which is separated from North Greenland only by a narrow sound, has a comparatively mild climate, and as Nares has shown is covered with verdure in summer. Still further, Nordenskiold, one of the most experienced arctic explorers, holds that it is probable that the interior of Greenland is itself verdant in summer, and is at this moment preparing to attempt to reach this interior oasis. Nor is it difficult with the aid of the facts cited by Wœickoff and Whitney,[12] to perceive the cause of the exceptional condition of Greenland. To give ice and snow in large quantities, two conditions are required: first, atmospheric humidity, and, secondly, cold precipitating regions. Both of these conditions

[12] Memoir on Glaciers, Geol. Soc'y, Berlin, 1881. Climatic Changes, Boston, 1883.

meet in Greenland. Its high coast ranges receive and condense the humidity from the sea on both sides of it and to the south. Hence the vast accumulation of its coast snow-fields, and the intense discharge of the glaciers emptying out of its valleys. When extreme glacialists point to Greenland and ask us to believe that in the glacial age the whole continent of North America as far south as the latitude of 40° was covered with a continental glacier, in some places several thousands of feet thick, we may well ask first what evidence there is that Greenland or even the Antarctic continent at present shows such a condition; and, secondly, whether there exists a possibility that the interior of a great continent could ever receive so large an amount of precipitation as that required. So far as present knowledge exists, it is certain that the meteorologist and the physicist must answer both questions in the negative. In short, perpetual snow and glaciers must be local and cannot be continental, because of the vast amount of evaporation and condensation required. These can only be possible where comparatively warm seas supply moisture to cold and elevated land, and this supply cannot in the nature of things penetrate far inland. The actual condition of interior Asia and interior America in the higher northern latitudes affords positive proof of this. In a state of partial submergence of our northern continents, we can readily imagine glaciation by the combined action of local glaciers and great ice-floes, but in whatever way the phenomena of the boulder clay and of the so-called terminal moraines are to be accounted for, the theory of a continuous continental glacier must be given up.

I cannot better indicate the general bearing of facts, as they present themselves to my mind in connection with this subject, than by referring to a paper by Dr. G. M. Dawson on the distribution of drift over the great Canadian plains, east of the Rocky mountains.[13] I am the more inclined to refer to this, because of its recency, and because I have so often repeated similar conclusions as to eastern Canada and the region of the Great Lakes.

The great interior plain of western Canada, between the Laurentian axis on the east and the Rocky mountains on the west, is seven hundred miles in breadth, and is covered with glacial drift presenting one of the greatest examples of this deposit in the world. Proceeding eastward from the base of the Rocky moun-

[13] Science, July 1, 1883.

tains, the surface, at first more than 4,000 feet above the sea level, descends by successive steps to 690 feet, and is based on Cretaceous and Laramie rocks, covered by boulder clay and sand, in some places from one hundred to two hundred feet in depth, and filling up preëxisting hollows, though itself sometimes piled into ridges. Near the Rocky mountains the bottom of the drift consists of gravel not glaciated. This extends to about one hundred miles east of the mountains, and must have been swept by water out of their valleys. The boulder clay resting on this deposit is largely made up of local debris, in so far as its paste is concerned. It contains many glaciated boulders and stones from the Laurentian region to the east, and also smaller pebbles from the Rocky mountains, so that at the time of its formation there must have been driftage of large stones for seven hundred miles or more from the east, and of smaller stones from a less distance on the west. The former kind of material extends to the base of the mountains, and to a height of more than 4,000 feet. One boulder is mentioned as being 42 × 40 × 20 feet in dimensions. The highest Laurentian boulders seen were at an elevation of 4,660 feet on the base of the Rocky mountains. The boulder clay when thick can be seen to be rudely stratified, and at one place includes beds of laminated clay with compressed peat, similar to the forest beds described by Worthen and Andrews in Illinois, and the so-called interglacial beds described by Hinde on Lake Ontario. The leaf beds on the Ottawa river and the drift trunks found in the boulder clay of Manitoba belong to the same category, and indicate that throughout the glacial period there were many forest oases far to the north. In the valleys of the Rocky mountains opening on these plains there are evidences of large local glaciers now extinct, and similar evidences exist on the Laurentian highlands on the east.

Perhaps the most remarkable feature of the region is that immense series of ridges of drift piled against an escarpment of Laramie and Cretaceous rocks, at an elevation of about 2,500 feet, and known as the "Missouri Coteau." It is in some places 30 miles broad and 180 feet in height above the plain at its foot, and extends north and south for a great distance; being in fact the northern extension of those great ridges of drift which have been traced south of the great lakes, and through Pennsylvania and New Jersey, and which figure on the geological maps as the edge of a continental glacier,—an explanation obviously inapplicable in those western regions where they attain their greatest development. It is plain that in the north it marks the western limit of the deep water of a glacial sea, which at some periods extended much farther west, perhaps with a greater proportionate depression in going westward, and on which heavy ice from the Laurentian districts on the east was wafted southwestward by the arctic currents, while lighter ice from the Rocky Mountains was being borne eastward from these mountains by the prevailing westerly winds. We thus have in the west on a very wide scale the same phenomena of varying submergence, cold currents, great ice floes and local glaciers producing icebergs, to which I have attributed the boulder clay and upper boulder drift of eastern Canada.

A few subsidiary points I may be pardoned for mentioning here. The rival theories of the glacial period are often characterized as those of land glaciation and sea-borne icebergs. But it must be remembered that those who reject the idea of a continental glacier hold to the existence of local glaciers on the high lands, more or less extensive during different portions of the great Pleistocene submergence. They also believe in the extension of these glaciers seawards, and partly water-borne in the manner so well explained by Mattieu Williams, in the existence of those vast floes and fields of current and tide-borne ice whose powers of transport and erosion we now know to be so great, and in a great submergence and reëlevation of the land bringing all parts of it and all elevations up to 5,000 feet successively under the influence of these various agencies, along with those of the ocean currents. They also hold that at the beginning of the glacial submergence, the land was deeply covered by decomposed rock, similar to that which still exists on the hills of the southern states, and which as Dr. Hunt has shown would afford not only earthy debris but large quantities of boulders ready for transportation by ice.

I would also remark that there has been the greatest possible exaggeration as to the erosive action of land ice. In 1865, after a visit to the Alpine glaciers, I maintained that in these mountains glaciers are relatively protective rather than erosive agencies, and that the detritus which the glacier streams deliver is derived mostly from the atmospherically wasted peaks and cliffs that project above them. Since that time many other observers have maintained like views, and very recently Mr. Davis of Cambridge and Mr. A. Irving have ably treated this subject.[14] Smoothing and striation of rocks are undoubtedly important effects both of land-glaciers and heavy sea-borne ice, but the levelling and filling agency of these is much greater than the erosive. As a matter of fact, as Newberry, Hunt, Belt, Spencer and others have shown, the glacial age has dammed up vast numbers of old channels which it has been left for modern streams partially to excavate.

The till or boulder clay has been called a "ground moraine," but there are really no Alpine moraines at all corresponding to it. On the other hand, it is more or less stratified, often rests on soft materials which glaciers would have swept away, sometimes contains marine shells, or passes into marine clays in its horizontal extension, and invariably in its embedded boulders and its paste, shows an unoxidized condition, which could not have existed if it had been a subaërial deposit. When the Canadian till is excavated and exposed to the air, it assumes a brown color, owing to oxidation of its iron, and many of its stones and boulders break up and disintegrate under the action of air and frost. These are unequivocal signs of a subaqueous deposit. Here and there we find associated with it, and especially near the bottom and at the top, indications of powerful water action, as if of land torrents acting at particular elevations of the land, or heavy surf and ice action on coasts, and the attempts to explain these by glacial streams have been far from successful. A singular objection sometimes raised against the subaqueous origin of the till is its general want of marine remains; but this is by no means universal, and it is well known that coarse conglomerates of all ages are generally destitute of fossils, except in their pebbles, and it is further to be observed that the conditions of an ice-laden sea are not those most favorable for the extension of marine life, and that the period of time covered by the glacial age must have been short compared with that represented by some of the older formations.

This last consideration suggests a question which might afford scope for another address of an hour's duration,—the question how long time has elapsed since the close of the glacial period. Recently the opinion has been gaining ground that the close of the ice-age is very recent. Such reasons as the following lead to this conclusion. The amount of atmospheric decay of rocks and of denudation in general which have occurred since the close of the glacial period are scarcely appreciable. Little erosion of river valleys or of coast terraces has occurred. The calculated recession of waterfalls and of production of lake ridges lead to the same conclusion. So do the recent state of bones and shells in the Pleistocene deposits and the perfectly modern facies of their fossils. On such evidence the cessation of the glacial cold and settlement of our continents at their present levels are events which may have occurred not more than 6000 or 7000 years ago, though such time estimates are proverbially uncertain in geology. This subject also carries with it the greatest of all geological problems, next to that of the origin of life, namely, the origin and early history of man. Such questions cannot be discussed in the closing sentences of an hour's address. I shall only draw from them one practical inference. Since the comparatively short Post-glacial and recent periods apparently include the whole of human history, we are but new comers on the earth and therefore have had little opportunity to solve the great problems which it presents to us. But this is not all. Geology as a science scarcely dates from a century ago. We have reason for surprise in these circumstances, that it has learned so much, but for equal surprise that so many persons appear to think it a complete and full-grown science and that it is entitled to speak with confidence on all the great mysteries of the earth that have been hidden from the generations before us. Such being the newness of man and of his science of the earth, it is not too much to say that humility, hard work in collecting facts, and abstinence from hasty generalization should characterize geologists, at least for a few generations to come.

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[14] Proceedings Boston Soc. Nat. Hist., XXII. Journal of Geological Society, February, 1883.

In conclusion, science is light, and light is good; but it must be carried high, else it will fail to enlighten the world. Let us strive to raise it high enough to shine over every obstruction which casts any shadow on the true interests of humanity. Above all, let us hold up the light and not stand in it ourselves.

PAPERS READ

IN

GENERAL SESSIONS.

A CLASSICATION OF THE NATURAL SCIENCES. By T. STERRY HUNT of Montreal, Can.

[ABSTRACT.]

To frame a rational classification of the natural sciences, and to define their mutual relations, has often been attempted. The present writer, in an essay read before the National Academy of Sciences in April, 1881, and published in the *L. E. & D. Philosophical Magazine*, for October, 1881, with the title of "The Domain of Physiology," suggested the basis of such a scheme, and he now, at the request of some of his readers, ventures, for the first time, to embody in a concise and tabulated form the views then and there enunciated, in the hope that other students may find it not unworthy of their notice.

The study of material nature constitutes what the older scholars correctly and comprehensively termed physics (the words physical and natural being synonymous) and presents itself in a twofold aspect: — first, as descriptive; and, second, as philosophical, — a distinction embodied in the terms Natural History and Natural Philosophy, or, more concisely, in the words Physiography and Physiology. The latter word has, from the time of Aristotle, been employed, in this general sense, to designate the philosophical study of nature, and will so be used in the present classification.

The world of nature is divided into the inorganic or mineralogical, and the organic or biological, kingdoms; the division of the latter into vegetable and animal being a subordinate one. The natural history, or physiography, of the inorganic kingdom, takes cognizance of the sensible characters of chemical species, and gives us descriptive and systematic mineralogy, which have hitherto been restricted to native species, but, in their wider sense, include all artificial species as well. The study of native mineral species, their aggregations, and their arrangement as constituents of our planet, is the object of geognosy and physical geography. The physiography of other worlds gives rise to descriptive astronomy.

The natural philosophy of the inorganic kingdom, or mineral physiology, is concerned, in the first place, with what is generally called dynamics or physics; including the phenomena of ordinary motion, sound, temperature, radiant energy, electricity, and magnetism. Dynamics, in the abstract, regards matter in general, without relation to species; chemism generates therefrom mineralogical or so-called chemical species, which, theoretically, may be supposed to be formed from a single elemental substance or *materia prima*, by the chemical process. Dynamics and chemistry build up our inorganic world, giving rise to geogeny, and, as applied to other worlds, to theoretical astronomy.

Proceeding now to the organic kingdom, its physiographical study leads us first to organography, and then to descriptive and systematic botany and zoölogy, two great subdivisions of natural history. Coming, next, to consider the physiological aspects of organic nature, we find, besides the dynamical and chemical activities manifested in the mineral, other and higher ones which characterize the organic kingdom. On this higher plane of existence, are found portions of matter which have become individualized, exhibit irritability, the power of growth by assimilation, and of reproduction, and moreover establish relations with the external world by the development of organs; all of which characters are foreign to the mineral kingdom. These new activities are often designated as vital; but since this word is generally made to include at the same time other manifestations, which are simply dynamical or chemical, I have elsewhere proposed for the activities characteristic of the organism the term biotics (*βιωτικός*, pertaining to life). The physiology of matter in the abstract is dynamical, that of mineral species is both dynamical and chemical, while that of organized forms is at once dynamical, chemical, and biotical. The study of the biotical activities of matter leads to organogeny and morphology, while the relations of organisms to one another, and to the inorganic kingdom, give us physiological botany and zoölogy. We thus arrive at a comprehensive and simple scheme of the natural sciences, which I have endeavored to set forth in the subjoined table.

Natural Sciences.	Inorganic Nature.	Organic Nature.
Descriptive. — General Physiography. or Natural History.	Mineral Physiography. — Descriptive and Systematic Mineralogy; Geognosy; Geography; Descriptive Astronomy.	Biophysiography. — Organography; Descriptive and Systematic Botany and Zoology.
Philosophical. — General Physiology or Natural Philosophy.	Mineral Physiology. — *Dynamics* or *Physics;* *Chemistry.* Geogeny; Theoretical Astronomy.	Biophysiology. — *Biotics.* Organogeny; Morphology; Physiological Botany and Zoology.

THE EVIDENCE FOR EVOLUTION IN THE HISTORY OF THE EXTINCT MAMMALIA. By E. D. COPE, of Philadelphia, Pa.

THE subject to which I wish to call your attention this morning requires neither preface nor apology, as it is one with the discussion of which you are perfectly familiar. My object in bringing it before the general session of the association is in view of the fact that you are all familiar with it in a general way, and that it probably interests the members of sections who do not pursue the special branch to which it refers, as well as those who do: also since it has been brought before us in various public addresses for many years, during the meetings of this association, I thought it might be well to be introduced at this meeting, in order that we might not omit to have all the sides of this interesting question presented.

The interests which are involved in it are large: they are chiefly, however, of a mental and metaphysical character; they do not refer so much to industrial and practical interests, nor do they involve questions of applied science. They involve, however, questions of opinion, questions of belief, questions which affect human happiness, I venture to say, even more than questions of applied science; certainly, which affect the happiness of the higher grades of men and women more than food or clothing, because they relate to the states of our mind, explaining as they do the reasons of our relations to our fellow-beings, and to all other things by which we are surrounded, and the general system of the forces by which we are controlled. So it has always appeared to me: hence I have selected the department of biology, and have taken a great interest in this aspect of it.

The doctrine of evolution, as taught by the biologists of to-day, has several stages or parts of its presentation. First, the foundation principle is this: that the species of animals and of plants, the species of organic beings, as well as the various natural divisions into which these organic beings fall, have not always been as we see them to-day, but they have been produced by a process of change which has progressed from age to age through the influence of natural laws; that, therefore, the species which now exist are the descendants of other species which have existed heretofore, by the ordinary processes of reproduction; and that all the various structures of organic beings, which make them what they are, and which compel them to act as they now act, are the result of gradual or sudden modifications and changes during the periods of geologic time. That is the first phase or aspect which meets the naturalist or biologist.

Another phase of the question relates to the origin of that life itself which is supposed to inhabit or possess organic beings. There is an hypothesis of evolution which derives this life from no-life, which derives vitality from non-vitality. That is another branch of the subject, to which I cannot devote much attention to-day.

There is still another department of the subject, which relates to the origin of mind, and which derives the mental organization of the higher animals, especially of man, from preëxistent types of mental organization. This gives us a genealogy of mind, a history of the production or creation of mind, as it is now presented in its more complex aspects as a function of the human brain. This aspect of the subject is, of course, interesting; and upon that I can touch with more confidence than upon the question of the origin of life.

Coming now to the question of the origin of structures, we have by this time accumulated a vast number of facts which have been collated by laborious and faithful workers, in many countries and during many years; so that we can speak with a good deal of confidence on this subject also. As to the phenomena which meet the student of zoölogy and botany at every turn, I would merely repeat, what every one knows,—and I beg pardon of my biological friends for telling them a few well-known truths, for there may be those present who are not in the biological section,— the phenomena which meet the student of biology come under two leading classes. The first is the remarkable fidelity of species in reproducing their like. "Like produces like," is the old theorem, and is true in a great many cases; just as coins are struck from the die, just as castings are turned out from a common mould. It is one of the most wonderful phenomena of nature, that such complex organisms, consisting of so many parts, should be repeated from age to age, and from generation to generation, with such surprising fidelity and precision. This fact is the first that strikes the student of these sciences. The first impression of the ordinary person would be, that these things must continue unchanged. When I began to study zoölogy and botany, I was surprised to find there was a science of which I had no conception, and that was this remarkable reproduction of types one after another in continued succession. After a man has had this idea thoroughly assimilated by his honest and conscientious studies, he will be again struck with another class of facts. He will find, not unfrequently, that this doctrine does not apply. He will find a series of facts which show that many individuals fail to coincide with their fellows precisely, the most remarkable variations and the most remarkable half-way attitudes and double-sided aspects occurring; and he will come to the conclusion, sooner or later, that like does not produce like in some animals with the same precision and fidelity with which it is accomplished in other animals. So that we have these two classes of facts,—the one relating to, and expressing, the law of heredity; the other, which expresses the law of variation. I should not like to say which class of facts is the most numerously presented to the student. In the present fauna we find many groups of species and varieties; but exactly how many species we have, how many genera and families we have, we cannot definitely state. The more precise and exact a person is in his definition and in his analysis, the more definite his science becomes, and the more precise and scientific his work. Biology is a science of analysis of forms. What the scales are to the chemist and the physicist, the rule and measure are to the biologist. It is a question of dimension, a question of length and breadth and thickness, a question of curves, a question of crooked shapes or simple shapes,—rarely simple shapes, mostly crooked, generally bilateral. It requires that one should have a mechanical eye, and should have also something of an artistic eye, to appreciate these forms, to measure them, and to be able to compare and weigh them.

Now, when we come to arrange our shapes and our measurements, we find, as I said before, a certain number of identities, and a certain number of variations. This question of variation is so common and so remarkable, that it becomes perfectly evident to the specialist in each department, that like does not at all times produce like. It is perfectly clear, and I will venture the assertion that nearly all the biologists in this room will bear me witness, that variability is practically unlimited in its range, and multiplied in the number of its examples. That is to say; species vary by adding or by failing to retain certain characteristics; and generic and other characters are found to appear or disappear in accordance with some law to be discussed farther on.

I believe that this is the simplest mode of stating and explaining the law of variation: that some forms acquire something which their parents did not possess; and that those which acquire something additional have to pass through more numerous stages than their ancestors; and those which lose something pass through fewer stages than their ancestors; and these processes are expressed by the terms "acceleration" and "retardation."

Of course we are met with the opposite side of the case,—the law of heredity. We are told that the facts there are not accounted for by any law of evolution; that we cannot pass from one class of facts to the other class of facts; that the law of the one class is not that of the other. Here is a question of rational processes, of ordinary reason. If the rules of chemistry are true in America, I imagine they are true in Australia and Africa, although I have not been there to see. If the law of gravitation is effective here, I do not need to go to Australia or New Zealand to ascertain whether it be true there. So, if we find in a group of animals a law sufficient to account for their creation, it is not necessary to know that others of their relatives have gone through a similar process. I am willing to allow the ordinary practical law of induction, the practical law of inference, to carry me over these gaps, over these interruptions. And I state the case in this way, because it is just here that some people differ from me, and it is just here that I say the simple question of rationality comes in. I cannot believe that nature's laws are so dissimilar, so irregular, so inexact, that those which we can see and understand in one place are not true in another; I also believe that the question of geological likelihood is similar to the question of geographical likelihood. If a given process be true in one of the geological periods, it is true in another; if it be true in one part of the world, it is true in another; because I find interruptions in the series here, it does not follow that their need be interruptions clear through from age to age. The assumption is on the side of the man who asserts that transitions have not taken place between forms which are now distinct.

We are told that we find no sort of evidence of that transition in past geological periods; we are assured that such changes have not taken place; we are even assured that no such sign of such transition from one species to another has ever been observed,—a most astonishing assertion to make *to* a biologist, or *by* a biologist; and such persons have even the temerity to cite special cases, as between the wolf and the dog. Many of our domestic dogs are nothing but wolves, which have been modified by the hand of man to a very slight extent indeed. Many dogs, in fact nearly all dogs, are descendants of wild species of various countries, and are variously modified.

To take the question of the definition of species. Supposing we have several species well defined, say four or five. In the process of investigation we obtain a larger number of individuals, many of which betray characters which invalidate the definitions. It becomes necessary to unite the four or five species into one. And so, then, because our system requires that we shall have accurate definitions (the whole basis of the system is definitions; you know the very comprehension of the subject requires definitions), we throw them all together, because we cannot define all the various special forms as we did before, until we have but one species. And the critic of the view of evolution tells us, "I told you so! There is but one species, after all. There is no such thing as a connection between species: you never will find it." Now, how many discoveries of this kind will be necessary to convince the world that there are connections between species? How long are we to go on finding connecting links, and putting them together, as we have to do for the sake of the definition, and then be told that we have, nevertheless, no intermediate forms between species? The matter is too plain for further comment. We throw them together, simply because our definitions require it. If we knew all the known individuals which have lived, we should have no species, we should have no genera. That is all there is of it. It is simply a question of a universal accretion of material, and the collection of information. I do not believe that the well-defined groups will be found to run together, as we call it, in any one geological period, certainly in no one recent period. We recognize, however, in looking backwards that they converge to a wonderful extent: one group has diverged at one period, and another one has become diversified in a different period; and so each one has its history, some beginning farther back than others, some reaching far back beyond the very beginning of the time when fossils could be preserved. I call attention to this view because it is a very easy matter for us to use words for the purpose of confusing the mind; for, next to the power of language to express clear ideas, is its power of expressing no ideas at all. As we all know, we can *say* many things which we cannot *think*. It is a very easy thing to say twice two are equal to six, but it is impossible to think it.

I would cite what I mean by variations of species in one of its phases: I mention a genus of snakes, Ophibolus, which is found in the United States. If we take the species of this snake-genus as found in the northern states, we have a good many species well defined. If we go to the gulf states, and examine our material, we see we have certain other species well defined, and they are very nicely distinguished. If, now, we go to the Pacific coast, to Arizona and New Mexico, we shall find another set of species well defined indeed. If we take all these different types of our specimens of different localities together, our species, as the Germans say, all tumble together: definitions disappear, and we have to recognize, out of the preliminary list of thirteen or fourteen, only four or five. That is simply a case of the kind of fact with which every biologist is perfectly familiar.

When we come to the history of the extinct forms of life, it is perfectly true that we cannot observe the process of descent in actual operation, because, forsooth, fossils are necessarily dead. We cannot perceive any activities, because fossils have ceased to act. But, if this doctrine be true, we should get the series, if there be such a thing; and we do, as a matter of fact, find longer or shorter series of structures, series of organisms proceeding from one form into another form, which are exactly as they ought to be if this process of development by descent had taken place.

I am careful to say this; because it is literally true, as we all must admit, that species must fall into some kind of order or other. You could not collect bottles, you could not collect old shoes, 'but you could make some kind of a serial order of them. There are, no doubt, characters by which such and such shoes could be distinguished from other shoes, these bottles from other bottles. But if serial order does not prove evolution, as is too often assumed, we have in recent forms of life in zoölogy and botany, irrefragable proofs of the metamorphoses, and transformations, and changes of the species, in accordance with the doctrine with which we commenced.

We now come to the second chapter of our subject. With the assumption, as I take it already satisfactorily proven, of species having changed into others, in considering this matter of geological succession or biological succession, I bring you face to face with the nature and mode of the change; and hence we may get a glance perhaps at its laws.

I have on the board a sketch or table which represents the changes which took place in certain of the mammalia. I give you a summary of the kind of thing which we find in one of the branches of paleontology. I have here two figures, one representing a restoration, and the other an actual picture, of two extinct species that belong to the early eocene periods. One represents the ancestor of the horse line, Hyracotherium, which has four toes on his anterior feet, and three behind; and the other, a type of animal, Phenacodus, which is antecedent to all the horse series, the elephant series, the hog, the rhinoceros, and all the other series of hoofed animals. It has five toes on all the feet. Each presents us with the primitive position in which their series first come to our knowledge in the history of geological time.

I have also arranged here a series of some leading forms of the three principal epochs of the mesozoic times, and six of the leading ones of the tertiary time. I have added some dates to show you the time when the faunæ which are entombed in those beds were discovered, in the course of our studies; and you will easily see how unsafe it is to say that any given type of life has never existed, or even to assert that such and such a form is unknown; and it is still more unsafe, I think, to assert that any given form of life properly defined, or that a specific intermediate form of life, will not be found. I think it is much safer to assert that such and such intermediate forms will be found. I have frequently had the pleasure of realizing anticipations of this kind. I have asserted that certain types would be found, and they have been found. You will see that I attend to the matter of time because there have been a great many things discovered in the last ten or fifteen years in this department. With these forms I give the date of the discovery of the fauna in which they are embraced.

Here we have the White-River fauna discovered in 1856; then we skip a considerable period of time, and the next one was in 1869, when the cretaceous series was found. Six or seven cretaceous faunæ have been discovered. Then we have the Bridger fauna in 1870, the Wasatch fauna in 1874. Next we have, in 1877, the Equus beds and the fauna which they embrace, part of which was also found in 1878. The Permian fauna, which is one of the last, in 1879; and the latest, the Puerco, which gives the oldest and ancestral types of the modern forms of mammalia, was only found in 1881. When I first commenced the study of this subject, about 1860, there were perhaps 250 species known. There are now somewhere near 2,000, and we are augmenting them all the time. I have found many myself: if they were distributed through the days of the year, I think in some years I should have had several every day. You see then that the accessions to knowledge which are constantly being made make it unsafe to indulge in any prophecies; as for instance, that, because such and such things have not been found, therefore they cannot be; for we find such and such things really have been and really are discovered.

The successive changes that we have in the mammalia have taken place in the limbs, feet, teeth, and brain, and the vertebral column. The parts which present us with the greatest numbers of variations are those in which many parts are concerned, as in the limbs and feet. In the lower eocene (Puerco), the toes were 5–5. In the Loup-Fork fauna, some possess toes but 1–1. Prior to this period no such reduction was known, although in the Loup-Fork fauna a very few species remained 5–5. Through this entire series we have transitions steady and constant, from 5–5, to 4-5, to 4–4, to 4–3, to 3–3, to 2–2, to 1–1. In the Puerco period there was not a single mammal of any kind which had a good ankle-joint; which had an ankle-joint constructed as ankle-joints ought to be, with tongue and groove. The model ankle-joint is a tongue-and-groove arrangement. In this period they were nearly all perfectly flat. As time passes on we get them more and more grooved, until in the Loup-Fork fauna and the White-River fauna they are nearly all grooved. In the sole of the foot, in the Puerco fauna, they are all flat; but in the Loup-Fork fauna the sole of the foot is in the air, and the toes only are applied to the ground, with the exception of the line of monkeys, in which the feet have not become erect on the toes, and the elephant, in which the feet are nearly flat also, and the line of bears, where they are also flat. As regards the angulation between the small bones of the palm and of the sole, there is not a single instance in which those bones are locked in the lower eocene, as they are in the later and latest tertiary. So also with the articulation of the toes with the foot.

When we come to the limbs, the species of the Puerco fauna have short legs. They have gradually lengthened out, and in the late periods they are nearly all relatively long.

Formation.	No of toes.	Feet.	Astragalus.	Carpus and tarsus.	Ulno-radius.	Superior molars.	Zygapophyses.	Brain.
Miocene . .	1-1 2-2	Digitigrade. (Plantigrade.)	Grooved. (Flat.)	Interlocking. (Opposite)	Faceted.	4-tubercles, crested and cemented.	Doubly involute. Singly involute.	Hemispheres larger, convoluted.
Upper . . . . . (Loup Fork.)	3-3 4-4 (5-5)							
Middle . . . . . (John Day.)	2-2 3-3 4-4	Digitigrade.	Grooved.	Interlocking.	Faceted. Smooth.	4-tubercles, and crested.	Singly involute. Doubly involute.	Hemispheres larger, convoluted.
Lower . . . . . (White River.)	3-3 4-3 4-4	Digitigrade. Plantigrade.	Grooved.	Interlocking.	Smooth. Faceted.	4-tubercles, and crested.	? Singly involute.	Hemispheres small; and larger.
Eocene . . Upper . . . . . (Bridger.) . .	3-3 4-3 4-5 5-5	(Digitigrade.) Plantigrade.	Grooved. (Flat.)	Opposite. Interlocking.	Smooth.	4-tubercles. 3-tubercles, and crested.	Singly involute. Plane.	Hemispheres small.
Middle . . . . . (Wasatch.)	4-3 4-5 5-5	Plantigrade. (Digitigrade.)	Flat. (Grooved)	Opposite. Interlocking.	Smooth.	4-tubercles. 3-tubercles, a few crested.	Plane. Singly involute.	Hemispheres small; mesencephalon sometimes exposed.
Lower . . . . . (Puerco.)	5-5	Plantigrade.	Flat.	Opposite.	Smooth.	3-tubercles. (4-tubercles), none crested.	Plane.	Mesencephalon; exposed hemisphere small and smoother.

Coming to the vertebræ as a part of the osseous system, I mention the zygapophyses, or antero-posteriorly directed processes, of which the posterior looks down, and the anterior looks up. They move on each other, as the vertebral column bends from side to side. In the lower forms of vertebrates they are always flat, and in the hoofed mammals of the Puerco period they are all flat. In the Wasatch period we get a single group in which the articulation, instead of being perfectly flat, becomes rounded; in the later periods we get them very much rounded; and, finally, in the latest forms, we get the double curve and the locking process in the vertebral column, which, as in the limb, secures the greatest strength with the greatest mobility.

In the first stages of the growth of the spinal column, it is a notochord, or a cylinder of cartilage or softer material. In later stages the bony deposit is made in its sheath until it is perfectly segmented. Now, all the Permian land-animals, reptiles, and batrachians, retain this notochord with the elements of osseous vertebræ, in a greater or less degree of completeness. There are some in South Africa, I believe, in which the ossification has come clear through the notochord; but they are few. In this characteristic the Permian appears almost, perhaps absolutely, peculiar as regards land-animals. There is something to be said as to the condition of the column from a mechanical standpoint, and it is this: that the chorda exists, with its osseous elements disposed about it; and in the batrachians equally related to salamanders and frogs, these osseous elements are arranged in the sheath or skin of the chorda; and they are in the form of regular concave segments, very much like such segments as you can take from the skin of an orange, — but parts of a cylinder, and having greater or less dimensions according to the group or species. Now, the point of divergence of these segments is on the side of the column. The contacts are placed on the side of the column where the segments separate, — the upper segments rising and the lower segments coming downwards. To the upper segments are attached the arches and their articulations, and the lower segments are like the segments of a cylinder. If you take a flexible cylinder, and cover it with a more or less inflexible skin or sheath, and bend that cylinder sidewise, you of course will find that the wrinkles or fractures of that part of the surface will take place along the line of the shortest curve, which is on the side; and, as a matter of fact, you have breaks of very much the character of the segments of the Permian batrachia. It may not be so symmetrical as in the actual animal, for organic growth is symmetrical so far as not interfered with; for, when we have two forces, the one of hereditary growth, and the other of change or alteration, and they contend, you will find in the organic being a quite symmetrical result. That is the universal rule. In the cylinder bending both ways, of course the shortest line of curve is right at the centre of the side of that cylinder, and the longest curve is of course at the summit and base, and the shortest curve will be the point of fracture. And that is exactly what I presume has happened in the case of the construction of the segments of the sheath of the vertebral column, by the lateral motion of the animal in swimming, and which has been the actual cause of the disposition of the osseous material in its form. I have gone beyond the state of the discussion in calling attention to one of the forces which have probably produced this kind of result. That is the state of the vertebral column of many of the vertebrata of the Permian period.

I go back to the mammalia, and call attention to the teeth. The ordinary tooth of the higher type of the mammalia, whether hoofed or not, with some exceptions, is complex with crests or cusps. In cutting the complex grinding surfaces we find they have been derived by the infolding of extensions of four original cusps or tubercles. They have been flattened, have been rendered oblique, have run together, have folded up, have become acute, have descended deeply or have lifted themselves, so that we have teeth of all sorts and kinds, sometimes very elegant, and oftentimes very effective in mechanism. In many primary ungulates, the primitive condition of four conical tubercles is found. In passing to older periods we find the mammalia of the Puerco period, which never have more than three principal tubercles, with the exception of three or four species. In the succeeding periods, however, they get the fourth tubercle on the posterior side. Finally, you get a complicated series of grinding or cutting apparatus, as the case may be.

Last, but not least, we take the series of the brain. No doubt the generalization is true, that the primitive forms of mammalia had small brains with smooth hemispheres; later ones had larger brains with complex hemispheres. In general the carnivora have retained a more simple form of brain, while herbivorous animals have retained a more complicated type of brain. The lowest forms of mammalia display the additional peculiarity of having the middle brain exposed; and the hemispheres or large lobes of the brain, which are supposed to be the seat of the mental phenomena, are so reduced in size at the back end that you see the middle brain distinctly, though it is smaller than in reptiles and fishes.

It is beyond the possibility of controversy, that these series have existed, that they have originated in simplicity, and have resulted in complication; and the further induction must be drawn, that the process of succession has been towards greater effectiveness of mechanical work. There are also cases of degradation, as in the growing deficiency in dentition in man. There is no doubt that a large number of people are now losing their wisdom-teeth in both jaws.

We are now brought to the question of the relations which mind bears to these principles. The question as to the nature of mind is not so complex as it might seem. There is a great deal of it, to be sure; but on examination it resolves itself into a few ultimate forms. An analysis reduces it to a few principal types or departments,— the departments of the intelligence and of emotions (with their primary simpler forms, likes and dislikes), and the will, if such there be. These three groups, proposed by Kant, are well known, and are adopted by many metaphysicians; and they stand the scrutiny of modern science as applied to both men and the lower animals. But the question of the material of the mind, the original raw stuff out of which mind was made, is one which is claiming attention now from biologists, as it always has done from physiologists proper, and physicians. This is sensibility, mere simple sensibility, unmodified sensibility, or consciousness. Sensibility, in connection with memory, is sufficient for the accomplishment of wonderful results. It is only necessary to impress the sensibility with the stimuli which this world affords, whether from the outside or the inside, to have the record made, and to have the record kept. Among wonderful things this is perhaps the most wonderful: that a given form of matter should be able to retain a record of events, a record which is made during a greater or less degree of sensibility; which is retained in a state of insensibility; and is finally returned to the sensibility by some curious process of adhesion, as the results of impresses which are found on the material tissue concerned.

And these simple elements of mind are found in animals. No zoologist who has perception or honesty, nor any farmer or breeder, nor any person who has charge of animals in any way, can deny sensibility to all the lower animals at times. The great stumbling-block in the way of the thinker in all this field, is the great evanescence of this sensibility; the great ease with which we dissipate it. The readiness with which we can deprive a fellow-being of his sense is a stumbling-block in more ways than one. While it is a question of the greatest difficulty, nevertheless, like other departments of nature, doubtless it will ultimately be explained by the researches of physiologists. I only need to call attention to consciousness as an important factor in evolution.

We now approach the question of the origin of organic machines with fresh resources. Did the consciousness of the animal find his structure made ready to hand, or did he, under the dire stimuli of necessity, produce through ages these modifications in his own structure? We are told by some of our friends, that law implies a lawgiver, that evolution implies an evolver: the next question is, Where is the lawgiver? where is the evolver? where are they located? This question is best answered, as it appears to me, as follows. In the first place, I may say, it is distinctly proven in some directions, that the constant applications of force or motion in the form of strains, in the form of impacts and blows, upon any given part of the animal organism, do not fail to produce results in change of structure. I believe the changes in the ungulates, to which I have called your attention, are the result of strains and impacts, precisely as I have shown you the manner of the fracture of the vertebral column of the primitive vertebrates of the Permian period. This would require long discussion to render clear; nevertheless, I venture to make the assertion that this series of structures is the result of definite and distinct organic forces, directed to special ends. We have yet to get at the conflicting forces which have produced the results we see. Mechanical evolution will give us a good deal to do for some time to come. Of course, if motion has had an effect in modifying structure, it behooves us to investigate those conditions which give origin to motion in animals. First in order come the sensibilities of the animal, which we have traced to simple consciousness, and stimuli, upon notice of which he immediately begins to move. The primary stimulus of all kinds of motion is necessarily touch. If a stone falls upon the tail of some animal which has a tail, he immediately gets out of that vicinity. If a jelly-fish with a stinging apparatus runs across an eel which has no scales, the eel promptly moves. External applications of unpleasant bodies will always cause an animal to change his location. Then he is constantly assaulted by the dire enemy of beasts, hunger, an instinct which is evidently universal, to judge from the actions of animals. This seems to have fashioned, in large part, all forms of life, from the least to the greatest, from the most unorganized to the most complex. Each exercised itself for the purpose of filling its stomach with protoplasm. Then come the stimuli which should be included under the class of touch, changes of temperature. No animals like to be cold or too hot; and when the temperature is disagreeable, the tendency is to go away from that locality. Among primary instincts must be included that of reproduction. After that comes the sensation of resistance, or, carried to a high degree, of anger: when an animal's interests are interfered with, its movements restricted, the most energetic displays are prompted. So, you see, it is a matter of necessity that mental phenomena lie at the back of evolution, provided always that the connecting link of the argument — that motion has ever affected structure — be true. That is a point which, of course, admits of much discussion. I have placed myself on the affirmative side of that question; and, if I live long enough, I expect to see it absolutely demonstrated.

Of course the development of mind becomes possible under such circumstances. It is not like a man lifting himself up by his boots, which it would be were there no such thing as memory. But with that memory which accumulates, which formulates, first habits, and then structures, especially in the soft, delicate nervous tissue, the development of the function of the mind, as well as the machinery of the mind becomes perfectly possible. We develop our intellect through the accumulation of exact facts; through the collation of pure truth, no matter whether it be a humble kind of truth,— as the knowledge of the changes of the seasons, which induces some animals to lay up the winter's store; whether it be knowledge of the fact that the sting of the bee is very unpleasant; or knowledge of the fact (of which the wild ox, no doubt, is thoroughly aware) that the teeth of the wolf are not pleasant to come in contact with; or whether it be the complex knowledge of man.

When the cerebral matter has become larger and more complex, it receives and retains a much greater number of impressions, and the animal becomes a more highly educated being.

As regards the department of emotions or passions, they are also much stimulated by the environment. Animals which live in a state of constant strife naturally have their antagonistic passions much developed; while amiable, sympathetic sentiments are better and more largely produced by peace-loving animals. Thus it is that the various departments of the mind have the beautiful results which we now find in the human species.

There are some departments of the mind which some of our friends decline to admit having had such an origin. The moral faculty, for instance, is excepted by many from this series. But the reasons why they object to its production in this way are, to my mind, not valid. The development of the moral faculty, which is essentially the sense of justice, appears to them not to fall within the scope of a theory of descent or of evolution. It consists of two parts. First is the sentiment of benevolence, or of sympathy with mankind, which gives us the desire to treat them as they should be treated. It is not sufficient for justice that it is unmixed mercy or benevolence, which is sometimes very injurious, and very often misplaced. It requires, in the second place, the criticism of the judgment, of the mature intellect, of the rational faculty, to enable the possessor to dispose of his sentiments in the proper manner. The combination of rational discrimination and judgment, with benevolence, constitutes the sense of justice, which has been derived, no doubt, as a summary of the development of those two departments of the mind,—the emotions and the intellect.

It is said, that a sense of justice could not be derived from the sense of no justice; that it could not have been derived from the state of things which we find in the animals, because no animal is known to exhibit real justice, and that objection is valid as far as it goes. I suspect that no animal has been observed to show a true sense of justice. That they show sympathy and kindness, there is no question; but when it comes to real justice, they do not display it. But do all men display justice? Do all men *understand* justice? I am very sure not. There are a good many men in civilized communities, and there are many tribes, who do not know what justice is. It does not exist as a part of every mental constitution. I never lived among the Bushmen, and do not know exactly what their mental constitution is; but in a general way the justice of savages is restricted to the very smallest possible circle,—that of their tribe or of their own family. There is a class of people who do not understand justice. I do not refer to people who know what right is, and do not do it; but to the primitive state of moral character, in which, as in children, a sense of justice is unknown. I call attention to the fact, because some of our friends have been very much afraid that the demonstration of the law of evolution, physical and metaphysical, would result in danger to society. I suspect not. The mode in which I understand this question appears to me to be beneficial to society, rather than injurious; and I therefore take the liberty of appending this part of the subject to its more material aspect.

I refer to another topic, that is to the nature of life, and the physical basis of life. The word "life" is so complex that it is necessary to define it, and so to define it away that really the word "life" does not retain its usual definition. Many phenomena of life are chemical, physical, mechanical. We have to remove all these from consideration, because they come within the ordinary laws of mechanical forces; but we have a few things left which are of a different character. One is the law of growth, which is displayed in the processes of embryonic succession; secondly, the wonderful phenomena of sensibility. Those two things we have not yet reduced to any identity with the ordinary laws of force, though we know of their dynamic equivalency. In the phenomena of embryology the phenomena of evolution are repeated, only concentrated in the early stages through which animals have to pass. So whatever explains the general phenomena of evolution explains the phenomena of embryology.

What is the nature of physical sensibility? In this planet, it is found residing only in one form of matter, which has a slightly varied chemical constitution, namely, protoplasm, so called from a physical standpoint. Now, this world, as you all know, has passed through many changes of temperature. Its early periods, it is probable, were so very hot that protoplasm had a very poor chance. Again, can we assume for a moment that this little speck in the great universe is the only seat of life? I suppose scarcely any scientific man will venture do do so. If, therefore, life exists in other planets, worlds and systems, does it necessarily occupy bodies of protoplasm in those different, remote spheres? It would be a great assumption. It is altogether improbable. The certainty is, that in those planets which are in proximity to the sun's heat there could be no protoplasm. Protoplasm in the remote planets would be a hard mineral, and near the sun it would be dissipated into its component gases. So that, if life be found in other parts of this universe, it must reside in some different kind of material. It is extremely probable that the physical conditions that reside in protoplasm might be found in other kinds of matter. It is in its chemical inertness, and in its physical constitution, that its adaptation to life resides; and the physical constitution necessary for the sustenation of life may be well supposed to exist in matter in other parts of the universe. I only say the door is open and not closed: any one who asserts that life cannot exist in any other material basis than protoplasm is assuming more than the world of science will permit him to assume. And that it is confined to this single planet, among the great systems of the universe,—that assumption will not for a moment be allowed. Therefore the subject is one which allows us a free field for future investigation: it is by no means closed in the most important laws which it presents to the rational thinker. I hope, therefore, that, if the evidence in favor of this hypothesis of the creation of living forms be regarded as true, that no one will find in it any ground for any very serious modification of existing ideas on the great questions of right and wrong, which have long since been known by men as a result of experience, and without any other scientific demonstration whatsoever.

SECTION A.

MATHEMATICS AND ASTRONOMY.

ADDRESS

BY

WILLIAM A. ROGERS,

VICE PRESIDENT, SECTION A.

THE GERMAN SURVEY OF THE NORTHERN HEAVENS.

The illustrious Argelander was accustomed to say, in the quaint form of speech which he often employed, "The attainable is often not attained if the range of inquiry is extended too far." In no undertaking is there greater need of a judicious application of this sound maxim than in the systematic determination of the exact positions of all the stars in the visible heavens which fall within the reach of telescopes of moderate power.

The first subject which engaged the attention of the Astronomische Gesellschaft, at its formation in 1865, was the proposition to determine accurately the coördinates of all the stars in the northern heavens down to the ninth magnitude. To this association of astronomers (at first national, but since become largely international, in its character and organization) belongs the credit of arranging a scheme of observations by which, through the coöperation of astronomers in different parts of the world, it has been possible to accomplish the most important piece of astronomical work of modern times. With a feasible plan of operations, undertaken with entire unity of purpose on the part of the observers to whom the several divisions of the labor were assigned, this great work is now approaching completion. While it is yet too early to speak with confidence concerning the definite results which the discussions of all the observations are expected to show, we may with profit consider the object sought in the undertaking, the general plan of the work, the difficulties which have been encountered, and the probable bearing which the execution of the present work will have upon the solution of a problem concerning which we now know absolutely nothing with certainty; a problem of which what we call universal gravitation is only one element, if, indeed, it be an element; a problem which reaches farther than all others into the mysteries of the universe, — the motion of the solar and the sidereal systems in space.

Our first inquiry will be with respect to the condition of the question of stellar positions at the time when this proposal was made by the Gesellschaft in 1865. All the observations which had been made up to this time possess one of two distinct characteristics. A portion of them were made without direct reference to any assumed system of stellar coördinates as a base, but by far the larger part are differential in their character. This remark holds more especially with reference to right ascensions. Nearly all of the observations of the brighter stars made previous to about 1818 were referred to the origin from which stellar coördinates are reckoned, by corresponding observations of the sun; but since that date it has been the custom to select a sufficient number of reference stars, symmetrically distributed both in right ascension and declination, and whose coördinates were supposed to be well known. The unequalled Pulkowa observations for the epoch 1845 form, I believe, the only exception to this statement. From the assumed system of primary stars are derived the clock errors and instrumental constants which are employed in the reduction of all the other stars observed. The positions of these secondary stars, therefore, partake of the errors of the assumed fundamental system, in addition to the direct errors of observation.

The following list comprises the most important of the catalogues which have been independently formed; viz., Bradley for 1755, the various catalogues of Maskelyne between 1766 and 1805, D'Agelet for 1783, Piazzi for 1805, Auwers' Cacciatore for 1805, Bessel for 1815, a few of the earlier catalogues of Pond, Brinkley for 1824, Bessel for 1825, Struve for 1824, Bessel for 1827, Argelander for 1830, and Pulkowa for 1845.

An analysis of the important catalogues of secondary stars published previous to 1865 reveals four important facts:—

1. That nearly all of the observations relate to bright stars, at least to stars brighter than the eighth magnitude.

2. That in a large number of cases the same star is found in different catalogues, but that no rule is discoverable in the selection.

3. That with the exception of the polar catalogues of Fedorenko, Groombridge, Schwerd, and Carrington, the double-star observations of Struve, and the zone observations of Bessel and Argelander, the observations were not arranged with reference to the accomplishment of a definite object.

4. That each catalogue involves a system of errors peculiar to the observers, to the character of the instrument employed, and to the system of primary stars selected, but that thus far there had been no attempt to reduce the results obtained by different observers to a homogeneous system. In estimating the value of these observations it will be necessary to refer to the researches which have been made subsequent to 1865.

The systematic deviations of different catalogues in right ascension, *inter se*, were noticed at an early date by several astronomers; but the first attempt to determine the law of these variations seems to have been made by Safford in a communication to the Monthly Notices of the Royal Astronomical Society in 1861 (xxi, 245), "On the Positions of the Radcliffe catalogue." I quote the equation derived by Safford, since it appears to be the first published account of a form of investigation almost exclusively followed since that time in the treatment of this problem. It is as follows:—

Diff. of R. A. (Greenw. 12 Year Cat. — Rad.) $= -.03\overset{s}{8} + .03\overset{s}{2} \sin (\alpha + \overset{h}{5}\ \overset{m}{32})$. Extending this expression to terms of the second order, it may be put under the form, $\Delta =$ a constant $+ (m \sin \alpha + n \cos \alpha) + (m' \sin 2\alpha + n' \cos 2\alpha)$.

Safford also seems to have been the first to notice the connection between the observed residuals, and the errors in position of the primary stars employed. He remarks, "In investigating the causes which give rise to such systematic discrepancies, I was struck with the fact that the same or nearly the same variations were apparent in the assumed places of the time stars for the years since 1845; that, if the correct positions of the time stars had been assumed, the resulting positions would have been free from these small errors." That the relation given by Safford should have been observed at all is the more remarkable, since the primary stars upon which the Radcliffe positions depend, are nearly the same as those employed at Greenwich. In reality, the systematic errors of both catalogues have since been found to be considerably greater than is here indicated, and the deviation pointed out by Safford is in the nature of a second difference, but the existence of periodic errors in the Greenwich catalogue was not, however, overlooked by him. The speaker has shown (*Proc. Amer. Acad.*, 1874, 182) that the weight of the errors of the provisional catalogue assumed fell between the first and third quadrants in the Radcliffe observations for 1841–42, on account of the omission of certain clock stars which were used at Greenwich.

Since the discordances which exist between two catalogues may arise from errors either in one or in both, it is clearly impossible either to determine the nature of the errors, or to assign their true cause, until a fundamental system has been established which is free both from accidental and from periodic errors, — from accidental errors, since a few abnormal differences may easily invalidate the determination of the errors which are really periodic; from periodic errors, because a relative system can only become an absolute one, when one of the elements of which it is composed becomes absolute.

We owe to the researches of Newcomb, published in 1869–70, a homogeneous system of stellar coördinates in right ascension, which are probably as nearly absolute in their character as it is possible to obtain from the data at present available. He determined the absolute right ascensions of thirty-two stars of the first, second, and third magnitudes, and comprised between the limits —30° and + 46° declination. A comparison of the places of these stars for a given epoch, with the same stars in any catalogue for the same epoch, enables us to determine with considerable precision the system of errors inherent in that catalogue. Several circumstances prevent the exact determination of this relation. Among them may be mentioned the fact that Newcomb's system cannot safely be extended far beyond the limits in declination of the stars composing the system, that the stars are not symmetrically distributed in declination, and that the system of errors derived from bright stars is probably not the same as that derived from stars of less magnitude.

To a certain extent all of these objections have been met in the later discussion by Auwers, to which reference will presently be made. The substantial agreement of these two systems, independently determined, furnishes satisfactory evidence that we have at last obtained a foundation system with which it is safe to make comparisons, from which we may draw conclusions with comparative safety. When the catalogues which were formed between 1825 and 1865 are compared with Newcomb's fundamental system, through the medium of these thirty-two stars, the following facts are revealed: —

a. The only catalogues in which there is freedom from both accidental and periodic errors are Argelander's Äbo catalogue for 1830, and the Pulkowa catalogue for 1845. One is reminded, in this connection, of the remark of Pond, that "we can hardly obtain a better test of our power of predicting the future positions of stars than by trying by the same formula, how accurately we can interpolate for the past. In a variety of papers which I have submitted to the Royal Society, I have endeavored to show, that, with us, the experiment *entirely* fails."

b. During this interval, the constant differences between the earlier catalogues and Newcomb's system vary between + 0.17^s for Pond, 1820; and — 0.19^s for Pond, 1830: and for later catalogues, between + 0.07^s for Cambridge, 1860; and + $.02^s$ for Greenwich, 1860.

c. All the right ascensions determined at English observatories, and especially those which depend upon the positions published by the British Nautical Almanac, are too large in the region of five hours, and too small in the region of eighteen hours. The general tendency of the constant part of the deviation from Newcomb's system is to neutralize the periodic errors in the region of five hours, and to augment them in the region of eighteen hours, where, in the case of a few catalogues, the error becomes as great as 0.10^s, — a quantity which can be readily detected from the observations of two or three evenings with an indifferent instrument, if it relates to a single star.

The right ascensions determined at French observatories exhibit systematic errors, which follow nearly the same law as those which characterize English observations.

Distinctively German observations are nearly free from systematic errors. As far as they exist at all, their tendency is to neutralize the errors inherent in distinctively English and French observations.

d. In the case of several catalogues, residual errors of considerable magnitude remain after the systematic errors depending upon the right ascensions have been allowed for. These errors are found to be functions of the declination of the stars observed, and without doubt have some connection with the form of the pivots of the instruments with which the observations were made. This statement holds true, especially with respect to the observations at Paris, Melbourne, and Brussels, between 1858 and 1871; and to the Washington observations between 1858 and 1861.

e. The systematic errors which exist in observations previous to 1865 follow the same law, and have nearly the same magnitude, as the errors of the same class which are inherent in the national ephemerides of the country in which they were made.

The British Nautical Almanac and the Connaissance des Temps are largely responsible for the perpetuation of this class of errors. For a few years before and after 1860, the ephemerides of the Nautical Almanac were based upon the observations of Pond, which contain large periodic errors. It is found that the errors of this system have been transferred without sensible diminution to every catalogue in which the observations depend upon Nautical Almanac clock stars. At English observatories, it has been the custom to correct the positions of the fundamental stars by the observations of each successive year; but this has produced no sensible effect on the diminution of the periodic errors, which belong to the fundamental system. The periodic errors of the American Ephemeris follow nearly the same law as the errors of the Nautical Almanac, but their magnitude is somewhat reduced. The error of equinox is also less.

Wolfers' *Tab. Reg.*, upon which the Berliner Jahrbuch is based, has no well-defined systematic errors; and the correction for equinox is nearly the same in amount as in the Americam Ephemeris, but with the opposite sign. The accidental errors seem to be rather larger than in the system of the American Ephemeris.

f. A general estimate may be formed of the relative magnitudes of the errors of secondary catalogues by comparing the average error for each star of the primary catalogue. The numbers given below represent the average deviation for each star, expressed in hundredths of seconds, after the various catalogues have been reduced to a common equinox.

		Average error for each star.
Argelander	1830	1.1
Pulkowa	1845	1.1
Greenwich	1845	2.0
Greenwich	1860	2.0
D'Agelet (Gould)	1783	2.2
Cape of Good Hope (Henderson)	1833	2.2
Greenwich	1850	2.2
Greenwich	1871	2.2
Paris	1867	2.4
Washington	1846–52	2.5
Struve	1830	2.5
Cape of Good Hope	1856	2.8
Radcliffe	1860	3.1
Greenwich	1840	3.1
Bessel	1825	3.2
Pond	1830	3.7
Gillis	1840	3.8
Madras (Taylor)	1830	3.9
Cape of Good Hope (Fallows)	1830	3.9
Radcliffe	1845	4.5
Armagh	1840	5.0
Piazzi	1800	5.3
Bessel's Bradley	1755	7.9
Lalande	1800	13.2
Lacaille	1750	24.9

It is obvious from these relations, that previous to about 1825 the magnitude of the accidental errors of observation, combined with the errors of reduction, prevents any definite conclusions with respect to the periodic errors inherent in these early observations. It is probable, also, that early observations of stars of the eighth and ninth magnitudes are subject to a class of errors peculiar to themselves, the nature of which it is now well nigh impossible to determine.

The systematic errors in declination which belong to the various secondary catalogues named are even more marked than those in right ascension. The experience of Pond in 1833 is the experience of every astronomer who has attempted to compare observations of the same star made at different times, under different circumstances, with different instruments, and by different observers. He says, "With all these precautions, we do not find, by comparing the present observations with those of Bradley made eighty years ago under the same roof, and computed by the same table of refractions, that we can obtain by interpolation any intermediate catalogue which shall agree with the observations within the probable limits of error."

We owe to the investigations of Auwers (*Astron. Nachr.*, nos. 1532–1536), the first definite system of declinations which is measurably absolute in its character. Yet the deviations of this system from that derived by the same author, but from much additional data in Publication xiv of the Gesellschaft, is no less than 1″.2. The present difference outstanding between the Pulkowa and Greenwich systems at 10° south declination is 1″.7.

Within the past five years, the labors of Auwers, of Safford, of Boss, and of Newcomb, have resulted in the establishment of a mean system of declinations from which accidental errors may be considered to be eliminated in case of a large number of stars; but the different systems still differ systematically, *inter se*, by quantities which are considerably greater than the probable error of any single position.

When the discussion of the question of a uniform determination of all the stars in the northern heavens to the ninth magnitude was taken up by the Gesellschaft at its session in Leipsic in 1865, Argelander, who was then president of the society, appears to have been the only astronomer who had a clear apprehension of the difficulties of the problem. He alone had detected the class of errors whose existence subsequent investigations have definitely established. He alone had found a well-considered plan by which these errors might be eliminated, as far as possible, from future observations.

Argelander, however, always claimed for Bessel the first definite proposal of the proposition under consideration (see *Astron. Nachr.*, vol. i, 257). It was in pursuance of this plan that the zones between —15° and +15° in declination were observed. These zones were to form the groundwork of the Berlin charts; and Argelander, in the execution of the Bonn Durchmusterung simply carried out the second part of Bessel's recommendation.

These two great works — the second being a continuation of the first, under a better and more feasible plan — are the only ones in existence which give us any knowledge of the general structure of the stellar system, with the exception of the observations of Cooper at Makree observatory, and the charts of Chacornac.

The observations of stars to the ninth magnitude, found in the catalogues of Bessel, Lalande, and Piazzi, form the groundwork of the Berlin charts. The coördinates in right ascension and declination of the stars found in these authorities were first reduced to the epoch 1800; the resulting right ascension being given to seconds of time, and the declination to tenths of minutes of arc. With these places as points of reference, all other stars were filled in, down to the ninth magnitude, by observations with equatorial instruments. The work was divided into zones of one hour each. Bremiker undertook five zones; Argelander and Schmidt, two; Wolfers, three; and Harding, two. The remaining zones were undertaken by different astronomers in widely separated localities.

The work seems to have been performed with somewhat unequal thoroughness, some zones containing nearly all the stars to the ninth magnitude, while in others a large number of stars having this limit in magnitude are wanting.

The Durchmusterung undertaken by Argelander at Bonn was a far more serious and well-considered undertaking. This unequalled work consists in the approximate determination of the coördinates of 324,198 stars situated between —2° and + 90° declination. It includes stars to the 9.5 magnitude, the coördinates being given to tenths of seconds of time, and the declinations to tenths of minutes of arc.

The first definite proposal of the work undertaken by the Gesellschaft, however, appears to have been made by Bruhns. In the course of a report upon the operations of the Leipsic observatory, he stated, that, in his view, the time had come for undertaking a uniform system of determinations of the places of stars to the ninth magnitude in the northern hemisphere by means of meridian circles but he proposed at the same time that the position of stars fainter than the ninth magnitude should be determined by means of differential observations with equatorial instruments. After explaining certain plans and arrangements relating particularly to his own observatory, he introduced the following resolution: —

"The Astronomische Gesellschaft regards it as needful that all the stars to the ninth magnitude, occurring in the Durchmusterung, should be observed with meridian circles, and commissions the council to arrange for the execution of the work."

This proposal occasioned a long and somewhat animated discussion, in which Argelander, Hirsch, Bruhns, Förster, Schönfeld, and Struve took part.

Argelander declared himself surprised at this proposal, which called for the rapid realization of a plan of organization which he had been considering for years with the greatest care, the difficulties of which he had maturely considered, and the execution of which still demanded the most careful deliberation and preparation. One of the necessary preliminary steps was a plan which he had already prepared, published and presented to the society in an informal way, which provided for contemporaneous and corresponding observations of the brighter stars. As president of the society, he felt unequal to undertaking the charge which the acceptance of the resolution proposed would involve; as this procedure seemed to him premature without previous preparation. He would admit, however, that every call to action of this kind tended to stimulate enthusiasm, and should therefore be encouraged; but he felt obliged to ask the society not to require from him the immediate execution of the plan, but to intrust the serious consideration of it, and the preparation for it, to his zealous friends in the council.

Upon the motion of Struve, the society, by a rising vote, expressed its confidence in the assurance of the president that he would bring forward his plan at the proper time, as soon as the means for its execution could be assured.

At the meeting held at Bonn in 1867, Argelander again brought up the subject in a communication which appears to have been an exhaustive discussion of the whole problem. This paper is not printed in the proceedings of the Gesellschaft; but at its conclusion a committee was appointed to take definite action with respect to the recommendations which it contained. The committee reported at the same session; and their report, which is published in the place of the paper presented by Argelander, is probably identical in substance with it. The plan proposed and adopted was finally published in the form of a programme, in which the details of the work are arranged with considerable minuteness. As this programme has been widely distributed, it seems unnecessary to give anything more than a general abstract of it. Since it differs in a few minor points from the first report of the committee at the Bonn meeting, the essential features of this report will be given instead of an abstract of the programme itself.

They are as follows: —

a. The limits in declination of the proposed series of observations are — 2° and + 80°. The first limit was chosen on account of the lack of suitable fundamental stars south of the equator. It is probable, also, that Argelander had a suspicion of the fact, since proven, that the uncertainty with respect to the systematic errors of southern stars is, of necessity, considerably greater than for northern stars, and that on this account it would be better to defer this part of the work until further investigations in this direction could be made.

The limit + 80° was chosen because the repetition of Carrington's observations between 81° and 90° was considered superfluous, and Hamburg had already undertaken the extension of Carrington's observations from 81° to 80°.

b. Within these limits, all stars in the Durchmusterung to the ninth magnitude, and, in addition, all stars which have been more exactly observed by Lalande, by Bessel at Koenigsberg, and by Argelander at Bonn, are to be observed.

c. The observations are to be differential. The clock errors are not to be found from the fundamental stars usually chosen for this purpose, and the equator point corrections are not to be derived from observations at upper and lower culminations, but these elements are to be derived from a series of 500 or 600 stars, distributed as uniformly as possible over the northern heavens. The exact coördinates of these are to be determined at Pulkowa, thus securing the unity necessary in order to connect in one system the observations of different zones.

d. Every star is to be observed twice. If the two observations differ by a quantity greater than ought to be expected, a third observation will be necessary.

e. In order to facilitate the work, it will be desirable to use only three or four transit threads, and only one or two microscopes.

In order to facilitate the reductions to apparent place, the working-list of stars should be comprised within narrow limits.

f. Before the commencement and after the close of each zone, two or three fundamental stars are to be observed upon the same threads and with the same microscopes as were used in the zone observations. When the seeing is not good, and when for any other cause it seems desirable, one or more fundamental stars may be observed in the course of the zone. The number and selection of the stars will depend upon the character of the instrument employed. If it remains steady for several hours, and has no strongly marked flexure or division errors, or if these errors have been sharply determined, the fundamental stars may be situated ten degrees or fifteen degrees away from the zone limits. However, there must remain many things for which no general rule can be given, and which must be left to the judgment of the observer, aided by an accurate knowledge of his instrument.

g. With a Repsold or a Martin instrument, one microscope will be sufficient, if its position with respect to the whole four can be determined. It will be sufficient, if the change in position during the observations can be interpolated to 0.2″.

h. It will be desirable to divide beforehand the zones into such time intervals that the observations can be easily made.

i. Zones exceeding one and one-half or at the most two hours are not advisable, first, because the zero points will be too far apart, and secondly, because a longer duration will involve too much fatigue physically and mentally.

At the conclusion of this report, all the astronomers present who were willing to take part in this work were requested to communicate with the council, stating the region of the heavens which they preferred to select for observation.

At this meeting, Berlin, Bonn, Helsingfors, Leipsic and Mannheim, signified their intention to share in the work. Leiden also expressed its intention of taking part as soon as the work already undertaken should be completed.

When the stars to be observed had been selected from the Durchmusterung, it was found that the number would not vary much from 100,000, requiring rather more than 200,000 observations.

Preparations for the work of observation were immediately commenced; and, by the time of the next report in 1869, considerable progress had been made.

In the report for this year, the provisional places of a catalogue of 539 fundamental stars were published. This catalogue is composed of two parts. The list of Hauptsterne consisting of 336 stars to the fourth magnitude, had been previously observed at Pulkowa by Wagner with the large transit instrument, and by Gyldén with the Ertel vertical circle. The list of Zusat-sterne consists of 203 stars fainter than the fourth magnitude. As the details of the work in the formation of the provisional places of the stars of this list are not given in the report, it is not quite clear upon what authority they rest. The work assigned to the Pulkowa observatory by the zone commission was the exact determination of the places of the stars of this list. The observations were undertaken by Gromadski with the Repsold meridian circle. In accordance with the plan adopted, each star was observed eight times,— four times in each position of the instrument. The observations were differential with respect to the Hauptsterne.

The results were published by Struve in 1876; and the places there given were used in the first reduction of the Harvard College observations for 1874–75, and perhaps in some other cases also.

About this time a change seems to have been made in the original plan with respect to the formation of the final catalogue of fundamental stars, of which I have been unable to find a clear account. The original intention was to make the positions depend entirely upon the observations at Pulkowa. The zone commission established by the Gesellschaft, however, committed the formation of this catalogue to Auwers; and it is to him that we owe the most complete and most perfect catalogue of fundamental stars yet published. The Pulkowa system for 1865 was adopted as the basis; but, in order to obtain greater freedom from accidental stars, the final catalogue was obtained by combining with the Pulkowa series, the Greenwich observations from 1836 to 1876, the Harvard College observations for 1871–2, the Leipsic observations, in declination only, between 1866 and 1870, and the Leiden observations in declination between 1864 and 1870. Before this combination was made, however, these observations were all reduced to the Pulkowa system.

The following observatories have taken part in the zone observations:—

Observatories.	Limits of zones in declination.	Observatories.	Limits of zones in declination.
Nikolaiew	— 2° to + 1°	Lund	+35° to +40°
Albany	+ 1 " + 5	Bonn	+40 " +50
Leipsic	+ 4 " +10	Harvard College .	+50 " +55
Leipsic	+10 " +15	Helsingfors . . .	+55 " +60
Berlin	+15 " +25	Christiana . . .	+65 " +70
Cambridge (Eng.) .	+25 " +30	Dorpat	+70 " +75
Leiden	+30 " +35	Kasan	+75 " +80

The zone between —2° and +1° was originally undertaken at Palermo, that between +1° and +4° at Neuchâtel, that between +4° and +10° at Mannheim, and that between +35° and +40° at Chicago.

In the latter case, the great fire at Chicago crippled the resources of the observatory to such an extent, that Safford was compelled to relinquish the work, which was at that time quite far advanced.

The chief items in connection with this work are found in the accompanying tabular statement.

Attention was called, at an early date, to the importance of continuing the survey of the northern heavens beyond the southern limit fixed by Argelander. The preparation necessary for the execution of this work consisted in the extension of the Durchmusterung to the tropic of Capricorn. This was undertaken by Schönfeld.

In the report to the Gesellschaft at the meeting held at Stockholm in 1877, he has given an account of this work, in which he stated that it was sufficiently near completion to invite the consideration of the question of the meridian circle determinations of the places of stars to the ninth magnitude. The lack of southern fundamental stars whose positions were well determined was still a hinderance to the immediate commencement of the work. Relatively more stars of this class are required than in the northern observations, in order to eliminate the inequalities due to refraction. Schönfeld stated, that, while the burden of the determination of the places of these southern fundamental stars must rest mainly upon southern observations, it seemed necessary to connect them with the Pulkowa system by a connecting link (Mittel-

eader.	Observations begun.	Observations completed.	Whole No. of observations of zone stars.	Whole No. of observations of fundamental stars.
…………	June 1877	—	4400	800
………… …………	Aug. 1878	Aug. 1882	20480	3000
…………	—	Apr. 1883	35000	400 stars observed.
…………	June 1869	June 1872	22532	2910
…………	Nov. 1879	Feb. 1883	24107	3556
…………	— 1872	—	10332 stars observed.	74 stars observed.
………… ………… akhbuyzen. …… …………	Sept. 1870	May 1876	21000+	2500
………… …………	Apr. 1879	Oct. 1882	23147	2400
………… ………… ………… ………… …………	Mar. 1869	—	41000	6300
………… ………… ………… …………	Nov. 1870	Jan. 1879	22400	15200
………… ………… ………… …………	Aug. 1869	Aug. 1880	33756	4 or more stars for each zone.
…………	Feb. 1870	Sept. 1881	11019	1861
	Mar. 1870	June 1882	3400 stars observed.	—
	—	—	—	—

glied), through observations at some observatory well situated for this purpose. At this meeting Sande Bakhuyzen, at Leiden, gave notice of intention to take part in this work. Gyldén urged the importance of securing the coöperation of Melbourne; and Peters suggested the advantage of securing Washington as an additional "mean term" (V. J. S., 1877, p. 265).

The next reference to this work is contained in the Vierteljahrsschrift for 1881, xv, p. 270. A list of three hundred and three southern stars is here given, whose exact places were at that time being determined at Leiden and at the Cape of Good Hope. This list was selected by Schönfeld and Sande Bakhuyzen, in a way to meet the requirements referred to in previous discussions.

A final catalogue of eighty-three southern fundamental stars by Auwers appears in this number of the Vierteljahrsschrift. The places depend upon the same authorities as for the northern stars, with the addition of the Cape of Good Hope catalogue for 1860, Williamstown, Melbourne for 1870, and Harvard College (Safford) for 1864. For stars not observed at Pulkowa, the general catalogue of Yarnall (1858–61), and the Washington observations, with the new meridian circle between 1872 and 1875, were employed. As in the case of the northern stars, these observations are all reduced to the Pulkowa system for 1865. It is understood that the coördinates of the list of three hundred and three stars are to depend upon this extension of the general system of Publication xiv, to the limits required by the southern Durchmusterung of Schönfeld.

It would be surprising if all the conditions of success were fulfilled in the first execution of a work having the magnitude, and involving the difficulties, of the scheme of observations undertaken under the auspices of the Gesellschaft. The extent of the discordances which are to be expected between the results obtained by different observers can only be ascertained when the observations by which the different zones are to be connected have been reduced. Each observer extended the working-list of his own zone 10′ north and south; and it is expected that a sufficient number of observations of this kind have been made to determine the systematic relations existing between the coördinates of each zone with those of its neighbor.

It is probable, however, that the experience of Gill will be repeated on a larger scale. In 1878 he solicited the coöperation of astronomers in the determination of the coördinates of twenty-eight stars, which he desired to employ in the reduction of his heliometer observations of the planet Mars for the purpose of obtaining the solar parallax. The results obtained at twelve observatories of the first class are published in vol. xxxix, p. 99, of the Monthly Notices of the Royal Astronomical Society. Notwithstanding the fact that the final values obtained at each observatory depend upon several observations, the average difference between the least and the greatest results, obtained by different observers for each star, is 0.24ˢ in right ascension and 2″.3 in declination. In four cases the difference in right ascension exceeds 0.30ˢ, and in four cases the difference in declination exceeds 3″.0.

Even after the results are reduced to a homogeneous system, the following outstanding deviations from a mean system are found :—

Authority.	Δ α	Δ δ	Authority.	Δ α	Δ δ
	s.	″		s.	″
Koenigsberg . .	+.006	—0.71	Leiden . . .	—.053	—0.19
Melbourne . . .	+.026	—0.49	Paris	+.055	+0.01
Pulkowa . . .	+.005	+0.36	Washington . .	—.120	+0.78
Leipsic	+.049	+0 40	Harvard College,	—.072	+0.09
Greenwich . . .	+.009	—0.56	Cordoba . . .	—.032	—0.20
Berlin	+.044	+0.67	Oxford . . .	+.076	+0.21

The observations of a second list of twelve stars, one-half of the number being comparatively bright, and the remaining half faint, showed no marked improvement, either with respect to the magnitude of errors which could be classed as accidental, or in regard to the systematic deviations from a mean system.

This discussion revealed one source of discordance which will doubtless affect the zone observations, viz.: the difference between right ascensions determined by the eye-and-ear method, and those determined with the aid of the chronograph.

The programme of the Gesellschaft makes no provision for the elimination of errors which depend upon the magnitude of the stars observed ; but special observations have been undertaken at several observatories for the purpose of defining the relation between the results for stars of different magnitudes. At Harvard College observatory, the direct effect of a reduction of the magnitude has been ascertained by reducing the aperture of the telescope by means of diaphragms. Besides this, the observations have been arranged in such a manner that an error depending upon the magnitude can be derived from an investigation of the observations upon two successive nights.

At Leiden, at Albany, and perhaps at other observatories, the effect of magnitude has been determined by observations through wire gauze. But notwithstanding all the precautions which have been taken in the observations, and which may be taken in the reductions, it will undoubtedly be found that the final results obtained will involve errors which cannot be entirely eliminated.

In the experience of the speaker, two other sources of error have been detected. It has been found, that there is a well-defined equation between the observations, which is a function of the amount, and the character of the illumination of the field of the telescope. It has also been found that observations made under very unfavorable atmospheric conditions differ systematically from those made under favorable conditions. When the seeing was noted as very bad, it is found that the observed right ascensions are about .08ˢ too great, and that the observed declinations are about 0″.8 too great.

There are doubtless other sources of error which the discussion of the observations will bring to light. The effect of the discovery of these and other errors will probably be to hasten the repetition of the zone observations under a more perfect scheme, framed in such a manner as to cover all the deficiencies which experience has revealed, or may yet reveal. The labor involved in this supplemental survey would not be very great, since the object sought would be accomplished by a special programme of observation, involving perhaps one-tenth of the number of observations made in the present survey. One would not probably go far astray in naming the year 1900 as the mean epoch of the new survey. If the observations are again repeated in 1950, sufficient data will then have been accumulated for at least an approximate determination of the laws of sidereal motion.

What is the present state of our knowledge upon this subject? It can be safely said that it is very limited. First of all, it cannot be affirmed that there is a sidereal system in the sense in which we speak of the solar system. In the case of the solar system, we have a central sun about which the planets and their satellites revolve in obedience to laws which are satisfied by the hypothesis of universal gravitation. Do the same laws pervade the interstellar spaces? Is the law of gravitation indeed universal? What physical connection exists between the solar system and the unnumbered and innumerable stars which form the galaxy of the heavens? Do these stars form a system which has its own laws of relative rest and motion? or is the solar system a part of the stupendous whole? Does the solar system receive its laws from the sidereal system? or did Newton indeed pierce the depths of the universe in the discovery of the law which gave him immortality? Are we to take the alternative stated by Ball,—either that our sidereal system is not an entirely isolated object, or its bodies must be vastly more numerous or more massive than even our most liberal interpretation of observations would seem to warrant? Are we to conclude, for example, that stars like 1830 Groombridge and *a* Centauri, "after having travelled from an infinitely great distance on one side of the heavens, are now passing through our system for the first and only time, and that after leaving our system they will retreat again into the depths of space to a distance which, for anything we can tell, may be practically regarded as infinite?" Can we assert with Newcomb, that in all probability the stars do not form a stable system in the sense in which we say that the solar system is stable, that the stars of this system do not revolve around definite attractive centres? Admitting that the solar system is moving through space, can we at the present moment even determine whether that motion is rectilinear or curved, to say nothing of the laws which govern that motion? How much of truth is there in the conjectures of Wright, Kant, Lambert, and Mitchel, or even in the more serious conclusions of Mädler, that Alcyone of the Pleiades is the central sun about which the solar system revolves?

These are questions which, if solved at all, must be solved by a critical study of observations of precision accumulated at widely separated epochs of time. The first step in the solution has been taken in the systematic survey of the northern heavens undertaken by the Gesellschaft, and in the survey of the southern heavens at Cordoba by Dr. Gould. The year 1875 is the epoch about which are grouped the data which, combined with similar data for an epoch not earlier than 1950, will go far towards clearing up the doubts which now rest upon the question of the direction and the amount of the solar motion in space; and it cannot be doubted that our knowledge of the laws which connect the sidereal with the solar system will be largely increased through this investigation. The basis of this knowledge must be the observed proper motions of a selected list of stars, so exactly determined that the residual mean error shall not affect the results derived; or, failing in this, of groups of stars symmetrically distributed over the visible heavens, sufficient in number to affect an elimination of the accidental errors of observation, without disturbing the equilibrium of the general system.

For an investigation of this kind, a complete system of zone observations, at widely separated intervals, will afford the necessary data, if the following conditions are fulfilled.

First: The proper motions must be derived by a method which does not involve an exact knowledge of the constants of precession. In every investigation with which I am acquainted, the derived proper motions are functions of this element.

Second: The general system of proper motions derived must be free from systematic errors. Errors of this class may be introduced either through the periodic errors inherent in the system of fundamental stars employed in the reduction of the zone observations, or in a change in the constants of precession. It is in this respect that the utmost precaution will be required. If from any cause errors of even small magnitude are introduced into the general system of proper motions at any point, the effect of these errors upon the values of the coördinates at any future epoch will be directly proportional to the interval elapsed. We can, therefore, compute the exact amount of the accumulated error for any given time.

When this test is applied to the stellar systems independently determined by Auwers, Safford, Boss and Newcomb, we find the following deviations *inter se* at the end of a century.

	Maximum mean deviation in a century.		Maximum systematic deviation in a century.	
	$\Delta\alpha$	$\Delta\delta$		
Auwers *minus* Safford . . .	*—0.22s.	+0.2"	0.23s.	1.1"
Auwers *minus* Boss	-	+0.8	-	2.1
Auwers *minus* Newcomb . .	—0.09	+0.8	0.06	2.2

*With Bessel's constants of precession.

It is the common impression, that both the direction and the amount of the motion of the solar system in space are now well established. The conclusions of Struve upon this point are stated in such explicit language that it is not surprising that this impression exists. He says, "The motion of the solar system in space is directed to a point in the celestial sphere situate on the right line which joins the two stars of the third magnitude π and ω Herculis at a quarter of the apparent distance between these stars measured from π Herculis. The velocity of this motion is such that the sun, with the whole cortége of bodies depending on him, advances annually in the direction indicated, through a space equal to 1.623 radii of the terrestrial orbit or one hundred and fifty-four millions of miles."

It must be admitted that there is a general agreement in the assignment by different investigators of the coördinates of the solar apex. This will be seen from the following tabular values:

Authorities.	Right ascension.	Declination.
Herschel, 1783	257° 00′	+25° 00′
Prevost	230 00	+25 00
Klugel, 1789	260 00	+27 00
Herschel, 1805	245 52	+49 38
Argelander, 1837	259 52	+32 29
Lundahl	252 24	+14 26
Struve	261 23	+37 36
Galloway	257 04	+34 18
Mädler	261 38	+39 54
Airy	256 54 261. 29	+39 29 +26 44
Dunkin	261 14 263 44	+32 55 +25 00

In estimating the value which should be attached to these results, several considerations must be taken into account.

(*a*) All of the results except those of Galloway depend practically upon the same authorities at one epoch, viz., upon Bradley.

(*b*) The deviations *inter se* probably result, in a large measure, from the systematic errors inherent in one or both of the fundamental systems from which the proper motions were derived. For example, Lundahl employed Pond as one of his authorities, and it is in Pond's catalogue that the most decided periodic errors exist.

(*c*) Biot in 1812, Bessel in 1818, and Airy in 1859, reached the conclusion that the *certainty* of the movement of the solar system towards a given point in the heavens could not be affirmed.

(*d*) The problem is indirect. In the case of a member of the solar system, exact data will determine the exact position in orbit at a given time; but here we have neither exact data, nor can we employ trigonometrical methods in the solution. We simply find that the observed proper motions are probably somewhat better reconciled under the hypothesis of an assumed position of the apex of the solar motion. The method of investigation employed by Safford, who has of late years given much attention to this subject, consists in assuming a system of coördinates for the pole of the solar motion, from which is determined the direction each star would have if its own proper motion were zero. Comparing this direction with the observed direction as indicated by the observed proper motion, equations of condidition are formed from which a correction is found to the assumed position of the apex, by the method of least squares.

It must always be kept in mind, that the quantities with which we must deal in this investigation are exceedingly minute, and that the accidental errors of observation are at any time liable to lead to illusory results. The weak link in the chain of Mädler's reasoning is to be found here. I think we can assume $0''.2$ as the limit of precision in the absolute determination of the coördinates of any star, however great the number of observations upon which it depends. Beyond this limit it is impossible to go, in the present date of instrumental astronomy. It is safe to say, that there is not a single star in the heavens whose coördinates are known with certainty within this limit. Do not misunderstand me. Doubtless there are many stars in which the error will at some future time be found to fall within this limit. But who is prepared to select a particular star, and say that the absolute position of this star in space cannot be more than $0''.2$ in error?

(*e*) At present an arbitrary hypothesis is necessary in the discussion of the problem. Airy assumed that the relative distances of the stars are proportional to their magnitudes; and he found slightly different results according to different modes of treatment. Safford in his investigation made use of a device which he had previously employed in the computation of the differential coëfficients of planetary elements, thereby reducing to a certain extent the magnitude of the residuals between observation and computation to quantities of the second order. The axis of Z was located in the direction of Argelander's determination of the apex of the solar motion or in right ascension 259° 50″.8 and declination +32° 29′.1. The axis of X was placed in the equator in right ascension 349° 50′.8 and that of Y in right ascension 79° 50′.8 and in declination 57° 30′.9. When the reductions to these three axes were made, it was found that nearly all of the motion was along the axis of Z as was to be expected from the assumption made. The tendency to move in this direction was therefore primarily assumed to represent the solar motion. Since it was found that the *average factor*, by which the proper motion must be multiplied in order to represent this tendency, changes only in a slight degree with the magnitude of the proper motion itself, the proper motion was assumed, as a first approximation, to be inversely proportional to this element.

Later investigations have been made by De Ball (Ueber die eigene Bewegung des Sonnensterns, Bonn, 1877) but the details have not yet come to hand. It is understood, however, that his results coincide in a general way with those previously obtained.

The most recent investigation is by Bolte. His paper may be considered as a numerical application of the formulæ by which Schönfeld has attempted to express the relation between the motion of the solar system and a systematic rotation of the fixed stars which is assumed to take place in a direction perpendicular to the plane of the Milky Way. The values of the precession constants derived are certainly interesting, but they can hardly be considered decisive, especially in view of the fact that the values derived from the observed declinations alone do not agree especially well with the values derived from the right ascensions.

It is clear from this brief review, that we have here a field of investigation worthy of the highest powers of the astronomer. The first step has been taken in the survey of the heavens carried on under the auspices of the Gesellschaft. It remains for the astronomers of the present generation to solve the difficulties which now environ the problem, and to prepare the way for a more perfect scheme of observation in the next century.

PAPERS READ.

The total Solar Eclipse of May 6, 1883. By Edward S. Holden of Madison, Wisconsin.

[Abstract.]

This eclipse had the longest totality of any which has been observed.

An expedition was sent by the National Academy of Sciences and the United States Coast Survey jointly, under direction of a committee from the former. Expenses were met by an appropriation of $5,000 by Congress and by the National Academy of Sciences from a fund left by Professor Watson. The navy department also placed the United States steamer Hartford at the disposal of the academy, to transport the expedition from Peru to Caroline Island, where the eclipse was to be observed, and thence to Honolulu.

The efforts of Mr. Rockwell to provide money by private subscription for this undertaking, though directly unsuccessful, prepared the way by drawing public attention.

Professor Young was the chairman of the committee of the National Academy of Sciences: it was at one time hoped that he would take charge of the observing party, but this proved impracticable. The reports of different members of the party are to be submitted to the National Academy of Sciences in November. Mr. Holden has, however, permission of the academy to present an account of the observation before the American Association. It is understood that the present is not by any means a final report. This especially applies to the observations of Dr. Hastings, from which that gentleman concludes that the solar corona is chiefly a phenomenon due to the diffraction of the solar light at the moon's limb. The computations to demonstrate this are not yet at hand, but are to be completed in a few weeks.

The American party consisted of Edward S. Holden, director of Washburn Observatory, Madison, Wis.; Charles S. Hastings, professor of physics in the Johns Hopkins University, Baltimore, Md.; Charles H. Rockwell, Tarrytown, N. Y.; E. D. Preston, aid U. S. Coast and Geodetic Survey, Washington, D. C.; Winslow Upton, U. S. Signal-office, Washington, D. C.; and Ensign S. J. Brown, U. S. N., U. S. Naval Observatory, Washington, D. C.

The original six members of the party were joined, on April 20, by four volunteer observers, all officers of the U. S. ship Hartford: these were Lieut. E. F. Qualtrough, U. S. N.; Passed Assistant-surgeon W. S. Dixon, U. S. N.; Midshipman W. S. Fletcher, U. S. N.; and Midshipman J. G. Doyle, U. S. N.

On March 11 the party was strengthened by the joining (at Colon) of the two English gentlemen who were sent out by the Royal Society of London to make photographic observations of the eclipse, under instructions from J. Norman Lockyer, Esq., F. R. S., and Capt. W. de W. Abney, R. E., of the Science and Art Department of the South Kensington Museum. These were H. A. Lawrance, London, Eng., and C. Ray Woods, London, Eng.

During the stay of the party on Caroline Island (April 21 to May 9) ten petty officers and men of the Hartford remained, and rendered very intelligent assistance.

In all, the party on the island consisted of twenty-two persons.

After giving details of the proceedings of the expedition, its arrival, and the preparations for the eclipse, Mr. Holden states, as to the event itself, that the following atmospheric conditions prevailed: the sky proved clear at first contact, cloudy at intervals till near totality, clear during totality except a slight haze during its first minute, cloudy a few minutes after third contact, and finally clear at fourth contact.

The meteorological observations were made by Mr. Upton, and are especially noteworthy. In two weeks, April 25 to May 9, twenty showers were recorded; but the rainfall in each was very small, the total in the two weeks being about eight inches. Half of this fell during the only considerable disturbance of the weather, which took place May 4, when it rained from midnight to 9.50 A. M.

The barometer was notably uniform. Its diurnal movements were plainly marked; the maxima being at 9 A. M. and P. M., the minima at 3 A. M. and P. M. The indications of the thermometer were very constant. The daily range was 9.3°, the highest reading was 89.3°, the lowest 72.4°, the daily maximum at *noon*, the minimum at 6 A. M. The relative humidity ranged from 70 per cent at midday to 84 in early morning, and at no time fell below 61. The island lies in the region of the south-east trades, but the wind (which was very steady) blew constantly between north and east. The average velocity of the wind was 6.05 miles; the largest during twenty-four hours was 212 miles, the least 59 miles; the highest velocity, registered in a squall, was 16 miles per hour.

The botanical and zoölogical observations are not yet ready for publication. During the voyage a series of observations was made by Mr. Upton on southern variable stars. Dr. Hastings and Mr. Holden, while on the island, discovered twenty-three new double stars, a list of which has appeared in SCIENCE.

In preparing for the eclipse, Mr. Holden assigned to each observer a single duty, not requiring him to move from one instrument to another. The entire field of photography was left to the English party accompanying our own, and to the French party under M. Janssen, who were very successful in photographing the corona.

The combination of polariscope and telescope was used, but not with successful results, the apparatus proving unsuitable. Dr. W. S. Dixon, who attended to a telescopic examination of the details of the inner corona, will report on the same separately, giving a drawing of the corona. With the spectroscope, the chief point of the observation of Dr. Hastings was as to the relative lengths of the line 1474 east and west of the sun. At second contact, this line was 12′ in length east and 3′ west. The length of 1474 east diminished, while 1474 west increased. At mid-totality these were equal. Before the third contact, the appearances were reversed: 1474 west was longer and brighter than 1474 east.

At the beginning of totality, the lines C, D_3, F, and (near G) were seen brilliant but very short. At mid-eclipse the spectrum was deliberately examined. On a continuous spectrum, two lines only were seen: 1474 *bright*, and the D line *dark*. C, E, b, F, were certainly wanting. Near the end of totality, C, D_3, and F appeared again, very short. Mr. Upton reports that five seconds after second contact, four curved lines were seen,— C, D_3, 1474, F. A light cloud passed over the sun; and on its disappearance the spectrum showed a small line, of about one-third the height of the others, between 1474 and *F.* One hundred seconds after second contact, three coronal rings took the place of the lines; they were red, yellowish-green, and green, and are supposed to be *C*, D_3, and 1474. Two hundred seconds after second contact, the red ring was decidedly the brightest, and it continued to increase in brightness during sixty seconds. Two hundred and ninety seconds after second contact, the four curved lines, *C*, D_3, 1474, *F*, appeared. The reversal of the bright lines at third contact was observed. The change was instantaneous, or nearly so. The reversal of the Fraunhofer lines was not seen. The only bright line seen for the first 190 seconds was 1474. A dark line was seen, which was probably *D*.

Mr. Rockwell, using a Rutherford grating and a narrow slit tangential to the limb, reported that 1474 *K* was not seen until a minute and a half had passed. It was followed 4′ or 5′ west of the limb, twice; and it was seen only on the western side of the moon. Two green lines were also seen, each brighter and broader than 1474, but much shorter.

Due credit is given by Mr. Holden to each of the observers of the party. His own observations were confined to a search for the planet Vulcan, reported to exist by Professors Watson and Swift. Mr. Holden's search continued during the whole of totality (five minutes and twenty-five seconds), with a six-inch telescope with a power of 44 and field of 57′ in declination. He saw every star on the map which he had previously published in SCIENCE (Feb. 23, 1883), down to the sixth magnitude, inclusive, except the three sixth magnitude stars nearest to the sun; and he saw only these stars. One of the stars of the map was of the same magnitude as Watson's "Vulcan." This was a conspicuous object. No star half so bright as this could possibly have escaped observation. Mr. Holden is therefore confident that Vulcan does not exist within the limits swept over. Mr. Holden also determined the direction of the motion of the diffraction bands before and after totality. This was an observation which he could not make successfully in Colorado in 1878, and which he believes has not been before made

INTERNAL CONTACTS IN TRANSITS OF THE INFERIOR PLANETS. By J. R. EASTMAN of Washington, D.C.

[ABSTRACT.]

FROM the general discussion which followed the announcement of the results from the modern reëxamination of the observations of 1761 and 1769 and of the methods employed in obtaining them, it became unpleasantly certain that the numerical value of the solar parallax depended chiefly upon the computer's interpretation of the observer's record.

From the earliest observations most of the observers had been more or less baffled by a phenomenon generally known as the "black drop" or "dark ligament" which, at or about the expected times of internal contact, had appeared between the limbs of the Sun and Venus, and rendered the recorded times of the contacts uncertain to the extent of fifteen or twenty seconds.

As the "black drop" was generally seen by those who observed the transits of the eighteenth century, the interpretation of the recorded observations was the principal problem which challenged the skill of those astronomers who attempted a new discussion of the observations of 1769.

POWALKY seems to have adopted no rule for explaining the meaning of the records, rejecting such as he deemed discordant, and in 1864 he deduced a new value of $\pi = 8''.832$.

STONE, after a careful study of all the records, adopted a rigid rule for interpreting the records in all cases where the "black drop" was seen and from his discussion obtained the value $8''.91$.

He concluded that the "black drop" or some modification of it was a necessary phenomenon whenever a black disk like Venus or Mercury was seen against the bright surface of the Sun.

In 1868, Mr. STONE said:[1] "The apparent diameter of the Sun is increased, and the apparent diameter of Venus, while on the Sun's disk, is decreased from the same causes which produce the spurious disk of a star in the focus of a lens. When the true limb of Venus falls in a line with the true limb of the Sun, the light is cut off; the spurious enlargement of the Sun's disk and encroachment on that of Venus is destroyed at the point of contact, and, as a consequence, a dark ligament appears to connect the apparent limbs of the Sun and Venus. This is the real internal contact. The breadth of the connecting ligament rapidly increases, and, after some eighteen seconds, the exact interval depending upon the circumstances of the transit and the success in first catching phase (1), the limbs of Venus and the Sun will appear in contact. This is the second phase for accurate observation, the *apparent* internal contact."

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[1] Monthly Notices R. A. S., vol. XXIX, p. 47.

This quotation from Mr. STONE expresses very fairly the opinion of those who believe that the "black drop" is, from the nature of the circumstances, a natural and *necessary* phenomenon.

In 1876, M. ANDRÉ,[2] the astronomer who had charge of the French expedition to Nouméa, in 1874, to observe the transit of Venus, announced as the result of analytical investigation and of a long series of experiments made by himself, that:

"The bridge, black ligament, or black drop as it is variously called, is a necessary phenomenon under certain circumstances, and not merely accidental. With an unchanging source of light of sufficient brilliancy the angular dimensions of the ligament are inversely proportional to the diameter of the object-glass, and with an aperture of five inches the drop is scarcely perceptible. Diaphragms should never be used when observing Venus in transit, as the apparent dimensions of the drop are augmented by any increase in focal length.

"It is always possible to get rid of the ligament and reduce the phenomena to "geometrical contacts, either

(*a*) by reducing sufficiently the intensity of the source of light, or augmenting the absorbing power of the dark glass employed: or, again

(*b*) by covering the object-glass with a network diaphragm composed of rings alternately full and empty, and all very thin, but bearing a certain proportion to the focal length of the lens."

These results and opinions of M. ANDRÉ were not generally known at the time of the transit of Mercury in 1878, though ANDRÉ travelled from Paris to Ogden in Utah to test his results, from analysis and experiment, by actual observation. His theories were confirmed by his observations in 1878, and in 1881 he published his results more fully together with the details of his experiments.

In 1878 the necessity of the "black drop" phenomena was generally recognized, and it was seen by many observers of the transit of Mercury. Some who saw it evidently regarded their success in finding it, an evidence of the good quality of their observations. In a few cases observers apologized for *failing* to see the black drop.

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[2] Monthly Notices R. A. S., vol. XXXVII, p. 59.

One fact, however, was worthy of notice, that *every* observer, so far as I could ascertain, who was accustomed to observe the limbs of the Sun for position; or, in other words, to get by means of shade glasses the best definition of the Sun's limbs with an illumination somewhat *less* than the eye could easily bear,—*did not* see any trace of the black drop.

Although I had then seen no account of ANDRÉ's experiments and had given but little attention to his deductions as announced in the Royal Astronomical Society by Father Perry, I was convinced after my observation of the transit of *Mercury* in '78, that the theory of a *necessary* "black drop" was fallacious. As I had been able to make only one experiment, the data to sustain my belief, from my own observations, were very meagre and I waited four years for another opportunity. Many other observers in 1878 failed to see the obstructing phenomenon, but out of the various descriptions of the contact observations I did not find the *exact* data which I desired as proof.

The experience gained in the observations of May 6, 1878, and from the subsequent discussions of the results, were probably not without their effect in the observations of the transit of Venus in Dec., 1882, for I have yet to learn of any observer in the eight American parties organized by the Transit-of-Venus Commission who saw any trace of the "black drop;" while in 1874 many of them report such phenomena.

Last December, at the Transit-of-Venus station at Cedar Keys, Florida, the observation of *first* contact was prevented by a cloud which covered only a small portion of the Sun's disk. This cloud soon disappeared and the illumination was reduced, with the sliding shade-glass, until the brightness was considerably less than could be easily borne by the eye, while the definition of the Sun's limb was perfect. The Sun was surrounded by haze, and thin cirri which increased the steadiness and definition of the limb and permitted the use of a less dense shade-glass.

The definition and steadiness of Venus were all that could be desired, and there was no modification, at the limbs, of the dense black color of the disk.

The objective of the telescope was *five* inches in diameter, and the whole aperture was used. The eye-piece magnified 216 diameters, and was used in connection with a Herschel solar prism. The shade-glass was a wedge of neutral tint mounted against a similar wedge of plain glass in a thin brass frame, the whole sliding freely in a slotted cap for the eye-piece.

My attention was of course directed, *first*, to fixing the exact time of contact; *second*, to looking for the black drop and its modifications, or for any change in the tint of the Sun's limb at the point of contact.

The entire disk of Venus was visible for several minutes before *second* contact, and the portion beyond the Sun's disk was bordered by a narrow line of light much less *bright* than the limb of the Sun, and of a lighter tint.

The progress of the transit was strikingly barren of notable events. About one minute before the contact, the apparent motion of the cusps of the Sun as they closed around the planet was noticeably increasing, though the movement was the perfection of steadiness.

I had not long to wait, for the cusps rapidly swept around the body of the planet in a perfectly steady line of sunlight of the same tint as the adjacent parts of the Sun. This line was as narrow as I could see with a power of 216, and was free from any tremors or pulsations.

There was not the slightest agitation of the limbs of either body near the point of contact, and no *trace* of any *black drop*, *ligament* or *band*.

There was no change of color or tint on the limb of Venus near the point of contact, and no indication of any *clinging* of the limbs.

The contact was as easily and I think as accurately observed as the transit of a star within 8° of the pole under the best conditions. The uncertainty of noting the time of the visible contact could not be greater than *three-tenths* of a second.

The phenomena at the *third* contact were similar to those at the *second* in the reversed order.

From my own experience in 1882 and in 1878, as well as from the work of other observers, I am thoroughly convinced that with a properly arranged telescope and shade glasses, no good observer need have any trouble from any phase of the "black drop." It must not be forgotten that the observer of contacts has but a

single purpose in view. He has no time to devote to the study of any branch of solar physics; he is simply to obtain a good definition of the *Sun's* limb as a reference point in the passage of the limb of *Venus*. Searching for some *new thing* detracts from the value of his proper work.

If it be urged that the light should be as strong as the eye can bear, thereby producing all the troublesome phenomena that diffraction or irradiation will give at the limit of vision, why not simplify the process by discarding shade glasses and all aids to good vision, and see,—*nothing* that is trustworthy.

OBSERVATIONS DE M. J. JANSSEN, DIRECTEUR DE L'OBSERVATOIRE DE MEUDON, CHEF DE LA MISSION FRANCAISE A L'ILE CAROLINE POUR L'OBSERVATION DE L'ÉCLIPSE TOTALE DU 6 MAI, 1883.

Communicated by Prof. J. R. EASTMAN of Washington, D. C.

OBSERVATIONS SPECTROSCOPIQUES.

LE but principal des observations de l'auteur était l'étude des raies obscures de la couronne.

D'après l'avis de l'auteur la visibilité de ces raies dépend beaucoup plus du pouvoir lumineux de l'instrument que de la perfection des images.

D'après ces vues l'auteur avait fait construire un téléscope (reflecting) dont le miroir a 50 centimêtres de diamêtre et seulement 1^m 60 de foyer. Dans cet instrument les images de la lune sont d'une intensité extraordinaire et qui blesse la vue. Avec ce téléscope on a pu obtenir de bonne images de la lumière cendrée en une minute d'exposition. C'est cet instrument qui a été employé à Caroline. Le spectroscope adapté au téléscope est formé d'un prisme à vision directe contenant les prismes de matière très pure. Le collimateur a un foyer et une ouverture calculés pour que tous les rayons envoyés par le miroir soient utilisés par lui.

Un chercheur très précis est disposé sur l'instrument de manière que l'un des yeux étant au chercheur l'autre œil puisse avoir l'analyse du phénomène.

Avant la totalité complète le miroir du téléscope était couvert en partie pour éviter que les rayons solaires ne brissent le prisme réflecteur qui renvoie la lumière dans le spectroscope.

Dès que la totalité a été presque complète le miroir a été complétement découvert, et on a commencé l'exploration de la couronne.

On a reconnu d'abord les raies brillantes que la couronne présente ordinairement mais, ce qui a été nouveau et plus complet qu'on ne s'y attendait même, c'est que le fond du spectre coronal a présenté le spectre *Fraunhoférien complet;* c'est-à-dire, les raies obscures du spectre solaire moins celles de ces raies qui deviennent brillantes dans la couronne, comme C. F. etc., et celles qui par leur finesse ne sont visibles que dans les spectroscopes extrêment dispersifs. Et ce qu'il faut bien remarquer c'est que dans les circonstances d'éclairement ou on était placé, toutes les raies obscures du spectre solaire qu'il était théoriquement possible de voir ont été vues.

J'ai même été si frappé de l'évidence du phénomène que j'ai demandé à M. Trouvelot, qui observait à côté de moi de venir le voir. Mais ces occupations l'en ont empêché. Ce phénomène remarquable a été observé dans les parties les plus brillantes de la couronne, mais non d'une manière égale à même distance du limbe lunaire. Ceci indique qu'il y a dans la couronne des parties qui réfléchissent beaucoup plus abondament la lumière émanée du globe solaire, circonstance en accord avec l'existence autour de ce globe de matières cosmiques circulant autour de lui.

Nous ne développerons pas ici toutes les conséquences du fait constaté à Caroline, nous voulons surtout constater l'existence dans le spectre coronal du spectre de Fraunhofer complet, nous réservant de revenir sur les conséquences de ce phénomène, relativement à la constitution de la couronne et des espaces circumsolaires. J'ai trouvé le spectre de la couronne continu dans certaines régions basses de la couronne.

J'ai étudié aussi les anneaux de Respighi. Ces anneaux ne se sont pas montrés réguliers autour du limbe lunaire; ils ont présenté deux particularités de structure qui montrent la complexité du phénomène coronal et qui seront discutées plus tard.

La lumière de la couronne était vivement et radialement polarisée.

Toutes ces études: examen des formes de la couronne, raies obscures anneaux de Respighi, polarisation étaient associées en vue de la question des matières cosmiques circumsolaires, nous pensons que les résultats obtenus à Caroline et spécialement la constatation du spectre fraunhoférien dans les régions circumsolaires avancent cette question.

PHOTOGRAPHIE.

J'avais emporté de France deux grands appareils sur lesquels étaient montés huit chambres photographiques, dans le but d'étudier la question des planêtes intra-mercurielles et celles des formes de l'étendue de la couronne.

Vu des appareils photographique avait un objectif de 8 pouces d'ouverture qui embrassait une région de 15° tout autour du soleil. Cette appareil donne, par une belle nuit, des étoiles de 6 et 7^e grandeur en quelques minutes. Il était spécialement destiné à Caroline, à la recherche des planêtes hypothétiques intra-mercurielles. Un premier examen des plaques permet de dire que rien d'important à ce point de vue ne s'est montré autour du soleil pendant l'éclipse totale.

Pour moi la non existence de planêtes importantes intra-mercurielles me semble démontré, sinon d'une manière absolue au moins avec une probabilité extrêment considérable. Les autres appareils photographiques avaient des pouvoirs lumineux très différents et étaient destinés à résoudre plusieurs questions nouvelles sur la constitution de la couronne.

Ces appareils ont montré que la couronne est un phénomène délimité et qu'au delà d'un certain temps de pose *son image photographique n'augmente plus en étendue.*

Vue des images de la couronne a été obtenue avec un objectif de 4 pouces à court foyer et qui a agi pendant cinq minutes complète sur une plaque à la gélatine très sensible. La couronne y est nettement limitée et d'étendue sensiblement égale à celle obtenue avec des actions lumineuses beaucoup plus faibles.

Les formes de la couronne se sont montrées fixes pendant toute la durée de la totalité. En résumé mes observations à Caroline tendent à établir: qu'il n'existe point de planêtes ultra-mercurielles importantes, et que les perturbations reconnues par Leverrier dans les mouvement de Mercure, doivent plutôt être recherchés dans la présence de matières cosmiques circulant autour du soleil.

ORBIT OF THE GREAT COMET OF 1882. By EDGAR FRISBY of Washington, D. C.

[ABSTRACT.]

THIS is a partial record of observations at Washington. Mr. Winlock of the U. S. Naval Observatory is preparing a description of all the physical phenomena of the comet which were there observed. The first observation of the comet made at Washington was with the 9.6 inch Equatorial at about two o'clock on the afternoon of September 19, comparisons being made with the sun for the position of the comet. Good meridian observations were obtained on September 20 and 21, and from the equatorial observation on the 19th and the two meridian observations of the 20th and 21st, calculations were made which served to fix the place of the comet with fair approximate accuracy for three months, which was a somewhat remarkable success. After the fifth of October a difficulty occurred in obtaining accurate observations, on account of several different points of light being presented in an ill-defined nucleus, and it was uncertain whether the observations always referred to the same luminous point, the nucleus sometimes extending over as much as four seconds of time in right ascension and as much as three or four tenths of a minute in declination.

The following elements were computed from observations made at the U. S. Naval Observatory on September 19, October 8 and November 24, astronomical time, the first and last being made on the meridian and the middle one on the equatorial.

Perihelion Time				Sept. $17.^{\mathrm{d}}2282$		Wash. M. T.
$\Omega =$	346°	1′	7.91″	Φ		89° 13′ 42.70″
$\pi - \Omega =$	69	36	12.79	log. a	$=$	1.9331366
$i =$	141	59	52.16	log. q	$=$	7.8904739
				period	$=$	793.689 years.

The elements are referred to the mean equinox of 1883.0.

The first accurate observation of this comet of which we have any record was made by Mr. Finlay at the Cape of Good Hope on September 8. Among the last were observations made at the Kempshot observatory, Jamaica, April 26, 27, May 5 and 6 by Mr. Maxwell Hall. The elements compared with the Cape of Good Hope observation give a correction of only half a second of time in right ascension and about one second in declination; compared with the Jamaica observations the corrections vary from 0 to 3^s in right ascension and from 0.′1 to 1.′8 in declination. Comparisons were also made with other observations which gave a correction sometimes as much as 3^s or 4^s in right ascension and about 0.′3 in declination; these variations probably being partly accounted for from observing different parts of the nucleus.

The elements were also compared with the computations of other astronomers. The most prominent variation was in respect to the period which others gave as 659, 654, 823, 852 and 997 years. The last period was that computed by Dr. Morrison of the Nautical Almanac Office at Washington.

The difference between 793 and 997 years seems considerable, but the elements agree very closely, the inclination being exactly the same as that here given; the position of the node, and the distance from the perihelion to the node differing only by about 0.′3. The period of 853 years was computed by Dr. Kreutz, that of 823 by Fabritius. A pasteboard model constructed by Mr. Winlock was exhibited, showing the relative positions in space of the earth, sun and comet.

OBSERVATIONS UPON URANUS MADE IN 1883, AT PRINCETON, N. J.

By C. A. YOUNG of Princeton, N. J.

[ABSTRACT.]

DURING May and June, 1883, a series of micrometric measurements upon the planet's disk was made, for the purpose of determining its ellipticity; the time being especially favorable because of the planet's nearness to the node of the satellite orbits The instrument used was the 23 inch equatorial of the Halste Observatory, with the filar micrometer: powers of 480 and 720.

For the polar diameter, reduced to the mean distance 19.1826, we obtained from 140 readings on eight different evenings, 3″.974.

For the equatorial diameter, ... 4″.280, from the same number of readings on the same evenings, May 18 to June 19 inclusive.

The ellipticity of the disk deduced is about $\frac{1}{14}$.

Whenever the seeing was good, faint markings were always visible on the disk of the planet.

They appeared to be dark *belts*, resembling somewhat those of Jupiter. Usually there were two of them, one on each side of the equator, their general trend being commonly in the direction of the major axis of the satellite orbits, though on one or two occasions they seemed to be inclined *sinistrorsum* to this line by some 10° or 20°. Nothing was made out which could give a satisfactory determination of the planet's axial rotation. Once or twice a difference was noted between the brightness of the two polar regions, the western being brighter than the eastern; and on one evening one of the observers (C. A. Y.) suspected a sort of spur running out from the polar edge of the western belt: but the object was very faint, and the appearance never recurred.

The observations were made by Professor Young and Mr. McNeill.

PHYSICAL PHENOMENA ON THE PLANET JUPITER. By G. W. HOUGH of Chicago, Ill.

The rapid motion of revolution of the planet, by changing the positions of the markings on the surface to our line of sight, makes great apparent differences in their shapes and sizes. This has perhaps been the occasion of reports of sudden and great changes upon the surface. The changes are not sudden but are gradual; and many of the features are permanent. Minor changes are constantly in progress in the equatorial belts. The author recently observed the belt drifting down toward the red spot; but although it partly surrounded it, they did not coalesce, and the spot forced a scallop into the belt,—a very curious phenomenon. The author saw a satellite pass over the red spot, though the satellites are not visible when on the white part of the disk. He had also had a chance to compare shadows of satellites on the disk and on the spot, and both are dark. The red spot has seemingly retrograded during the past four years; that is to say, the rotation of Jupiter has seemingly increased from 9 h. 55 m. 33 s., to 9 h. 55 m. 38 s. The future observer should attend more carefully to what he sees, and theorize afterward.

ON A METHOD OF OBSERVING THE ECLIPSES OF JUPITER'S SATELLITES, EMPLOYED AT THE LAWRENCE OBSERVATORY OF AMHERST COLLEGE, BEARING ON THE DETERMINATION OF CERTAIN CORRECTIONS PROPER TO BE APPLIED TO THE EARLY OBSERVATIONS OF THESE ECLIPSES. By DAVID P. TODD of Amherst, Mass.

[ABSTRACT.]

AMONG the more important corrections which have to be applied to the early observations of the eclipses of Jupiter's satellites to render them homogeneous and comparable, is the correction for aperture of the telescopes used in making the observations. With reference to throwing some light on this question, a method has been devised whereby any given eclipse may be repeatedly observed at either phase by a single observer with varying apertures of his telescope. This method was brought into use at the Lawrence Observatory early in the present year, and the results so far obtained indicate the desirability of continuing the work in the future.

OBSERVATIONS ON THE TRANSIT OF VENUS MADE AT COLUMBIA COLLEGE OBSERVATORY. By J. K. REES of New York, N. Y.

INVESTIGATION OF THE LIGHT-VARIATIONS OF SAWYER'S VARIABLE. By S. C. CHANDLER, JR., of Cambridge, Mass.

[ABSTRACT.]

THIS paper contains a discussion of the elements and light curve of this variable of the Algol type, from all the available material. The period is shown to be at present 20 h., 7 m., 42.0 s., and the presence of an irregularity in the period is demonstrated by observations at Cordoba and Copenhagen in 1871 and 1863 respectively. Anomalies in the light curve are also shown very probably to exist.

RESULTS OF TESTS WITH THE ALMUCANTAR, IN TIME AND LATITUDE.

By S. C. CHANDLER, JR., of Cambridge, Mass.

[ABSTRACT.]

THE instrument which has been named the "almucantar" was described and figured in a paper presented to the association at its meeting in 1880. In its general nature it is an equal altitude instrument. A hollow rectangular trough containing mercury revolves horizontally on an upright central pillar. The trough contains a float which is perfectly free to obtain equilibrium, while it is constrained to revolve with the trough. The float carries a telescope which turns on a horizontal axis, and can be clamped at any desired altitude. When this instrument is revolved on its vertical axis, any given point in the field of view describes a horizontal small circle, or almucantar, in the heavens. The transits of stars over a series of horizontal lines will thus afford means of determining the altitude of the instrument, the error of the clock, the latitude or the declinations of the stars, by a proper distribution of the observations in azimuth.

A higher degree of accuracy is attainable by this instrument than by a transit or a zenith telescope of the same size. The author's comparison of results is as follows: the probable error of a single star in determining the clock error is only ± 0.05^s or ± 0.06^s. With a transit instrument of the same size, the quantity is not less than ± 0.08^s. With the almucantar the probable error in determining the latitude from a single star is ± 0.55$''$, including the error of the star's place. This is about equal to the probable error of a pair of stars by Talcott's method, with the larger telescopes of the United States coast-survey.

The instrument was a small one, — 1$\frac{3}{4}$ inches aperture and 25 inches focus. It was constructed for experiment only, in a provisional way, at a cost of $150. There are obvious defects in design and construction: when these are remedied, the errors can be much reduced.

A series of observations with this instrument are given by the author, for the latitude of a pier about 80 feet north of the Harvard-college observatory. The value obtained by averaging these is 0.7$''$ less than given by Professor Peirce in his discussion of the prime vertical transit observations taken by the Messrs. Bond, and

adopted as the standard value of the latitude of the observatory, allowance of course being made for differences between piers. The author concludes that Professor Peirce's value is too large by fully three-quarters of a second. By way of proof the author gives a series of observations on the five stars used by Professor Peirce. These are compared with those of Auwers and Boss, and the correction of the hitherto accepted value of the latitude now indicated by the almucantar is thereby confirmed.

The clock errors of two nights selected at random, as given by the almucantar, were exhibited by the author. The results both in time and latitude would be considered satisfactory with an ordinary instrument of two or three times the size. The almucantar can be made much larger than the one under trial, certainly of five or six inches aperture, with corresponding increase of precision along with greater optical power. Its mechanical construction is simple, and reduces the sources of error. Thus in the older instruments there are involved: 1. The accurate construction of parts as of pivots, level, graduated circles. 2. Fixity of mounting, to avoid a shifting of the instrumental plane. 3. Rigidity of the instrument itself, to secure constancy of collimation and flexure. In the almucantar only the last condition has to be satisfied, and it is by far the easiest of the three to be attained mechanically.

The author regards the principle of flotation adopted as being as delicate an indication of the direction of gravity as is obtained by the spirit-level.

The almucantar gives promise of a new instrumental resource in the higher practical astronomy. It is competent to deal with the most delicate problems. It will evade some of the minute sources of errors that still cling to meridian instruments. Especially, it furnishes a method for obviating difficulties, hitherto regarded as almost insuperable, connected with flexure and refraction, in observations with the meridian circle.

A STANDARD TIME-POINTER AND A TIME LONGITUDE DIAL. By SAMUEL EMERSON of New York, N. Y.

A SYSTEM OF ALGEBRAIC GEOMETRY. By SAMUEL EMERSON of New York, N. Y.

AN IMPROVED METHOD OF PRODUCING A DARK-FIELD ILLUMINATIO OF LINES RULED UPON GLASS. By W. A. ROGERS of Cambridge, Mass.

A NEW METHOD OF INVESTIGATING THE FLEXURE CORRECTIONS OF A MERIDIAN CIRCLE. By W. A. ROGERS of Cambridge, Mass.

THE ROTATION OF DOMES. By G. W. HOUGH of Chicago, Ill.

[ABSTRACT.]

OBSERVATORY domes are in general very heavy. As they grow old, owing to the settling of walls and other changes, they are apt to become almost unmanageable. The dome at Chicago is very weighty, everything about the observatory being built in a very substantial manner. When Professor Hough first tried to move the dome, he found its two sides working with unequal friction; and this was afterward remedied to some extent, but by no means fully. About two months ago a gas-engine was placed in position to revolve the dome. It was a great satisfaction to see the great dome go round continuously, without hitches. The cost of moving the dome by such means is a mere trifle, aside from the first cost of the engine. The use of water power where that was easily accessible must, however, be preferred in many instances where a sufficient head is supplied by street mains.

Dr. C. A. Young said, in discussing the foregoing, that when he came to Princeton he found a very heavy dome there. One man, using thirty pounds pressure on a two-foot crank, was very tired after giving the dome one turn. A gas-engine has since been put in below, and the power is communicated by a belt. A revolution can be made in four minutes, and the shutter raised in two. In general, the dome is placed and the shutter opened within five minutes. Dr. Young expressed a hope that the Brush storage batteries would furnish electrical illumination and power for the work of observatories, as the electricity might be stored even from a gas-engine operating a dynamo during hours of the day when there was no other use for its power. At present the direct action of a gas-engine on a dynamo, with no intervention between the dynamo and the light, was too irregular to serve the purpose.

DESCRIPTION OF THE NEW OBSERVATORY OF COLUMBIA COLLEGE, NEW YORK CITY. By J. K. REES of New York, N. Y.

[ABSTRACT.]

DESCRIPTION of the observatory as built on iron girders 110 ft. from the ground. Account of the paper dome and the paper transit slit-shutters opening by springs.

OBSERVATIONS ON THE TRANSIT OF VENUS MADE AT COLUMBIA COLLEGE OBSERVATORY. By J. K. REES of New York, N. Y.

THE LIGHT VARIATIONS OF T. MONOCEROTIS. By E. F. SAWYER of Cambridgeport, Mass.

THE CALCULUS OF DIRECTION AND POSITION.[1] By E. W. HYDE of Cincinnati, Ohio.

[ABSTRACT.]

THE paper consists of a comparison of Hamilton's Quaternions with Grassmann's "Ausdehnungslehre," with the design of showing the superior advantages of the latter as a practical instrument for geometrical and mechanical purposes.

First is given a brief sketch of the basis of Quaternions with a statement of its peculiarity in giving to a directed quantity, or vector, in addition to its inherent qualities of magnitude and direction, the *rotative* property possessed by $\sqrt{-1}$

Then a sketch is given of Grassmann's system of geometrical multiplication of points, lines and planes showing its completeness, clearness and adaptability to the uses to which it is to be put, as well as the advantages which, in the opinion of the writer, it possesses over Hamilton's system.

In conclusion the solutions of several problems by the two methods are given in parallel columns to illustrate the relative brevity and conciseness of Grassmann's method, and the condition of static equilibrium is derived as an illustration of the suitableness of the same for the purposes of mechanics.

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[1] Published in full in the American Journal of Mathematics. No. I, vol. vi.

TIDE METER, FOR USE ON SOUNDINGS AT A DISTANCE FROM THE SHORE. By JOHN M. BATCHELDER of Cambridge, Mass.

[ABSTRACT.]

AN instrument for ascertaining the rise and fall of the tide, from a vessel at anchor remote from the land, was constructed by order of Prof. A. D. Bache, in 1858, and has been used for this purpose by the U. S. Coast Survey at various places.

It consists of an india-rubber bag filled with air, its capacity being four or five gallons: an india-rubber tube of about one quarter inch internal diameter and a manometer or pressure gauge. The bag is contained in a weighted basket or case, which rests upon the bottom, and the india-rubber tube leads from this through the hawse hole or other opening to the manometer, which is portable and may be placed in any convenient position on board ship.

The instrument is graduated in feet and fathoms, each forty-four one-hundredths of a pound pressure per inch being equal to one foot in depth.

The longest continued series of observations by the pressure tide gauge were made in 1862 and 1863 upon the "Martin's Industry" lightship off Hilton Head, eleven miles from land, in about eight fathoms of water.

The profiles, made from the records of the Coast Survey are of very symmetrical character, in this respect comparing favorably with records of the staff-gauge kept at the same time at Station Creek, thirteen miles distant from the lightship.

With this instrument on board, vessels waiting to cross a bar at the mouth of a harbor may know the exact time of high water.

CONSTRUCTION OF CERTAIN PROBLEMS RELATING TO THE GENERAL ELLIPSOID, BY THE METHOD OF DESCRIPTIVE GEOMETRY. By C. M. WOODWARD of Washington University, St. Louis, Mo.

[ABSTRACT.]

A lemma was explained, which gave methods for readily constructing lines represented by $\frac{x}{a}$, $\frac{x}{a^2}$, $\frac{x}{a^3}$, etc., when x and a are known lines, and it was then shown that for every point x, y, z, on the surface of an ellipsoid, there is a corresponding point $\frac{x}{a}$, $\frac{y}{b}$, $\frac{z}{c}$, on the surface of a unit sphere, either of which being assumed the other can be easily found.

From the equations of the perpendicular to a tangent plane the projections of the perpendicular are readily constructed. Hence follows the solution of the problem: *To pass a plane tangent to a general ellipsoid at a given point.*

By means of the expression for the length of a perpendicular to the polar plane of any point the plane is constructed, both when the point is at a finite, and at an infinite distance. Thus are solved problems relating to *lines of shade for diverging and parallel rays of light.*

Finally, by the use of similar methods is solved the problem: *To pass a plane through a given line and tangent to an ellipsoid.*

DESCRIPTIVE-GEOMETRICAL TREATMENT OF SURFACES OF THE SECOND DEGREE. By J. BURKITT WEBB of Ithaca, N. Y.

[ABSTRACT.]

FOR the purpose of greater conciseness the speaker confined his remarks to the general ellipsoid, remarking that the usual treatment of problems upon this surface — as, for instance, such problems as finding the shade and shadow, or drawing tangent planes — is lacking in generality; the body being taken in such special position, or referred to such special axes, as reduce the general problem to a specially simple one.

The speaker then drew the projections of three conjugate diameters of a general ellipsoid upon the board, stating that this was the best method of defining that body. He then proceeded to define the projections of the enveloping cylinders, and the shadow of the body, which he showed could as easily be done for the general ellipsoid in a perfectly general position, as for special cases. In fact, it appeared that problems on this body gained nothing in simplicity by special methods and devices which detract from the generality of the treatment.

CONIC SECTIONS IN DESCRIPTIVE GEOMETRY. By J. BURKITT WEBB of Ithaca, N. Y.

SOME HITHERTO UNDEVELOPED PROPERTIES OF SQUARES. By O. S. WESTCOTT of Chicago, Ill.

METHODS of obtaining the square roots of numbers[1] may be classed as ordinary, contracted and intuitive.

The ordinary method founded upon the properties of the square of an algebraic binomial needs no remark.

The contracted method was published in a little book written by Samuel Emerson in 1865. It may be indicated thus:

39' 06' 25(625
3
62.

Of necessity, this operation, like the other, depends upon the properties of the trinomial square just mentioned, the only difference in the method being with reference to the point of attack. Thus, in the above operation, 39' 06' 25(6
3
bearing in mind that $(a + b)^2 = a^2 + 2ab + b^2$, after subtracting 6^2 from 39 leaving a remainder 3, take $2a = 12$, for a permanent trial divisor.[2] Then $b = 2$. Now, instead of finding a permanent divisor by noting the possibility of factoring $2ab + b^2$ as $b\ (2a + b)$ and obtaining the value of $2a + b$, remove at once from the power the value of b^2 and subsequently that of $2ab$. The operation will then stand: 39' 06' 25(62
3
62 the mental operation being $b^2 = 2^2 = 4$. $6 - 4 = 2$. $2ab = 2 \cdot 6 \cdot 2 = 24$.[3] $30 - 24 = 6$. A line through the 3 may indicate its removal, and the remainder may be written in the line above if the operator so desires.

The operation then proceeds by determining with our old divisor 12 that the next figure of the root is 5. Then we have:

39' 06' 25(625
3
62 as the completed operation.

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[1] Unless otherwise indicated, the word number throughout this discussion signifies integer or integral number.

[2] In the ordinary operation for the extraction of the square root every observer must have noticed that the left hand figures of the several trial divisors rarely change.

[3] Locality rather than local value of 6 being kept in mind without its necessarily being formulated in thought as 60.

The thought, however, is something more. It is as follows: $5^2=25$. $25-25=0$. Since $2\cdot 5\cdot 62$ is obviously $(2\cdot 5\cdot 2)+(2\cdot 5\cdot 60)$ (see note 2) the operation then proceeds $2\cdot 2\cdot 5=20$. $20+2=22$. $22-22=0$. $2\cdot 5\cdot 6=60$. $60+2=62$. $62+62=0$. No originality is claimed for this operation, but by its elucidation, I hope to have called attention to its great practical utility.

I come now to what I have ventured to call the intuitive method. In more than a score of years of school routine in mathematics, I have found it time-saving and advantageous in other respects, to develop a plan of determining at sight, the square root of any perfect square within moderate limits. Expedition in obtaining results, particularly in the elementary matter of quadratic equations is so desirable, that an independent discussion of squares is believed to be to a thoughtful teacher a great desideratum.

$$
\begin{aligned}
(a+b)^2 &= a^2+2ab+b^2.\\
(a-b)^2 &= a^2-2ab+b^2.\\
\therefore\ (a+b)^2-(a-b)^2 &= 4ab,\\
\text{and } (a+b)^2 &= 4ab+(a-b)^2.
\end{aligned}
$$

$$
\begin{aligned}
&\text{Make } a = 25.\\
&\text{And } b = 1, 2, 3, \text{ etc.}\\
&\text{Then } 26^2 = 100 + 24^2.\\
&\qquad\ \ 27^2 = 200 + 23^2.\\
&\qquad\ \ 28^2 = 300 + 22^2, \text{ etc., etc.}
\end{aligned}
$$

Evidently, then, the tens and units figures of all perfect squares of numbers from 26 to 49, inclusive, are the same as the tens and units figures of perfect squares of numbers from 24 to 1 inclusive.

Any one may now easily form the following table:

$$
\begin{aligned}
576 &= 24^2 \therefore 576 + 100 = 26^2 = 676.\\
529 &= 23^2 \therefore 529 + 200 = 27^2 = 729.\\
484 &= 22^2 \therefore 484 + 300 = 28^2 = 784.\\
441 &= 21^2 \therefore 441 + 400 = 29^2 = 841.\\
400 &= 20^2 \therefore 400 + 500 = 30^2 = 900.\\
361 &= 19^2 \therefore 361 + 600 = 31^2 = 961.\\
324 &= 18^2 \therefore 324 + 700 = 32^2 = 1024.\\
289 &= 17^2 \therefore 289 + 800 = 33^2 = 1089.\\
256 &= 16^2 \therefore 256 + 900 = 34^2 = 1156.\\
225 &= 15^2 \therefore 225 + 1000 = 35^2 = 1225.
\end{aligned}
$$

$$\begin{aligned}
196 &= 14^2 \therefore 196 + 1100 = 36^2 = 1296.\\
169 &= 13^2 \therefore 169 + 1200 = 37^2 = 1369.\\
144 &= 12^2 \therefore 144 + 1300 = 38^2 = 1444.\\
121 &= 11^2 \therefore 121 + 1400 = 39^2 = 1521.\\
100 &= 10^2 \therefore 100 + 1500 = 40^2 = 1600.\\
81 &= 9^2 \therefore 81 + 1600 = 41^2 = 1681.\\
64 &= 8^2 \therefore 64 + 1700 = 42^2 = 1764.\\
49 &= 7^2 \therefore 49 + 1800 = 43^2 = 1849.\\
36 &= 6^2 \therefore 36 + 1900 = 44^2 = 1936.\\
25 &= 5^2 \therefore 25 + 2000 = 45^2 = 2025.\\
16 &= 4^2 \therefore 16 + 2100 = 46^2 = 2116.\\
9 &= 3^2 \therefore 9 + 2200 = 47^2 = 2209.\\
4 &= 2^2 \therefore 4 + 2300 = 48^2 = 2304.\\
1 &= 1^2 \therefore 1 + 2400 = 49^2 = 2401.
\end{aligned}$$

Hence, to determine the square of any number between 25 and 50, one has merely to find the corresponding number below 25 and augment *its* square by the number of hundreds indicated by its remoteness from 25. Thus 43 being 18 more than 25, and 7 being 18 less, the square of 43 will be $1800 + 7^2 = 1849$.

Again, since $a - b = 2a - (a + b)$, we may more conveniently obtain results by the following rule: To square any number from 25 to 50, *take the excess above 25 as hundreds, and augment by the square of what the number lacks of 50.*

Thus, $43^2 = (43 - 25)\cdot 100 + (50 - 43)^2 = 1849$, as above.

$47^2 = (47 - 25)\cdot 100 + 3^2 = 2209$, etc., etc.

Conversely: required, the square root of 1764. The root is plainly in the fourth decade, that is, it is between 25 and 50. The tens and units figures indicate 8. Therefore, the square root of $1764 = 50 - 8 = 42$.

Again, $\sqrt{2116} = 50 - 4 = 46$, and $\sqrt{1444} = 50 - 12 = 38$, etc., etc.

Make $a = 50$.
And $b = 1, 2, 3$, etc.
Then $51^2 = 200 + 49^2$.
$52^2 = 400 + 48^2$.
$53^2 = 600 + 47^2$,
etc., etc.

It is here further observable that the tens and units figures of perfect squares of numbers from 51 to 99 inclusive are the same as the tens and units figures of the squares of numbers from 49 to 1 inclusive. Now, since four times *any numbers of hundreds plus* 25, 50 or 75, gives an exact number of hundreds, it follows that the tens and units figures of the squares of numbers less than 25, represent all the possible combinations of figures in those orders of units, for *all square numbers.*

Now, as these squares give only 22 different combinations, viz.: 00, 01, 04, 09, 16, 21, 24, 25, 29, 36, 41, 44, 49, 56, 61, 64, 69, 76, 81, 84, 89, 96, it is evident that familiarity with these terminations is eminently desirable.

Remembering again, that $a - b = 2a - (a + b)$, we may formulate the rule: To square any number from 50 to 100, *take twice the excess above* 50 *as hundreds and augment by the square of what the number lacks of* 100.

Thus, $89^2 = 200\,(89 - 50) + (100 - 89)^2 = 7921$. And $93^2 = 8649$, etc.[4]

Conversely: required the square root of 3249. The root is plainly between 50 and 60. The tens and units figures indicate 7. Hence $\sqrt{3249} = 50 + 7 = 57$. And $\sqrt{3721} = 50 + 11 = 61$, etc., etc.

Note, however, that the square root of 7921 is $50 + 39$ or $100 - 11$. Now it is obviously better to make use of the latter combination. Hence, in accordance with principles already developed, if the root is in the fourth quarter of the hundred, subtract the number indicated by the tens and units figures from 100, and the difference is the required root. Thus $\sqrt{8281} = 100 - 9 = 91$, $\sqrt{9216} = 100 - 4 = 96$, $\sqrt{7569} = 100 - 13 = 87$, etc., etc.

From what we have learned already, the following are convenient and useful inferences:

To square any number from 100 to 200, *take four times the excess above* 100 *as hundreds, and augment by the square of what the number lacks of* 200. Thus $186^2 = 86 \cdot 4 \cdot 100 + 14^2 = 34596$, $178^2 = 78 \cdot 4 \cdot 100 + 22^2 = 31684$, etc., etc.

Since four times 125 is one-half of one thousand, to square any number from 125 to 250, *take one-half the excess above* 125 *as*

[4] It is well not to forget that in the case of many numbers, another plan is still more expeditious. I refer to the common scheme developed from the formula $(a + b) \cdot (a - b) = a^2 - b^2$. By transposition, this becomes $a^2 = (a + b) \cdot (a - b) + b^2$, and $93^2 = (93 + 7)(93 - 7) + 7^2 = 8649$ as before. But this plan, though commonly enough used and indispensable in its place, is not sufficiently broad for our present discussion.

thousands, and augment by the square of what the number lacks of 250. Thus $239^2 = \frac{1}{2}(239 - 125)\cdot 1000 + 11^2 = 57121$, $246^2 = \frac{1}{2}(246 - 125)\cdot 1000 + 4^2 = 60516$, etc., etc.

Also, to square any number from 250 to 500, *take the excess above 250 as thousands, and augment by the square of what the number lacks of* 500. Thus, $479^2 = (479 - 250)\ 1000 + 21^2 = 229441$, $467^2 = (467 - 250)\cdot 1000 + 33^2 = 218089$. It is obvious that these rules are the most useful in the vicinity of the upper limit of their scope, but when one has once rendered himself familiar with the squares of small numbers (say to 100) the ease with which succeeding steps may be taken, as indicated in the last examples, renders the more common methods almost ridiculous.

It is obvious, also, that the principle involved in these processes is universal, and that rules constructed on this plan may be multiplied *ad infinitum.* Thus, to square any number from 500 to 1000, *take twice the excess above 500 as thousands, and augment by the square of what the number lacks of* 1000, *e. g.:* $987^2 = 2\cdot(487)\cdot 1000 + 13^2 = 974169$.

It need not, however, be forgotten that here as in many other cases, there is a choice of methods, 987^2 being also $(987 - 13)\ 1000 + 13^2 = 974169$. This, as will be observed, is only an application of the other formula, briefly discussed above, viz.: $a^2 = (a + b)\ (a - b) + b^2$. The utility of this formula is too often kept out of the sight of learners, until they themselves, mayhap, stumble upon it. Observe, *e. g.:*

$$
\begin{aligned}
19^2 &= 20\cdot 18 + 1.\\
28^2 &= 30\cdot 26 + 4.\\
59^2 &= 60\cdot 58 + 1.\\
97^2 &= 100\cdot 94 + 9.\\
107^2 &= 100\cdot 114 + 49.\\
983^2 &= 966\cdot 1000 + 17^2 = 966289.\\
1031^2 &= 1062\cdot 1000 + 31^2 = 1062961, \text{etc., etc., etc.}
\end{aligned}
$$

Incidentally, this process should be made perfectly familiar. Let us now drop this branch of the subject and return to the discussion of the converse operation of evolution, ascertaining, if possible, how the principles and methods developed with numbers whose roots are less than 100, may be applied to numbers indefinitely larger.

Since the squares of numbers from 1 to 25 end in a series of numbers in the tens and units places, which are repeated in regular, but inverse order in the squares of numbers from 25 to 50, again, directly, in the squares of numbers from 50 to 75, and again, inversely, in the squares of numbers from 75 to 100, and so on, it is evident that what we have already proved within the limit of 100 holds good as we advance.[5]

Now, as we have already seen $\sqrt{7396} = 100 - 14 = 86$.

Similarly. $\sqrt{18769}$ is plainly between 130 and 140, *i. e.*, it is in the second quarter of the hundred. Since 69 indicates 13, the root is evidently $150 - 13 = 137$. $\sqrt{24964}$ is between 150 and 160, *i. e.*, in the third quarter of the hundred. Since 64 indicates 8, $\sqrt{24964}$ is, therefore, $150 + 8 = 158$.

In general, to read the square root of a square number, first, observe the last two figures of the power. These figures always indicate a root which is at once evident to any one who will familiarize himself with the squares of the numbers from 1 to 25.

Next, determine in which quarter of which hundred the root lies. If the root is in the first or third quarter, *add* the number indicated by the tens and units figures to the hundreds in the former case, and to the hundreds plus fifty in the latter case. If the root is in the second or fourth quarter, *subtract* the indicated number from the hundreds plus 50 in the former case and from the next greater number of hundreds in the latter case.

Thus $\sqrt{393129}$ is in the second quarter of the sixth hundred. Twenty-nine indicates 23, since $23^2 = 529$. Therefore $\sqrt{393129} = 650 - 23 = 627$. Again $\overline{580644}$ is in the third quarter of the seventh hundred. 44 indicates 12. Therefore $\overline{580644} = 750 + 12 = 762$.

The only possible difficulty remaining is to determine to which quarter of the known hundred the root of the specified power belongs. Since 24^2 and 26^2 both give 76 as tens and units figures, it is at first perhaps doubtful whether the square root of 104976 is $300 + 24$, or $350 - 24 = 326$. To obviate this possible difficulty, familiarity with the following table may first be acquired.

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[5] See remark above as to four times *any number of hundreds plus* 25, 50 *or* 75.

$$\begin{aligned}
\tfrac{1}{16} &= .0625.\\
\tfrac{1}{8} = \tfrac{2}{16} &= .125.\\
\tfrac{3}{16} &= .1875.\\
\tfrac{1}{4} = \tfrac{4}{16} &= .25.\\
\tfrac{5}{16} &= .3125.\\
\tfrac{3}{8} = \tfrac{6}{16} &= .375.\\
\tfrac{7}{16} &= .4375.\\
\tfrac{1}{2} = \tfrac{8}{16} &= .5.\\
\tfrac{9}{16} &= .5625.\\
\tfrac{5}{8} = \tfrac{10}{16} &= .625.\\
\tfrac{11}{16} &= .6875.\\
\tfrac{3}{4} = \tfrac{12}{16} &= .75.\\
\tfrac{13}{16} &= .8125.\\
\tfrac{7}{8} = \tfrac{14}{16} &= .875.\\
\tfrac{15}{16} &= .9375.
\end{aligned}$$

The student is here requested to observe that he may readily fall into the desirable habit of inserting or omitting decimal points at will, if thereby he can expedite his mental mathematical operations. He must also keep in mind the elementary method of squaring such binomials as $5 + \frac{1}{2}$. From an algebraic standpoint, this is obviously $5^2 + 5 \cdot \frac{1}{2} \cdot 2 + \frac{1}{4} = 6 \cdot 5 + \frac{1}{4} = 30\frac{1}{4}$. Also,

$$\begin{aligned}
(8 + \tfrac{1}{2})^2 &= 8 \cdot 8 + 8 + \tfrac{1}{4},\\
&= 9 \cdot 8 + \tfrac{1}{4},\\
&= 72\tfrac{1}{4},
\end{aligned}$$

$$\text{and } 12.5^2 = 12 \cdot 13 + .25 = 156.25$$[5]

$$\text{or } 125^2 = 120 \cdot 130 + 25 = 15625, \text{ etc., etc.}$$

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[5] I am reminded by this discussion of having once published an application of these principles combined, which gives at once $5^{16} = 152587890625$.

For $5^2 = 25$.
And $5^4 = 25^2 = 625$.
" $5^8 = 625^2 = 390625$.

For 625 by a point placed after the 6 becomes $6\frac{1}{4}$. Now $(6\frac{1}{4})^2 = 6 \cdot 6 + 3 + (\frac{1}{4})^2 = 39$ and 1-16th.

Writing the sixteenth decimally and removing the decimal point, we have 390625.

But $5^{16} = 390625^2 = ?$.

Restore the decimal point and $(39\ 1\text{-}16)^2 = 39^2 + 4\frac{7}{8} + (1\text{-}16)^2$. 39^2 (see above) $= 38 \cdot 40 + 1 = 1521$. $1521 + 4\frac{7}{8} = 1525\frac{7}{8}$. The square of 1-16 (.0625) if written decimally, must have the same significant figures as 625^2 just obtained, preceded by two zeros to give the correct decimal place and relation. Recollecting then that there must be eight decimal places following the 1525 and that the decimal for $\frac{7}{8}$ (.875) occupies three places, while the square of 625 must occupy the last six places, we may combine our thoughts and write without the decimal point $390625^2 = 152587890625$, which is the sixteenth power of 5 obtained mentally and by a process not extraordinarily difficult.

We may now construct mentally a table somewhat like the following:

$125^2 = 15625$		$(1\frac{1}{4})^2 = 1\ 9\text{-}16.$
$175^2 = 30625$		$(1\frac{3}{4})^2 = 3\ 1\text{-}16.$
$225^2 = 50625$		$(2\frac{1}{4})^2 = 5\ 1\text{-}16.$
$275^2 = 75625$		$(2\frac{3}{4})^2 = 7\ 9\text{-}16.$
$325^2 = 105625$	or	$(3\frac{1}{4})^2 = 10\ 9\text{-}16.$
$375^2 = 140625$		$(3\frac{3}{4})^2 = 14\ 1\text{-}16.$
$425^2 = 180625$		$(4\frac{1}{4})^2 = 18\ 1\text{-}16.$
$475^2 = 225625$		$(4\frac{3}{4})^2 = 22\ 9\text{-}16.$
$525^2 = 275625$		$(5\frac{1}{4})^2 = 27\ 9\text{-}16.$
$575^2 = 330625$		$(5\frac{3}{4})^2 = 33\ 1\text{-}16.$

One may readily extend this table indefinitely by remembering that $(n + \frac{1}{4})^2 = n^2 + \frac{1}{2}n + \frac{1}{16}$; $(n + \frac{1}{2})^2 = n(n + 1) + \frac{1}{4}$; and $(n + \frac{3}{4})^2 = n(n + 1) + \frac{1}{2}n + \frac{9}{16}$.

It is unnecessary to discuss the suggested law of increase of squares of numbers varying by $\frac{1}{2}$ or by $\frac{1}{2}$ of 100 as here indicated, but the mnemotechnist will be sure to note that in these tables the square has the $\frac{1}{16}$ or 0625 as terminal figures for the $\frac{3}{4}$ point of the *odd* hundreds, and the $\frac{1}{4}$ point of the *even* hundreds, while the square has the $\frac{9}{16}$ or 5625 as terminal figures for the $\frac{1}{4}$ point of the *odd* hundreds, and the $\frac{3}{4}$ point of the *even* hundreds. The reason is apparent.

Another advantage of this discussion will occur to arithmeticians when in doubt as to whether a certain number is a perfect square. For example, the number 316471 cannot be a square for 71 is not in our list of the 22 and only terminations of squares. The number 172276 may or may not be a perfect square. A glance shows that if a square, the root is 424. But $424 = 400 \cdot 448 + 24^2$ which is plainly greater than the number given. Hence, 172276 is not a square number. If the doubt in any case is not so readily dissipated, the contracted method may profitably be brought into requisition.

SECTION B.

PHYSICS.

ADDRESS

BY

PROFESSOR H. A. ROWLAND,

VICE PRESIDENT, SECTION B.

A PLEA FOR PURE SCIENCE.[1]

THE question is sometimes asked us as to the time of year we like the best. To my mind, the spring is the most delightful; for Nature then recovers from the apathy of winter, and stirs herself to renewed life. The leaves grow, and the buds open, with a suggestion of vigor delightful to behold; and we revel in this ever-renewed life. But this cannot always last. The leaves reach their limit; the buds open to the full and pass away. Then we begin to ask ourselves whether all this display has been in vain, or whether it has led to a bountiful harvest.

So this magnificent country of ours has rivalled the vigor of spring in its growth. Forests have been levelled, and cities built and a large and powerful nation has been created on the face of the earth. We are proud of our advancement. We are proud of such cities as this, founded in a day upon a spot over which but a few years since, the red man hunted the buffalo. But we must remember that this is only the spring of our country. Our glance must not be backward; for, however beautiful leaves and blossoms are, and however marvellous their rapid increase, they are but leaves and blossoms after all. Rather should we look forward to discover what will be the outcome of all this and what the chance of harvest. For if we do this in time, we may discover the worm which threatens the ripe fruit, or the barren spot where the harvest is withering for want of water.

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[1] In using the word "science," I refer to physical science, as I know nothing of natural science. Probably my remarks will, however, apply to both, but I do not know.

I am required to address the so-called physical section of this association. Fain would I speak pleasant words to you on this subject; fain would I recount to you the progress made in this subject by my countrymen, and their noble efforts to understand the order of the universe. But I go out to gather the grain ripe to the harvest, and I find only tares. Here and there a noble head of grain rises above the weeds; but so few are they, that I find the majority of my countrymen know them not, but think that they have a waving harvest, while it is only one of weeds after all. American science is a thing of the future, and not of the present or past; and the proper course of one in my position is to consider what must be done to create a science of physics in this country, rather than to call telegraphs, electric lights, and such conveniences, by the name of science. I do not wish to underrate the value of all these things; the progress of the world depends on them, and he is to be honored who cultivates them successfully. So also the cook who invents a new and palatable dish for the table benefits the world to a certain degree; yet we do not dignify him by the name of a chemist. And yet it is not an uncommon thing, especially in American newspapers, to have the *applications* of science confounded with pure science; and some obscure American who steals the ideas of some great mind of the past, and enriches himself by the application of the same to domestic uses, is often lauded above the great originator of the idea, who might have worked out hundreds of such applications, had his mind possessed the necessary element of vulgarity. I have often been asked, which was the more important to the world, pure or applied science. To have the applications of a science, the science itself must exist. Should we stop its progress, and attend only to its applications, we should soon degenerate into a people like the Chinese, who have made no progress for generations, because they have been satisfied with the applications of science, and have never sought for reasons in what they have done. The reasons constitute pure science. They have known the application of gunpowder for centuries; and yet the reasons for its peculiar action, if sought in the proper manner, would have developed the science of chemistry, and even of physics, with all their numerous applications. By contenting themselves with the fact that gunpowder will explode, and seeking no farther, they have fallen behind in the progress of the world; and we now regard this oldest and most numerous of nations as only barbarians, and yet our own country is in this same state. But we have done better, for we have taken the science of the old world, and applied it to all our uses, accepting it like the rain of heaven, without asking whence it came, or even acknowledging the debt of gratitude we owe to the great and unselfish workers who have given it to us; and, like the rain of heaven, this pure science has fallen upon our country, and made it great and rich and strong.

To a civilized nation of the present day, the applications of science are a necessity, and our country has hitherto succeeded in this line, only for the reason that there are certain countries in the world where pure science has been and is cultivated, and where the study of nature is considered a noble pursuit; but such countries are rare, and those who wish to pursue pure science in our own country must be prepared to face public opinion in a manner which requires much moral courage. They must be prepared to be looked down upon by every successful inventor whose shallow mind imagines that the only pursuit of mankind is wealth, and that he who obtains most has best succeeded in this world. Everybody can comprehend a million of money; but how few can comprehend any advance in scientific theory, especially in its more abstruse portions! And this, I believe, is one of the causes of the small number of persons who have ever devoted themselves to work of the higher order in any human pursuit. Man is a gregarious animal, and depends very much, for his happiness, on the sympathy of those around him; and it is rare to find one with the courage to pursue his own ideas in spite of his surroundings. In times past, men were more isolated than at present, and each came in contact with a fewer number of people. Hence that time constitutes the period when the great sculptures, paintings and poems were produced. Each man's mind was comparatively free to follow its own ideals, and the results were the great and unique works of the ancient masters. To-day the railroad and the telegraph, the books and newspapers, have united each individual man with the rest of the world: instead of his mind being an individual, a thing apart by itself, and unique, it has become so influenced by the outer world, and so dependent upon it, that it has lost its originality to a great extent. The man who in times past would naturally have been in the lowest depths of poverty, mentally and physically, to-day measures tape behind a counter, and with lordly air advises the naturally born genius how he may best bring his outward appearance down to a level with his own. A new idea he never had, but he can at least cover his mental nakedness with ideas imbibed from others. So the genius of the past soon perceives that his higher ideas are too high to be appreciated by the world; his mind is clipped down to the standard form; every natural offshoot upwards is repressed, until the man is no higher than his fellows. Hence the world, through the abundance of its intercourse, is reduced to a level. What was formerly a grand and magnificent landscape, with mountains ascending above the clouds, and depths whose gloom we cannot now appreciate, has become serene and peaceful. The depths have been filled, and the heights levelled, and the wavy harvests and smoky factories cover the landscape.

As far as the average man is concerned, the change is for the better. The average life of man is far pleasanter, and his mental condition better, than before. But we miss the vigor imparted by the mountains. We are tired of mediocrity, the curse of our country; we are tired of seeing our artists reduced to hirelings, and imploring congress to protect them against foreign competition; we are tired of seeing our countrymen take their science from abroad, and boast that they here convert it into wealth; we are tired of seeing our professors degrading their chairs by the pursuit of applied science instead of pure science, or sitting inactive while the whole world is open to investigation; lingering by the wayside while the problem of the universe remains unsolved. We wish for something higher and nobler in this country of mediocrity, for a mountain to relieve the landscape of its monotony. We are surrounded with mysteries, and have been created with minds to enjoy and reason to aid in the unfolding of such mysteries. Nature calls to us to study her, and our better feelings urge us in the same direction.

For generations there have been some few students of science who have esteemed the study of nature the most noble of pursuits. Some have been wealthy, and some poor; but they have all had one thing in common,— the love of nature and its laws. To these few men the world owes all the progress due to applied science, and yet very few ever received any payment in this world for their labors.

Faraday, the great discoverer of the principle on which all machines for electric lighting, electric railways, and the transmission of power, must rest, died a poor man, although others and the whole world have been enriched by his discoveries; and such must be the fate of the followers in his footsteps for some time to come.

But there will be those in the future who will study nature from pure love, and for them higher prizes than any yet obtained are waiting. We have but yet commenced our pursuit of science, and stand upon the threshold wondering what there is within. We explain the motion of the planets by the law of gravitation; but who will explain how two bodies, millions of miles apart, tend to go toward each other with a certain force? We now weigh and measure electricity and electric currents with as much ease as ordinary matter, yet have we made any approach to an explanation of the phenomenon of electricity? Light is an undulatory motion, and yet do we know what it is that undulates? Heat is motion, yet do we know what it is that moves? Ordinary matter is a common substance, and yet who shall fathom the mystery of its internal constitution?

There is room for all in the work, and the race has but commenced. The problems are not to be solved in a moment, but need the best work of the best minds, for an indefinite time.

Shall our country be contented to stand by, while other countries lead in the race? Shall we always grovel in the dust, and pick up the crumbs which fall from the rich man's table, considering ourselves richer than he because we have more crumbs, while we forget that he has the cake, which is the source of all crumbs? Shall we be swine, to whom the corn and husks are of more value than the pearls? If I read aright the signs of the times, I think we shall not always be contented with our inferior position. From looking down we have almost become blind, but may recover. In a new country, the necessities of life must be attended to first. The curse of Adam is upon us all, and we must earn our bread.

But it is the mission of applied science to render this easier for the whole world. There is a story which I once read, which will illustrate the true position of applied science in the world. A boy, more fond of reading than of work, was employed, in the early days of the steam-engine, to turn the valve at every stroke. Necessity was the mother of invention in his case: his reading was disturbed by his work, and he soon discovered that he might become free from his work by so tying the valve to some movable portion of the engine, as to make it move its own valve. So I consider that the true pursuit of mankind is intellectual. The scientific study of nature in all its branches, of mathematics, of mankind in its past and present, the pursuit of art, and the cultivation of all that is great and noble in the world,— these are the highest occupation of mankind. Commerce, the applications of science, the accumulation of wealth, are necessities which are a curse to those with high ideals, but a blessing to that portion of the world which has neither the ability nor the taste for higher pursuits.

As the applications of science multiply, living becomes easier, the wealth necessary for the purchase of apparatus can be obtained, and the pursuit of other things beside the necessities of life becomes possible.

But the moral qualities must also be cultivated in proportion to the wealth of the country, before much can be done in pure science. The successful sculptor or painter naturally attains to wealth through the legitimate work of his profession. The novelist, the poet, the musician, all have wealth before them as the end of a successful career. But the scientist and the mathematician have no such incentive to work: they must earn their living by other pursuits, usually teaching, and only devote their surplus time to the true pursuit of their science. And frequently, by the small salary which they receive, by the lack of instrumental and literary facilities, by the mental atmosphere in which they exist, and, most of all, by their low ideals of life, they are led to devote their surplus time to applied science or to other means of increasing their fortune. How shall we, then, honor the few, the very few, who, in spite of all difficulties, have kept their eyes fixed on the goal, and have steadily worked for pure science, giving to the world a most precious donation, which has borne fruit in our greater knowledge of the universe and in the applications to our physical life which have enriched thousands and benefited each one of us? There are also those who have every facility for the pursuit of science, who have an ample salary and every appliance for work, yet who devote themselves to commercial work, to testifying in courts of law, and to any other work to increase their present large income. Such men would be respectable if they gave up the name of professor, and took that of consulting chemist or physicist. And such men are needed in the community. But for a man to occupy the professor's chair in a prominent college, and, by his energy and ability in the commercial applications of his science, stand before the local community as a newspaper exponent of his science, is a disgrace both to him and his college. It is the death-blow to science in that region. Call him by his proper name, and he becomes at once a useful member of the community. Put in his place a man who shall by precept and example cultivate his science, and how different is the result! Young men, looking forward into the world for something to do, see before them this high and noble life, and they see that there is something more honorable than the accumulation of wealth. They are thus led to devote their lives to similar pursuits, and they honor the professor who has drawn them to something higher than they might otherwise have aspired to reach.

I do not wish to be misunderstood in this matter. It is no disgrace to make money by an invention, or otherwise, or to do commercial scientific work under some circumstances; but let pure science be the aim of those in the chairs of professors, and so prominently the aim that there can be no mistake. If our aim in life is wealth, let us honestly engage in commercial pursuits, and compete with others for its possession; but if we choose a life which we consider higher, let us live up to it, taking wealth or poverty as it may chance to come to us, but letting neither turn us aside from our pursuit.

The work of teaching may absorb the energies of many; and, indeed, this is the excuse given by most for not doing any scientific work. But there is an old saying, that where there is a will there is a way. Few professors do as much teaching or lecturing as the German professors, who are also noted for their elaborate papers in the scientific journals. I myself have been burdened down with work, and know what it is; and yet I here assert that all *can* find time for scientific research if they desire it. But here, again, that curse of our country, mediocrity, is upon us. Our colleges and universities seldom call for first-class men of reputation, and I have even heard the trustee of a well-known college assert that no professor should engage in research because of the time wasted. I was glad to see, soon after, by the call of a prominent scientist to that college, that the majority of the trustees did not agree with him.

That teaching is important, goes without saying. A successful teacher is to be respected; but if he does not lead his scholars to that which is highest, is he not blameworthy? We are, then, to look to the colleges and universities of the land for most of the work in pure science which is done. Let us therefore examine these latter, and see what the prospect is.

One, whom perhaps we may here style a practical follower of Ruskin, has stated that while in this country he was variously designated by the title of captain, colonel, and professor. The story may or may not be true, but we all know enough of the customs of our countrymen not to dispute it on general principles. All men are born equal: some men are captains, colonels, and professors, and therefore all men are such. The logic is conclusive; and the same kind of logic seems to have been applied to our schools, colleges, and universities. I have before me the report of the commissioner of education for 1880. According to that report, there were 389,[1] or say, in round numbers, 400 institutions, calling themselves colleges or universities, in our country! We may well exclaim that ours is a great country, having more than the whole world beside. The fact is sufficient. The whole earth could hardly support such a number of first-class institutions. The curse of mediocrity must be upon them, to swarm in such numbers. They must be a cloud of mosquitoes, instead of eagles as they profess; and this becomes evident on further analysis. About one-third aspire to the name of university; and I note one called by that name which has two professors and eighteen students, and another having three teachers and twelve students! These instances are not unique, for the number of small institutions and schools which call themselves universities is very great. It is difficult to decide from the statistics alone the exact standing of these institutions. The extremes are easy to manage. Who can doubt that an institution with over eight hundred students, and a faculty of seventy, is of a higher grade than those above cited having ten or twenty students and two or three in the faculty? Yet this is not always true; for I note one institution with over five hundred students which is known to me personally as of the grade of a high school. The statistics are more or less defective, and it would much weaken the force of my remarks if I went too much into detail. I append the following tables, however, of 330 so-called colleges and universities:—

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[1] 364 reported on, and 25 not reported.

218 had from 0 to 100 students.
88 " " 100 " 200 "
12 " " 200 " 300 "
6 " " 300 " 500 "
6 over 500

Of 322 so-called colleges and universities:—

206 had 0 to 10 in the faculty.
99 " 10 " 20 " "
17 " 20 or over " "

If the statistics were forthcoming,—and possibly they may exist,—we might also get an idea of the standing of these institutions and their approach to the true university idea, by the average age of the scholars. Possibly also the ratio of number of scholars to teachers might be of some help. All these methods give an approximation to the present standing of the institutions. But there is another method of attacking the problem, which is very exact, yet it only gives us the *possibilities* in the case of the institution. I refer to the wealth of the institution. In estimating the wealth, I have not included the value of grounds and building, for this is of little importance, either to the present or future standing of the institution, as good work can be done in a hovel as in a palace. I have taken the productive funds of the institution as the basis of estimate. I find:—

234 have below $500,000.
8 " between $500,000 and $1,000,000.
8 " over $1,000,000.

There is no fact more firmly established, all over the world, than that the higher education can never be made to pay for itself. Usually the cost to a college, of educating a young man, very much exceeds what he pays for it, and is often three or four times as much. The higher the education, the greater this proportion will be; and a university of the highest class should anticipate only a small accession to its income from the fees of students. Hence the test I have applied must give a true representation of the possibilities in every case. According to the figures, only sixteen colleges and universities have $500,000 or over of invested funds, and only one-half of these have $1,000,000 and over. Now, even the latter sum is a very small endowment for a college; and to call any institution a university which has less than $1,000,000 is to render it absurd in the face of the world. And yet more than 100 of our institutions, many of them very respectable colleges, have abused the word "university" in this manner. It is to be hoped that the endowment of the more respectable of these institutions may be increased, as many of them deserve it; and their unfortunate appellation has probably been repented of long since.

But what shall we think of a community that gives the charter of a university to an institution with a total of $20,000 endowment, two so-called professors, and eighteen students! or another with three professors, twelve students, and a total of $27,000 endowment, mostly invested in buildings! And yet there are very many similar institutions; there being sixteen with three professors or less, and very many indeed with only four or five.

Such facts as these could only exist in a democratic country, where pride is taken in reducing everything to a level. And I may also say, that it can only exist in the early days of such a democracy; for an intelligent public will soon perceive that calling a thing by a wrong name does not change its character, and that truth, above all things, should be taught to the youth of the nation.

It may be urged, that all these institutions are doing good work in education; and that many young men are thus taught, who could not afford to go to a true college or university. But I do not object to the education,— though I have no doubt an investigation would disclose equal absurdities here,— for it is aside from my object. But I do object to lowering the ideals of the youth of the country. Let them know that they are attending a school, and not a university; and let them know that above them comes the college, and above that the university. Let them be taught that they are only half educated, and that there are persons in the world by whose side they are but atoms. In other words, let them be taught the truth.

It may be that some small institutions are of high grade, especially those which are new; but who can doubt that more than two-thirds of our institutions calling themselves colleges and universities are unworthy of the name? Each one of these institutions has so-called professors, but it is evident that they can be only of the grade of teachers. Why should they not be so called? The position of teacher is an honored one, but is not made more honorable by the assumption of a false title. Furthermore, the multiplication of the title, and the ease with which it can be obtained, render it scarcely worth striving for. When the man of energy, ability, and perhaps genius is rewarded by the same title and emoluments as the commonplace man with the modicum of knowledge, who takes to teaching, not because of any aptitude for his work, but possibly because he has not the energy to compete with his fellow-men in business, then I say one of the inducements for first-class men to become professors is gone.

When work and ability are required for the position, and when the professor is expected to keep up with the progress of his subject, and to do all in his power to advance it, and when he is selected for these reasons, then the position will be worth working for, and the successful competitor will be honored accordingly. The chivalric spirit which prompted Faraday to devote his life to the study of nature may actuate a few noble men to give their lives to scientific work; but if we wish to cultivate this highest class of men in science, we must open a career for them worthy of their efforts.

Jenny Lind, with her beautiful voice, would have cultivated it to some extent in her native village; yet who would expect her to travel over the world, and give concerts for nothing? and how would she have been able to do so if she had wished? And so the scientific man, whatever his natural talents, must have instruments and a library, and a suitable and respectable salary to live upon, before he is able to exert himself to his full capacity. This is true of advance in all the higher departments of human learning, and yet something more is necessary. It is not those in this country who receive the largest salary, and have positions in the richest colleges, who have advanced their subject the most: men receiving the highest salaries, and occupying the professor's chair, are to-day doing absolutely nothing in pure science, but are striving by the commercial applications of their science to increase their already large salary. Such pursuits, as I have said before, are honorable in their proper place; but the duty of a professor is to advance his science, and to set an example of pure and true devotion to it which shall demonstrate to his students and the world that there is something high and noble worth living for. Money-changers are often respectable men, and yet they were once severely rebuked for carrying on their trade in the court of the temple.

Wealth does not constitute a university, buildings do not: it is the men who constitute its faculty, and the students who learn from them. It is the last and highest step which the mere student takes. He goes forth into the world, and the height to which he rises has been influenced by the ideals which he has consciously or unconsciously imbibed in his university. If the professors under whom he has studied have been high in their profession, and have themselves had high ideals; if they have considered the advance of their particular subject their highest work in life, and are themselves honored for their intellect throughout the world,—the student is drawn toward that which is highest, and ever after in life has high ideals. But if the student is taught by what are sometimes called good teachers, and teachers only, who know little more than the student, and who are often surpassed and even despised by him, no one can doubt the lowered tone of his mind. He finds that by his feeble efforts he can surpass one to whom a university has given its highest honor; and he begins to think that he himself is a born genius, and the incentive to work is gone. He is great by the side of the molehill, and does not know any mountain to compare himself with.

A university should have not only great men in its faculty, but have numerous minor professors and assistants of all kinds, and should encourage the highest work, if for no other reason than to encourage the student to his highest efforts.

But, assuming that the professor has high ideals, wealth such as only a large and high university can command is necessary to allow him the fullest development.

And this is specially so in our science of physics. In the early days of physics and chemistry, many of the fundamental experiments could be performed with the simplest apparatus. And so we often find the names of Wollaston and Faraday mentioned as needing scarcely anything for their researches. Much can even now be done with the simplest apparatus, and nobody, except the utterly incompetent, need stop for want of it; but the fact remains, that one can only be free to investigate in all departments of chemistry and physics, when he not only has a complete laboratory at his command, but a fund to draw on for the expenses of each experiment. That simplest of the departments of physics, namely, astronomy, has now reached such perfection that nobody can expect to do much more in it without a perfectly equipped observatory; and even this would be useless without an income sufficient to employ a corps of assistants to make the observations and computations. But even in this simplest of physical subjects, there is great misunderstanding. Our country has very many excellent observatories, and yet little work is done in comparison, because no provision has been made for maintaining the work of the observatory; and the wealth which, if concentrated, might have made one effective observatory which would prove a benefit to astronomical science, when scattered among a half-dozen merely furnishes telescopes for the people in the surrounding region to view the moon with. And here I strike the keynote of at least one need of our country, if she would stand well in science; and the following item which I clip from a newspaper will illustrate the matter:—

"The eccentric old Canadian, Arunah Huntington, who left $200,000 to be divided among the public schools of Vermont, has done something which will be of little practical value to the schools. Each district will be entitled to the insignificant sum of $10, which will not advance much the cause of education."

Nobody will dispute the folly of such a bequest, or the folly of filling the country with telescopes to look at the moon, and calling them observatories. How much better to concentrate the wealth into a few parcels, and make first-class observatories and institutions with it!

Is it possible that any of our four hundred colleges and universities have love enough of learning to unite with each other and form larger institutions? Is it possible that any have such a love of truth that they are willing to be called by their right name? I fear not; for the spirit of expectation, which is analogous to the spirit of gambling, is strong in the American breast, and each institution which now, except in name, slumbers in obscurity, expects in time to bloom out into full prosperity. Although many of them are under religious influence, where truth is inculcated, and where men are taught to take a low seat at the table in order that they may be honored by being called up higher, and not dishonored by being thrust down lower, yet these institutions have thrust themselves into the highest seats, and cannot probably be dislodged.

But would it not be possible so to change public opinion that no college could be founded with a less endowment than say $1,000,000, or no university with less than three or four times that amount? From the report of the commissioner of education, I learn that such a thing is taking place; that the tendency towards large institutions is increasing, and that it is principally in the west and southwest that the multiplication of small institutions with big names is to be feared most, and that the east is almost ready for the great coming university.

The total wealth of the four hundred colleges and universities in 1880 was about $40,000,000 in buildings, and $43,000,000 in productive funds. This would be sufficient for one great university of $10,000,000, four of $5,000,000, and twenty-six colleges of $2,000,000 each. But such an idea can of course never be carried out. Government appropriations are out of the question, because no political trickery must be allowed around the ideal institution.

In the year 1880 the private bequests to all schools and colleges amounted to about $5,500,000; and, although there was one bequest of $1,250,000, yet the amount does not appear to be phenomenal. It would thus seem that the total amount was about five million dollars in one year, of which more than half is given to so-called colleges and universities. It would be very difficult to regulate these bequests so that they might be concentrated sufficiently to produce an immediate result. But the figures show that generosity is a prominent feature of the American people, and that the needs of the country only have to be appreciated to have the funds forthcoming. We must make the need of research and of pure science felt in the country. We must live such lives of pure devotion to our science, that all shall see that we ask for money, not that we may live in indolent ease at the expense of charity, but that we may work for that which has advanced and will advance the world more than any other subject, both intellectually and physically. We must live such lives as to neutralize the influence of those who in high places have degraded their profession, or have given themselves over to ease, and do nothing for the science which they represent. Let us do what we can with the present means at our disposal. There is not one of us who is situated in the position best adapted to bring out all his powers, and to allow him to do most for his science.

All have their difficulties, and I do not think that circumstances will ever radically change a man. If a man has the instinct of research in him, it will always show itself in some form. But circumstances may direct it into new paths, or may foster it so that what would otherwise have died as a bud now blossoms and ripens into the perfect fruit.

Americans have shown no lack of invention in small things; and the same spirit when united to knowledge and love of science, becomes the spirit of research. The telegraph-operator, with his limited knowledge of electricity and its laws, naturally turns his attention to the improvement of the only electrical instrument he knows anything about; and his researches would be confined to the limited sphere of his knowledge, and to the simple laws with which he is acquainted. But as his knowledge increases, and the field broadens before him, as he studies the mathematical theory of the subject, and the electro-magnetic theory of light loses the dim haze due to distance, and becomes his constant companion, the telegraph instrument becomes to him a toy, and his effort to discover something new becomes research in pure science.

It is useless to attempt to advance science until one has mastered the science: he must step to the front before his blows can tell in the strife. Furthermore, I do not believe anybody can be thorough in any department of science, without wishing to advance it. In the study of what is known, in the reading of the scientific journals, and the discussions therein contained of the current scientific questions, one would obtain an impulse to work, even though it did not before exist; and the same spirit which prompted him to seek what was already known would make him wish to know the unknown. And I may say that I never met a case of thorough knowledge in my own science, except in the case of well-known investigators. I have met men who talked well, and I have sometimes asked myself why they did not do something; but further knowledge of their character has shown the superficiality of their knowledge. I am no longer a believer in men who could do something if they would, or would do something if they had a chance. They are impostors. If the spirit is there, it will show itself in spite of circumstances.

As I remarked before, the investigator in pure science is usually a professor. He must teach as well as investigate. It is a question which has been discussed in late years, as to whether these two functions had better be combined in the same individual, or separated. It seems to be the opinion of most, that a certain amount of teaching is conducive, rather than otherwise, to the spirit of research. I myself think that this is true, and I should myself not like to give up my daily lecture; but one must not be overburdened. I suppose that the true solution, in many cases, would be found in the multiplication of assistants, not only for the work of teaching but of research. Some men are gifted with more ideas than they can work out with their own hands, and the world is losing much by not supplying them with extra hands. Life is short: old age comes quickly, and the amount one pair of hands can do is very limited. What sort of shop would that be, or what sort of factory, where one man had to do all the work with his own hands? It is a fact in nature, which no democracy can change, that men are *not* equal,—that some have brains, and some hands; and no idle talk about equality can ever subvert the order of the universe.

I know of no institution in this country where assistants are supplied to aid directly in research; yet why should it not be so? Even the absence of assistant professors and assistants of all kinds, to aid in teaching, is very noticeable, and must be remedied before we can expect much.

There are many physical problems, especially those requiring exact measurements, which cannot be carried out by one man, and can only be successfully attacked by the most elaborate apparatus, and with a full corps of assistants. Such are Regnault's experiments on the fundamental laws of gases and vapors, made thirty or forty years ago by aid from the French government, and which are the standards to this day. Although these experiments were made with a view to the practical calculation of the steam-engine, yet they were carried out in such a broad spirit that they have been of the greatest theoretical use. Again, what would astronomy have done without the endowment of observatories? By their means, that science has become the most perfect of all branches of physics, as it should be from its simplicity. There is no doubt, in my mind, that similar institutions for other branches of physics, or, better, to include the whole of physics, would be equally successful. A large and perfectly equipped physical laboratory with its large revenues, its corps of professors and assistants, and its machine-shop for the construction of new apparatus, would be able to advance our science quite as much as endowed observatories have advanced astronomy. But such a laboratory should not be founded rashly. The value will depend entirely on the physicist at its head, who has to devise the plan, and to start it into practical working. Such a man will always be rare, and cannot always be obtained. After one had been successfully started, others could follow; for imitation requires little brains.

One could not be certain of getting the proper man every time, but the means of appointment should be most carefully studied so as to secure a good average. There can be no doubt that the appointment should rest with a scientific body capable of judging the highest work of each candidate.

Should any popular element enter, the person chosen would be of the literary-scientific order, or the dabbler on the outskirts who presents his small discoveries in the most theatrical manner. What is required is a man of depth, who has such an insight into physical science that he can tell when blows will best tell for its advancement.

Such a grand laboratory as I describe does not exist in the world, at present, for the study of physics. But no trouble has ever been found in obtaining means to endow astronomical science. Everybody can appreciate, to some extent, the value of an observatory; as astronomy is the simplest of scientific subjects, and has very quickly reached a position where elaborate instruments and costly computations are necessary to further advance. The whole domain of physics is so wide that workers have hitherto found enough to do. But it cannot always be so, and the time has even now arrived when such a grand laboratory should be founded. Shall our country take the lead in this matter, or shall we wait for foreign countries to go before? They will be built in the future, but when and how is the question.

Several institutions are now putting up laboratories for physics. They are mostly for teaching, and we can expect only a comparatively small amount of work from most of them. But they show progress; and, if the progress be as quick in this direction as in others, we should be able to see a great change before the end of our lives.

As stated before, men are influenced by the sympathy of those with whom they come in contact. It is impossible to change public opinion in our favor immediately; and, indeed, we must always seek to lead it, and not be guided by it. For pure science is the pioneer who must not hover about cities and civilized countries, but must strike into unknown forests, and climb the hitherto inaccessible mountains which lead to and command a view of the promised land,—the land which science promises us in the future; which shall not only flow with milk and honey, but shall give us a better and more glorious idea of this wonderful universe. We must create a public opinion in our favor, but it need not at first be the general public. We must be contented to stand aside, and see the honors of the world for a time given to our inferiors; and must be better contented with the approval of our own consciences, and of the very few who are capable of judging our work, than of the whole world beside. Let us look to the other physicists, not in our own town, not in our own country, but in the whole world, for the words of praise which are to encourage us, or the words of blame which are to stimulate us to renewed effort. For what to us is the praise of the ignorant? Let us join together in the bonds of our scientific societies, and encourage each other, as we are now doing, in the pursuit of our favorite study; knowing that the world will sometime recognize our services, and knowing, also, that we constitute the most important element in human progress.

But danger is also near, even in our societies. When the average tone of the society is low, when the highest honors are given to the mediocre, when third-class men are held up as examples, and when trifling inventions are magnified into scientific discoveries, then the influence of such societies is prejudicial. A young scientist attending the meetings of such a society soon gets perverted ideas. To his mind, a molehill is a mountain, and the mountain a molehill. The small inventor or the local celebrity rises to a greater height, in his mind, than the great leader of science in some foreign land. He gauges himself by the molehill, and is satisfied with his stature; not knowing that he is but an atom in comparison with the mountain, until, perhaps, in old age, when it is too late. But, if the size of the mountain had been seen at first, the young scientist would at least have been stimulated in his endeavor to grow.

We cannot all be men of genius; but we can, at least, point them out to those around us. We may not be able to benefit science much ourselves; but we can have high ideals on the subject, and instil them into those with whom we come in contact.

For the good of ourselves, for the good of our country, for the good to the world, it is incumbent on us to form a true estimate of the worth and standing of persons and things, and to set before our own minds all that is great and good and noble, all that is most important for scientific advance, above the mean and low and unimportant.

It is very often said, that a man has a right to his opinion. This might be true for a man on a desert island, whose error would influence only himself; but when he opens his lips to instruct others, or even when he signifies his opinions by his daily life, then he is directly responsible for all his errors of judgment or fact. He has no right to think a molehill as big as a mountain, nor to teach it, any more than he has to think the world is flat, and teach that it is so. The facts and laws of our science have *not* equal importance, neither have the men who cultivate the science achieved equal results. One thing is greater than another, and we have no right to neglect the order. Thus shall our minds be guided aright, and our efforts be toward that which is the highest.

Then shall we see that no physicist of the first class has ever existed in this country, that we must look to other countries for our leaders in that subject, and that the few excellent workers in our country must receive many accessions from without before they can constitute an American science, or do their share in the world's work.

But let me return to the subject of scientific societies. Here American science has its hardest problem to contend with. There are very many local societies dignified by high-sounding names, each having its local celebrity, to whom the privilege of describing some crab with an extra claw, which he found in his morning ramble, is inestimable. And there are some academies of science, situated at our seats of learning, which are doing good work in their locality. But distances are so great that it is difficult to collect men together at any one point. The American Association, which we are now attending, is not a scientific academy, and does not profess to be more than a gathering of all who are interested in science, to read papers and enjoy social intercourse. The National Academy of Sciences contains eminent men from the whole country, but then it is only for the purpose of advising the government freely on scientific matters. It has no building, it has no library; and it publishes nothing except the information which it freely gives to the government, which does nothing for it in return. It has not had much effect directly on American science; but the liberality of the government in the way of scientific expeditions, publications, etc., is at least partly due to its influence, and in this way it has done much good. But it in no way takes the place of the great Royal society, or the great academies of science at Paris, Berlin, Vienna, St. Petersburgh, Munich, and, indeed, all the European capitals and large cities. These, by their publications, give to the young student, as well as to the more advanced physicist, models of all that is considered excellent; and to become a member is one of the highest honors to which he can aspire, while to write a memoir which the academy considers worthy to be published in its transactions excites each one to his highest effort.

The American Academy of Sciences in Boston is perhaps our nearest representation of this class of academies, but its limitation of membership to the state deprives it of a national character.

But there is another matter which influences the growth of our science.

As it is necessary for us still to look abroad for our highest inspiration in pure science, and as science is not an affair of one town or one country, but of the whole world, it becomes us all to read the current journals of science and the great transactions of foreign societies, as well as those of our own countries. These great transactions and journals should be in the library of every institution of learning in the country, where science is taught. How can teachers and professors be expected to know what has been discovered in the past, or is being discovered now, if these are not provided? Has any institution a right to starve mentally the teachers whom it employs, or the students who come to it? There can be but one answer to this; and an institution calling itself a university, and not having the current scientific journals upon its table or the transactions of societies upon its library-shelves, is certainly not doing its best to cultivate all that is best in this world.

We call this a free country, and yet it is the only one where there is a direct tax upon the pursuit of science. The low state of pure science in our country may possibly be attributed to the youth of the country; but a direct tax, to prevent the growth of our country in that subject, cannot be looked upon as other than a deep disgrace. I refer to the duty upon foreign books and periodicals. In our science, no books above elementary ones have ever been published, or are likely to be published in this country; and yet every teacher in physics must have them, not only in the college library, but on his own shelves, and must pay the government of this country to allow him to use a portion of his small salary to buy that which is to do good to the whole country. All freedom of intercourse which is necessary to foster our growing science is thus broken off, and that which might, in time, relieve our country of its mediocrity, is nipped in the bud by our government, which is most liberal when appealed to directly on scientific subjects.

One would think that books in foreign languages might be admitted free; but to please the half-dozen or so workmen who reprint German books, not scientific, our free intercourse with that country is cut off. Our scientific associations and societies must make themselves heard in this matter, and show those in authority how the matter stands.

In conclusion, let me say once more, that I do not believe that our country is to remain long in its present position. The science of physics, in whose applications our country glories, is to arise among us, and make us respected by the nations of the world. Such a prophecy may seem rash with regard to a nation which does not yet do enough physical work to support a physical journal. But we know the speed with which we advance in this country: we see cities springing up in a night, and other wonders performed at an unprecedented rate. And now we see physical laboratories being built, we see a great demand for thoroughly trained physicists, who have not shirked their mathematics, both as professors and in so-called practical life; and perhaps we have the feeling, common to all true Americans, that our country is going forward to a glorious future, when we shall lead the world in the strife for intellectual prizes as we now do in the strife for wealth.

But if this is to be so, we must not aim low. The problems of the universe cannot be solved without labor; they cannot be attacked without the proper intellectual as well as physical tools; and no physicist need expect to go far without his mathematics. No one expects a horse to win in a great and long race who has not been properly trained; and it would be folly to attempt to win with one, however pure his blood and high his pedigree, without it. The problems we solve are more difficult than any race; the highest intellect cannot hope to succeed without proper preparation. The great prizes are reserved for the greatest efforts of the greatest intellects, who have kept their mental eye bright and flesh hard by constant exercise. Apparatus can be bought with money, talents may come to us at birth; but our mental tools, our mathematics, our experimental ability, our knowledge of what others have done before us, all have to be obtained by work. The time is almost past, even in our own country, when third-rate men can find a place as teachers, because they are unfit for everything else. We wish to see brains and learning, combined with energy and immense working power, in the professor's chair; but, above all, we wish to see that high and chivalrous spirit which causes one to pursue his idea in spite of all difficulties, to work at the problems of nature with the approval of his own conscience, and not of men before him. Let him fit himself for the struggle with all the weapons which mathematics and the experience of those gone before him can furnish, and let him enter the arena with the fixed and stern purpose to conquer. Let him not be contented to stand back with the crowd of mediocrity, but let him press forward for a front place in the strife.

The whole universe is before us to study. The greatest labor of the greatest minds has only given us a few pearls; and yet the limitless ocean, with its hidden depths filled with diamonds and precious stones, is before us. The problem of the universe is yet unsolved, and the mystery involved in one single atom yet eludes us. The field of research only opens wider and wider as we advance, and our minds are lost in wonder and astonishment at the grandeur and beauty unfolded before us. Shall we help in this grand work, or not? Shall our country do its share, or shall it still live in the almshouse of the world?

PAPERS READ.

DETERMINATION OF THE RELATION BETWEEN THE IMPERIAL YARD AND THE METRE OF THE ARCHIVES. By WILLIAM A. ROGERS of Cambridge, Mass.

[ABSTRACT.]

THIS paper was a continuation of one upon the same subject presented at the Montreal meeting. The mean result of the determinations up to that time was as follows: Imperial Yard + 3.37015 inches = Metre des Archives.

The writer stated at that time, that he should not like to be held to a very strict account with regard to the last decimal figure, or even the last two decimal figures, on account of the difficulty of obtaining the requisite data.

Since the meeting last year, additional data have been obtained. In February of the present year, a combined yard and metre was received from Paris. The yard was compared with the Imperial Yard, in 1880, by Mr. Chaney, the Warden of the Imperial Standards. During the interval between 1880 and February of the present year, this metre has received repeated comparisons with the metre of the International Bureau, under the direction of Dr. Pernet. According to his report, this metre is 310 mikrons too short at 0° centigrade; for the same temperature, the yard was found by Mr. Chaney to be 20.7 mikrons too short.

Comparing the metre and the yard upon this bar with the bronze yard and metre described at Montreal, and combining the results with those previously found, the relation was found as follows: Imperial Yard + 3.37027 inches = Metre des Archives.

The details of the investigation will be found in the Proceedings of the American Academy of Arts and Sciences for 1883.

THE MAGNETOPHONE, OR THE MODIFICATION OF THE MAGNETIC FIELD BY THE ROTATION OF A PERFORATED METALLIC DISK.

By H. S. CARHART of Evanston, Ill.

[ABSTRACT.[1]]

THE experiments of Bell, Preece, and others, on the radiophone, suggested the possibility of interrupting, or at least periodically modifying, the lines of force proceeding from the poles of a magnet, by means of a disk of sheet-iron, perforated with a series of equidistant holes, and rotated so that the holes should pass directly in front of the magnetic pole. It is well known that the armature placed on the poles of a permanent magnet diminishes the strength of the external field of force by furnishing superior facilities for the formation of polarized chains of particles from pole to pole. This is the case even when the armature does not touch the poles, but is in close proximity to them.

If a piece of sheet-iron be placed over the poles of a magnet without touching, and magnetic curves be developed on paper above the iron, they will be found to exhibit less intense and less sharply defined magnetic action than when the sheet-iron is removed. If, however, a small hole be drilled directly over each magnetic pole, the screening action of the sheet-iron is modified in much the same way as when a hole is made in a screen opaque to light; for the developed curves show distinctly the outline of the holes. If, therefore, the sheet-iron in the form of a circular plate, pierced with a number of holes, be rapidly rotated between the pole of a magnet and a small induction bobbin, the action of the magnet on the core of the bobbin will be periodically modified, because of the passing holes; and hence induced currents will flow through a circuit including the bobbin. A disk of sheet-iron was pierced with two circles of quarter-inch holes concentric with the disk, the number of holes in the two circles being thirty-two and sixty-four respectively. On one side of the disk was placed a horseshoe magnet with its poles very near the rows of holes; on the other side were arranged two corresponding induction bobbins. The circuit was completed through a telephone and either bobbin at pleasure. Upon rotating the disk rapidly, a clear musical sound was produced in the telephone, the pitch rising with the rapidity of rotation. Moreover, the bobbin opposite the circle of sixty-four holes gave the octave above the other, and each gave a note of the same pitch as was produced by blowing a stream of air through the corresponding holes.

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[1] This paper is published in full with illustrations in SCIENCE for September 21, 1883.

Non-magnetic metallic disks produce similar musical notes by the periodic modification of the magnetic field by means of the distortion or bending of the lines of force. A periodic movement of the lines of force will take place when the disk rotates, because the solid parts deflect the lines in the direction of the rotation, while the passage of a hole allows them to fall back.

MAGNETIC SURVEY OF MISSOURI. By F. E. NIPHER of St. Louis, Missouri.

[ABSTRACT.]

IN the spring of 1878 a survey of Missouri was begun, which was expected to determine all points in regard to terrestrial magnetism: 160 points have been covered. The work was undertaken under private auspices, most of the money tendered unasked, and the work has been carried on successfully until the present time. The first three years were spent in making a preliminary survey. In the early part of the survey we labored under great difficulties, because I supposed that the lines of equal value, laid down upon the observations given in the coast-survey charts, were substantially correct; so that time was frequently lost in repeating values at stations left behind, in order to be certain that no error had been committed. But when we settled down to the conclusion that we really knew nothing about the matter, we had very much less trouble. At first, intensity determinations were made at each station; but in later years, since the magnets have proved so satisfactory, the plan was adopted of making absolute determinations only at regular intervals during the summer. The temperature corrections for the magnet were made twice,— once in 1878, and once two years ago,— and they agreed very closely with each other.

The dip circle was a large one, such as was formerly much used, and which was found to be an excellent instrument, though rather clumsy to carry. The charts which have been prepared show what the results were. In a former communication to the association at Cincinnati, I suggested an explanation of the peculiar flexures of the isogonic lines, as being due to earth-currents which seemed to be deflected by the moist river-valleys. The map upon which that hypothesis was based represented observations taken over the entire state. By re-determination we have found that those observations were all correct; but more detailed work shows that this explanation is not admissible. There is no evidence of the fact that contour has anything to do with the deviation of the needle from the normal values. Similar flexures are also seen in the lines of equal inclination and the lines of intensity. One and perhaps two years will be required to accomplish the work properly. There is nothing new in the subject, except the rather unexpected flexures which we found in these lines. It shows very clearly that the isogonic lines which are published for the use of surveyors are of no earthly use. Work ought to be done in a detailed way over the whole country; and I hope we shall some time be able to combine with these determinations a series of magnetic values at ten or twelve different stations in the state of Missouri, and also simultaneous determinations of earth-currents upon lines making angles with each other at the different stations. Similar variations would probably be found in the states of Illinois and Iowa.

A METHOD OF DISTRIBUTING WEATHER FORECASTS BY MEANS OF RAILWAYS. By T. C. MENDENHALL of Columbus, Ohio.

[ABSTRACT.]

THIS system has only been in operation in Ohio for about a year. To distribute forecasts, we place signals upon the sides of the baggage-cars, as distinct as possible from each other, so as to be easily recognized at considerable distances, and also to convey as much meaning as possible, so as to predict as many different conditions. We adopted a combination of form and color. The signals are three in number as to form, and two in number as to color. The red signals are confined to predictions as to temperature,— rise in temperature, stationary temperature, falling temperature. The other color is blue, and that is confined to predictions in regard to the general state of the weather. The question of form was a good deal considered, and three forms were adopted. We adopted the sun, moon, and star, because everybody was familiar with those words. We experimented with the triangle, and finally rejected it. The device for attaching to the car is due to Mr. Anderson, who has been in the service of the board of commissioners for the past year, and it is a really happy device. The signal is made as large as possible, and the disk can be seen a long distance. The red sun and blue moon mean higher temperature and general rain. The crescent means lower temperature; the full disk of blue means general rain; the star represents local rains. With regard to the proper working of the system, though it has been in operation but a short time, it has really done good work. We receive special telegrams every morning, and they are transmitted to the train-despatchers at five o'clock. We are as yet operating it only on one railroad. It happens, fortunately, that that road goes through an agricultural region of considerable importance. It is the road connecting the cities of Columbus and Cleveland. Two trains start out in the morning, at the middle point between those cities. The signals are put on the cars at five o'clock in the morning; and as they run through the morning hours, the farmers along the line can have an opportunity of seeing them, and predicting the weather for the day. The railway company circulated through the whole line little cards, having these signals displayed in colors, with their meaning in every combination. This helps us, because it enables everybody to understand what is meant. A recent communication from Gen. Hazen indicates a disposition on the part of the general government to take hold of the matter, and bring it into general operation as far as possible. Postal-cards have been sent to various persons along the line, with questions in regard to the practical working of the system, which are answered and sent in at the end of every week; and we find, that, on the average, eighty per cent of the predictions are verified.

PLAN FOR A STATE WEATHER SERVICE. By F. E. NIPHER of St. Louis, Mo.

[ABSTRACT.]

WHILE a good many are accommodated by the weather-signals which Professor Mendenhall has already inaugurated, many live a distance from the railroad, and cannot be interested in a scheme which makes it necessary to travel eight or ten miles to learn about the weather, because they might be interested in a different kind of weather by the time they got home. The information might be most easily circulated by telegraphing from picket-stations to the westward. There might be a line of stations on the railroad north and south; and stations might be found necessary in Nebraska, which would give immediate warning to the central office whenever it began to rain at the station; and a code might be arranged, so as to give the idea of the operator as to the probable violence or duration of the rain. Of course it would be necessary to make special study of the general laws for the progress of summer rains. Supposing the information is telegraphed to the central station, the predictions can easily be made out as soon as the picket-stations could be reached, and a clear idea obtained as to the probable direction of the storm, and the time at which it would reach the different portions of the state. That information could be transmitted by the railway companies. Finally, we should make more intimate connection between these and private telegraph-lines which can be constructed by the persons who are to be served with the weather signals. This plan contemplates the erection of private telegraph-lines leading in from the country to the stations. Upon a twenty-mile line, which would be a frequent length in Missouri, ten farmers will have to pay for the erection of a couple of miles of wire, which can be put up for $30 a mile, and the instruments. Some person could be sent from the vicinity to the director of the service, and instructions given him in regard to the manner of operating the line and the management of the batteries. The cost of the line, therefore, to each farmer, would be, say, $75, which might be distributed over ten years. Mr. Nipher stated that in several localities the farmers will undertake it just as soon as the information can be furnished them. At the stations the lines could easily be made to terminate in the store of some merchant, who is anxious

to secure the trade of the people on the line. This can be done at once in Missouri. The only thing necessary is for the state to appropriate a small amount of money to supply the persons with instruments for observations, rain-gauges, etc. The two things necessary to make it successful are information as to rainfall, and time of beginning and ending of rains.

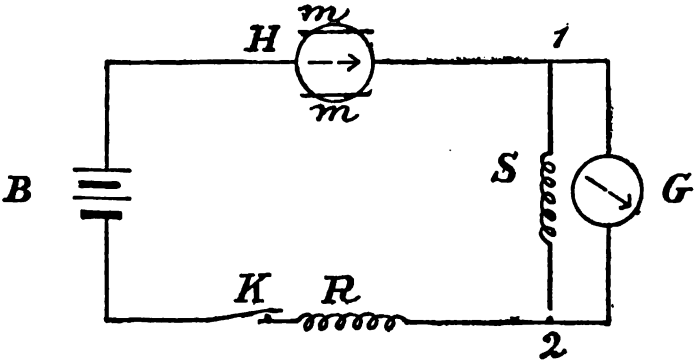
THE EXTENSION OF THE THEOREM OF THE VISIAL TO ROTARY OSCILLATION. By H. T. EDDY of Cincinnati, Ohio.

A METHOD FOR THE CALIBRATION OF A GALVANOMETER. By B. F. THOMAS of Columbia, Mo.

[ABSTRACT.]

A BATTERY of any sort is joined in circuit with a sensitive galvanoscope H, a galvanometer G, and any variable resistance R. When the circuit is closed at K, the current is so adjusted by varying R, as to give the highest desirable deflection of the galvanometer needle. The needle of H will be forced against the stops. By means of magnets m and m, the needle of H is brought back to zero. If these magnets and the galvanoscope be undisturbed, the original current strength will be indicated when the needle stands at zero, whatever changes may have been made in the circuit. If now the shunt S be connected at 1, 2, and the re-

FIG. 1.



sistance of the shunt is made equal to that of the galvanometer (previously determined), and the needle of *H* brought back to zero by increasing *R*, a new deflection of the galvanometer needle will be produced, the deflection being that due to a current of one-half the strength of the original current. By giving to S values equal to 8, 3, 2, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, etc., times the resistance of G, and bringing the needle of H to zero each time, deflections of G will result, due to currents whose strength are as 1, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{1}{5}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{2}$, etc. The curve is then plotted with deflections and current strengths as co-ordinates. Any desired number of points in the curve may be obtained by giving S the proper values. The calibration may be checked by making a new adjustment for the unit current, so that the deflection of G shall be about two-thirds the first deflection, and proceeding as above. Plotting the new values obtained, the curves will coincide if the work is correct. If it be found desirable, the battery may be exchanged for another during the determination.

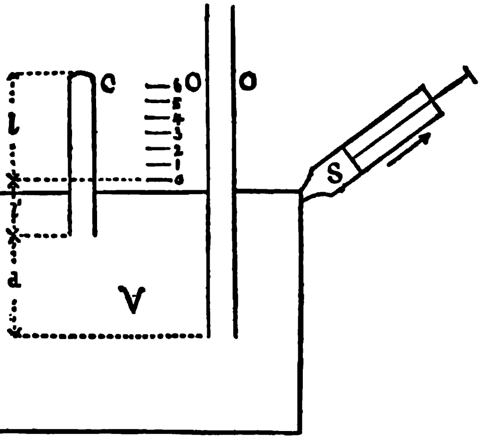
Two forms of Apparatus for Boyle's Law.

By B. F. Thomas of Columbia, Mo.

[ABSTRACT.]

THESE pieces are intended to enable one to adjust with accuracy and ease the mass of air to be experimented upon.

FIG. 1.



V is an iron cistern into which the open or pressure tube *O*, the closed tube *C*, and the reversible air-syringe *S* are screwed air-tight, and the cistern nearly filled with mercury. The syringe being connected for exhausting, and operated, air is withdrawn from *C*, until the mercury sinks to the bottom of the open tube, when air escapes from it, and rises through the mercury. No more air can be withdrawn from *C*. The mass of air remaining in *C* will evidently depend on the difference in depth of immersion of *C* and *O*. Let $d =$ this difference, and let it be required to find such a value of d as will permit just enough air to remain in *C* to fill it from the zero of the scale, when at atmospheric pressure H. Let $L =$ length of *C* from top to zero, and let $l' =$ the length from zero to the open end of *C*. If now the mass of air which will fill the length l at H be expanded to fill the length l', the pressure H' at the bottom of *C* by Boyle's law is $H' = \frac{H\,l}{l + l'}$.

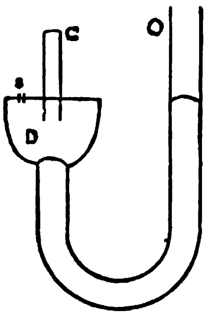
The pressure at the open end of $O = H$. The difference in pressure at the ends of *C* and *O* is that due to a column (d) of mercury. Hence $H' = H - d$.

Equating, $H - d = \frac{H\,l}{l + l'}$. $\therefore d = \frac{H\,l'}{l + l'}$.

On reversing the syringe, and forcing air in, the mercury will be found to rise and stand at zero in both tubes together. The demonstration is continued by forcing in more air.

A second form consists of two glass tubes connected by a strong rubber tube, and mounted on a stand with scales. The closed tube *C* is sealed into the screw-cover of an iron cistern *D*. Mercury being poured in, it will expel the air in *D*, and rise in an open screw-hole *S* in the cover. The hole being sealed by insertion of the screw, and *O* lowered, the air in *C* expands, filling *C* and *D*. On raising *O*, the mercury rises, and cuts off communication between *C* and *D*, preventing the return of some of the air. By making *D* of proper volume, the desired mass of air will remain in *C*. Let the volume of *C* above the zero $= V$. Let the entire volume of $C = V'$, and the volume of *D* above the open end of *C* $= V''$. Following the above steps it will be seen that a volume V' at H becomes a volume $V' + V''$ at H'; also that a volume

FIG. 2.



V at H becomes a volume V' at H'. Hence the proportion $V : V' :: V' : V' + V'', \therefore V'' = (V' - V)\frac{V'}{V}$. The use of the rubber tube is not new. This form is convenient because it answers for demonstrating the law at pressures above and below one atmosphere. Of course, for the latter, the air in D must be removed through S as at first.

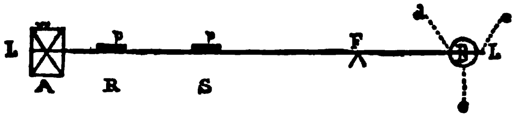
A Method of determining the Centre of Gravity of a Mass.

By B. F. Thomas of Columbia, Mo.

[ABSTRACT.]

A bar, $L\ L$, is balanced on a knife-edge, F, so as to form a very sensitive balance. The body, B, of mass, M, is placed with a marked point in contact with a fine point, d; and another body of mass, W, is placed at A, so as to nearly balance B. A small body of mass, p, is placed at S, to complete equilibrium. B is then rotated 180° horizontally, bringing its marked spot in contact with a second fixed point, e. Equilibrium is restored by placing p at R. The equations of moments in the two positions are, respectively,—

FIG. 1.



$$W \times AF + p \times SF = M\,(Fd + dc)\,;$$

(c being the centre of gravity); and

$$W \times AF + p \times RF = M\,(Fe - dc)\,;\ (ec = dc).$$

Subtracting the first equation from the second,—

$$p \times RS = M\,(de - 2\,cd)\,;$$

$$\therefore cd = \frac{de}{2} - \frac{p \times RS}{2\,M}\,;$$

cd is therefore the distance from the marked spot to a vertical plane containing the centre of gravity. Taking a second marked spot in the plane thus found, the operation is repeated, with the plane horizontal. This gives a second plane through the centre of gravity. A third operation, with the intersection of the two planes in the line de, locates the centre of gravity.

A NEW HELIOSTAT. By B. F. THOMAS of Columbia, Mo.

THE KINETIC THEORY OF THE SPECIFIC HEAT OF SOLIDS.[1] By H. T. EDDY of Cincinnati, Ohio.

[ABSTRACT.[1]]

THIS paper applies to molecules of solid bodies the same principles which the author has elsewhere applied to gaseous molecules as to the distribution of vibratory energy among the different degrees of freedom possible to the molecules of a solid; and in particular it attempts to show that if the chemical atoms of all substances are formed of equal and similar ultimate atoms, then systems of solids which are in thermal equilibrium by radiation will also be in thermal equilibrium when brought into contact, *i. e.*, when the equilibrium depends upon collisions of the molecules.

THE KINETIC THEORY OF MELTING AND BOILING[2]. By H. T. EDDY of Cincinnati, Ohio.

[ABSTRACT.]

THIS paper discusses at length the two kinds of vibratory motion which the molecule of a solid body may have, rotary and translatory. It demonstrates that the mean kinetic energy of such an oscillatory motion as is possible for a molecule of a solid reaches a maximum value which can only decrease, whether the amplitude of the oscillation be increased or diminished, and that the only way in which it is possible to increase the mean kinetic energy of this kind of motion is to impart sufficient additional energy to change the motion into one of complete rotation. By such a process, greater freedom of motion is given to the molecules, and a large amount of energy becomes potential. This is regarded as explaining the phenomenon of liquefaction.

It is next shown by an extended mathematical discussion of the cohesive forces and resistance to compression by which molecules hold each other at mean fixed distances, that the mean kinetic energy of the vibration of molecules about their mean positions also has a maximum value which can only be increased by removing them to such mutual distances that the cohesive forces no longer act. In this process a large amount of energy also becomes potential. This is regarded as the rationale of the phenomenon of vaporization.

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[1] This paper was published in full in Science, Sept. 28, 1883.

[2] The principal conclusions in this paper were published in the Franklin Inst. Journal for July, 1883.

It is further shown that on this theory it might very readily occur that the specific heat of a liquid should at first decrease and then increase, as Rowland has proved is the case with water, but that the specific heat could not at last decrease.

The cause of the relatively large specific heat of most liquids is treated. It is shown that the distribution of rotary velocities in free rotation such as the molecules of a liquid are supposed on this theory to have is such that the atoms of some small per cent of the molecules in any given mass must be torn asunder. What per cent of the liquid may be thus dissociated will depend upon the temperature and constitution of the liquid, it being smaller for the simpler liquids and increasing with the temperature. Electrolysis is an evidence of this action. Such dissociation sufficiently accounts for the generally high specific heats of liquids.

There is a general qualitative accordance of the theory with observed specific heats. A further confirmation of the theory is found in the clear explanation it affords of the existence of a critical temperature above which a vapor is incondensable by pressure alone; for when the mean kinetic energy of all the molecules of a liquid acquires a value greater than the maximum possible in a liquid state, the liquid is not only vaporized but necessarily becomes an incondensable gas and remains so.

REMARKS ON THE TRACINGS OF SELF-REGISTERING INSTRUMENTS, AND THE VALUE OF THE SIGNAL-SERVICE INDICATIONS FOR JUNE AND JULY, 1883.[1] By GUSTAVUS HINRICHS of Iowa City, Mo.

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[1] A short notice of this paper is given in SCIENCE, Aug. 31, 1883, p. 285.

The Utilization of the Sun's Rays for Warming and Ventilating Apartments. By E. S. Morse of Salem, Mass.

[ABSTRACT.]

The device consists mainly of a slaty surface, painted black, standing vertically upon a wall, outside the building, with flues to conduct warmed air to the inside. The slates are inserted in a groove, much as one might place glass in a frame. One made within the last year was three feet wide and eight long. It was placed where it received the sun's rays as directly as practicable. Its service was to warm a room used for a library. During an entire winter the room was thus made comfortable, except on a few of the coldest days. The current of air passing through it, when the sun's rays impinged directly upon it, was raised about 30°; it discharged 3,206 feet of warmed air in an hour. This was in the morning. At 11.45 the air of the apartment was raised 29°, with 3,326 cubic feet of air discharged; at 12.45, 29° and 4,119 feet; at 1.55, 24° and 3,062 feet; at 2.45, 20° and 1.299 feet. The room measured 20×14 and was ten feet high.

The apparatus works to most advantage in a room that is ventilated by an open chimney. But some very good results have been obtained in closed rooms. One was cited where the air, as it entered the room from the heater, was raised to nearly 40° above the outside temperature. In general, a difference of 30° to 35° can thus be secured during four or five working hours of the day.

Natural Snow Balls, or Snow Rollers. By Samuel Hart of Hartford, Conn.

[ABSTRACT.]

A more extended example of "Natural Snowballs" or "Snow Rollers" than has yet been recorded occurred in Connecticut and southern Massachusetts in February last. Snow had fallen on the 18th, the level surface of which had been covered with a crust from the freezing of a drizzling rain. On the evening of the 20th, a light but damp snow fell quietly upon this smooth crust, and afterwards a light wind sprang up from the south. In the morning the college campus, the city park, and open lots in every direction were strewn with masses of snow. Some were spherical, varying in diameter from one to nine or more inches; but most had the appearance of large old-fashioned muffs, of the shape of cylinders with a conical depression at each end reaching nearly or quite to the middle. The largest of these latter which I measured was eighteen inches long and twelve inches in diameter; and some were reported much larger. The path which each left behind it showed how it had been formed, a pellet of snow having been started by the wind and having gained not only in diameter but also in length as it rolled; so that a long isosceles triangle of snow was rolled up from its vertex, producing the peculiar shape just described. In many cases the path could be traced for twenty-five or thirty feet, and in some cases, it was reported, for sixty feet. The path of the round balls was, of course, of nearly the same width throughout. The direction of most of the paths was straight; but in some cases the snow had been carried by the wind in different directions, while occasionally it had been blown up a decided incline. The snow was not at all closely packed; it yielded to the least touch, and it was impossible to move one of the masses without breaking it.

The rollers were seen on the Connecticut river and along its banks for some forty miles below Hartford, as far north as Massachusetts, for some distance on the east, and as far west as the Housatonic valley. It seems quite within limits to say that they were spread thickly over all the open and level ground within more than 2500 square miles, and that they might have been numbered by millions.

TORNADO AT RACINE, WISCONSIN[1]. By P. R. HOY of Racine, Wis.

[ABSTRACT.]

FRIDAY, May 18, 1883, was a pleasant day; a slight shower of rain occurred soon after noon. At 6.45 there was almost continuous flashes of lightning, in the west, passing from a dark cloud coming from the northwest to a lighter colored, and less elevated cloud moving rapidly from the southwest, accompanied with thunder; these two clouds appeared to meet, producing a whirling, boiling tumultuous action, accompanied by a *roar* not unlike the passing of a heavy train of cars over a long bridge. The tornado passed the track of the C. and N. W. railroad, at just 7.10. The direction of the tornado was north of east; in six miles the storm advanced north 1¾ miles. Rain and hail fell over an area, commencing 1½ miles north of the tornado, and extending two miles north. Some of the hailstones were unusually large. Caledonia Station, on the St. Paul R. R., two miles north of the tornado track, was struck by lightning at 6.45 P. M. at the time of the hail-storm. The temperature of the atmosphere, just before the tornado struck Racine, was 64°. Fifteen minutes after it had passed the thermometer indicated 74°; this rise of 10° in so short a time was remarkable at that late hour. The barometer marked 29 inches at an elevation of 645 feet above the sea-level. There was only a pleasant breeze, rendering it comfortable to be out-of-doors at the time the tornado was passing 1½ miles north of us. No rain fell in Racine, at the time, or for three days after the storm. My attention was called to a peculiar odor that pervaded the atmosphere soon after the tornado. This odor, which I then detected, was between sulphur and phosphorus; undoubtedly, it was ozone, resulting from electrical disturbance that filled the atmosphere at this time. It is proved that the onward motion was not more than thirty miles an hour, many families gained their cellars, and safety, after they had witnessed the destruction of houses situated not over twenty-five rods distant.

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[1] The paper was accompanied by maps and photographs, illustrating facts relating to the tornado. For the discussion following the reading of this paper, see SCIENCE for Aug. 31, p. 281.

The gyratory motion, from right to left, is the principal, if not the only, factor in accounting for the destruction that followed in the immediate track of the storm.

The width of the tornado path was from ten to twenty rods, occasionally, however, much narrower; it was not straight but oscillatory, the variations being from 300 to 600 feet on either side of a straight line. Everything was thrown towards the centre of the track; not unfrequently objects on the outer borders were thrown at an acute angle west of either south or north, against the direction of the advancing storm. Still further, just beyond the line of total destruction, houses were pulled partly off their foundations; in these instances, the windows were thrown out on the storm side towards the path of the tornado.

There were ten persons killed, all of whom save two lived near where the storm struck the city; they were surprised, not being aware of its approach. All were saved who gained their cellars, with one exception, that of a family living in a *brick veneered house*, the brick falling into the exposed cellar, killing two persons.

When the tornado passed over the lake, the sight was grand indeed, one long to be remembered; one, probably never before seen, for this is the only instance known of a tornado reaching Lake Michigan.

The water was whirled up at a great height, from 150 to 300 feet; the cloud would occasionally rise when the great volume of water would tumble back only to be caught up in a moment with redoubled force. While the water was being forced up in the centre by the whirling vortex, it was thrown over on the outside, thus forming irregularly circular cascades, swashing, splashing and foaming, in the wildest confusion. There were five or six accessary "water spouts," some of which, apparently, revolved around the "primary whirl;" one of these passed on the lake, at least one half mile south of the main storm. These accessaries were by no means insignificant either in size or force; only by comparison were they small. I saw two flashes of lightning in the tornado-cloud, while it was passing over the lake, but heard no thunder.

The storm left the lake covered with debris, for the distance of one mile from the shore. There was no well-formed funnel-shaped cloud; it was multiform, and resembled, somewhat, the smoke from a large factory chimney, when the condition of the atmosphere is such that the smoke flows over with whirling motion towards the ground. Imagine one hundred such columns, closely packed together, minus the chimneys, with volumes of light-colored smoke rising rapidly up in the centre with a gyratory motion, and you have a feeble representation of the Racine tornado cloud.

THE TORNADO OF MAY 18, 1883. By JNO. H. FRICK of Warrenton, Mo.

SECTION C.

CHEMISTRY

PAPERS READ.

REPORT OF THE COMMITTEE ON INDEXING THE LITERATURE OF CHEMICAL ELEMENTS.

THE undersigned, a committee appointed at the Montreal meeting of the American Association for the Advancement of Science, "to devise and inaugurate a plan for the proper indexing of the literature of the chemical elements," respectfully submit the following report.

The members have conferred with each other orally and by correspondence. Several plans have been suggested, and their merits discussed. Three methods of collecting material for the indexes may be named:—

1. Reviewing the Catalogue of Scientific Papers published by the Royal Society (8 vols. 4to).

2. Indexing special journals by different individuals, and collating the matter.

3. The independent plan, whereby each chemist indexes all the journals available to him with reference to a given element, in which he is presumably especially interested.

Each of these schemes is open to objections, and has its difficulties. The first would necessitate an enormous amount of clerical labor, for which volunteers would scarcely be secured; besides, data previous to 1800 could not be obtained from this catalogue.

The second involves, also, securing a large number of self-sacrificing volunteers; and both plans would require a vast amount of editorial work on the part of this committee.

The third plan seems, to a majority of the committee, the only feasible one at present. On the independent plan, seven indexes have already been compiled.

The best arrangement of material has also been considered; and here again a threefold problem occurs:—

1. Chronologically.
2. Alphabetically, by authors.
3. Topically.

The committee do not venture to dictate to independent workers, but recommend the chronological arrangement, with the understanding that a topical index accompany each monograph.

The best channel of publication has also been considered by the committee. All the indexes hitherto published have been printed in the Annals of the New York Academy of Sciences; and the Academy has generously offered, through its officers, to continue its good work. The Smithsonian Institution further agrees to distribute, free of expense, all circulars and documents in furtherance of this undertaking; an offer which is of greatest importance, and for which this committee expresses sincere thanks.

Since the appointment of the committee, Mr. Webb's Index to the Literature of Electrolysis has been published in the Annals of the New York Academy of Sciences; and several chemists have expressed a willingness to coöperate in the proposed undertaking. Prof. R. B. Warder of Cincinnati has promised an index to the literature of the velocity of chemical reactions; Dr. Henry Leffmann of Philadelphia proposes to index the important element arsenic, and Professor C. E. Munroe offers a Bibliography of Explosives.

H. C. BOLTON, *Chairman;* IRA REMSEN; F. W. CLARKE; A. R. LEEDS; A. A. JULIEN.

ON THE SUB-AQUEOUS DISSOCIATION OF CERTAIN SALTS. By JOHN W. LANGLEY and CHAS. K. McGEE of Ann Arbor, Mich.

IN his "Leçons sur la Dissociation" (Paris Chemical Society, March and April, 1864), Sainte-Claire Deville directs the attention of chemists to certain properties which salts in solution display: such as contraction of volume, the evolution or absorption of heat, variations in density, etc. But he particularly calls attention to some of the chemical changes which the salt appears to undergo by the mere fact of solution, changes which increase in amount with the quantity of water added and which cause the salt to act finally, rather like a mere mixture of its components, than like a compound such as it is known to be in its solid state.

Deville considers that there is no absolute distinction between solution and chemical union; the difference is one of degree rather than one of kind, for out of the possible unions and rearrangements between atoms and molecules which are embraced in the term "chemical changes," some are synthetic, some analytic, and some partake of both processes. Now in Deville's mind solution is of the last order; synthetic and analytic changes always occur in solution, only the latter is the predominant one, so that there is absorption of heat accompanying contraction of volume instead of evolution of heat as would be expected *á priori;* and in addition a constant tendency for the salt to be changed to simpler components.

Thus, when a salt is dissolved in water there is a combination of the solid with the whole of the water, but at the same time the salt undergoes a species of vaporization, for its molecules are distributed through the entire mass of the water, and consequently occupy a larger amount of space, just as a vapor would do if produced by the action of heat. The heat units necessary to perform this vaporization are taken from surrounding bodies, and are, therefore, virtually unlimited in amount; now as the heat absorbed increases with the degree of dilution, the quantity will finally become sufficient to completely dissociate and resolve, even into its elements, any salt when dissolved in a sufficiently large body of a neutral solvent such as water.

We have here briefly recalled Deville's theory of dissociation as the necessary result of solution, because we have made some experiments in one limited direction which were suggested to us by the theory in question.

Every chemist is familiar with the classic researches of Graham on diffusion, and of the admirable work of Berthelot on thermo-chemistry, and of the bearing on Deville's theory of solution. We have applied the theory to account for the changes in the degree of acidity which some salts display when dissolved.

It is a well-known fact that many salts which are neutral by theory are not neutral to litmus paper; for instance, cupric sulphate in solution is acid to litmus, and it will remain acid however much potassic hydrate may be added to it, so long as all the copper is not precipitated. It is also a well-known elementary fact that certain salts whose metals form very feeble bases are decomposed with the formation of basic precipitates, while the greater part of the acid is found in the liquid, for example, bismuth and antimony chlorides.

If it be true that salts tend to dissociate when dissolved, and are actually decomposed when sufficiently diluted, we should expect that they would break up along the lines of least resistance into simpler molecules first, and ultimately into free atoms. Now when water is present we may take three views of the condition of the salt. We may suppose it to be merely attached to the water by a species of physical adhesion, the salt nucleus remaining unchanged when the quantity of water is very small; or second, we may consider the water and the salt to be united into a new group which acts as a compound molecule, so long as the body of the solvent is so small that the amount of diffusion is negligeable. Then for sodic sulphate in two molecules of water we should have, for the first case [Na_2SO_4 ,$2H_2O$], and for the second [$2(NaOH)$, H_2SO_4], where the comma indicates a molecular, as distinguished from an atomic, union.

Finally, we may decline to theorize about the initial state of the salt and water, and consider that we have a certain quantity of different kinds of matter held momentarily in equilibrium by internal and external forces, but which will form definite associations with each other whenever the external forces change. On this view the above symbols might be written [$Na_2H_4SO_6$], and we should have no theory as to whether the Na was combined with H, with S, or with SO_4.

Researches in thermo-chemistry tell us that the heat of combination between H_2SO_4 and $2NaOH$ is less than the heat of formation of sodium hydrate starting with metallic sodium and water, or of sulphuric acid starting with SO_3 and water. The line of least resistance in the group $Na_2H_4SO_6$ probably, therefore, passes through the comma in the symbol $2(NaOH)$, H_2SO_4 and indicates that the first stage of dissociation will be into free sodium hydrate and free sulphuric acid. The general theory of dissociation shows also that this change will be only partial for all finite ratios between the quantities of the salt and the water; it should first become apparent as traces, and proceed under increasing dilution to amounts of free acid which could be quantitatively estimated.

When antimony or bismuth chlorides are diluted, we find the metals separating from the liquid as insoluble basic chlorides, whose action on litmus paper is however very slight; that is, the basic element has very feeble alkaline characters. On the other hand, the power to redden litmus possessed by the mineral acids H_2SO_4, HCl and HNO_3 is very marked and is of nearly the same intensity in the three, while the corresponding power to blue the litmus possessed by the hydrates varies greatly. It is very strong in the alkali metals, but is quite feeble in metallic hydrates such as those of zinc, iron and copper. If then a salt actually separates partially into free acid and free base when dissolved, and if the power of the base to turn litmus blue is not the exact quantitative counterpart of the acid to turn it red, there must necessarily occur, with differing degrees of dilution of the solution, differing powers of coloring litmus possessed by any salt, even if it were perfectly neutral, both by composition and to litmus, when it was in the solid state.

To test this hypothesis we have used the following method:

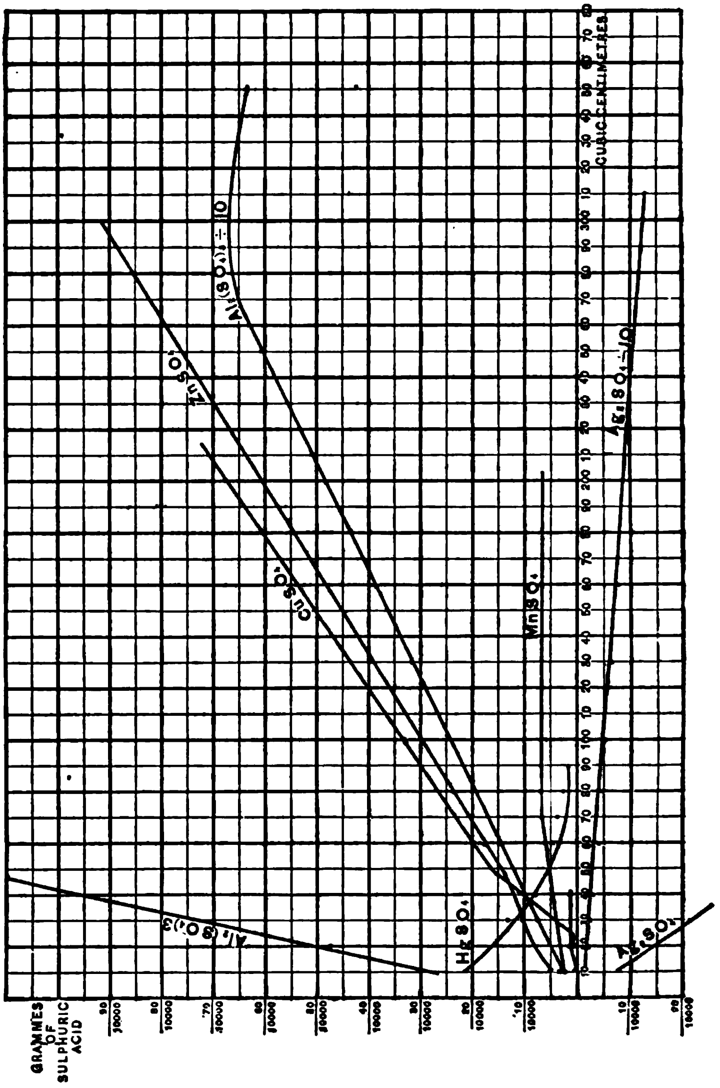
Chemically pure salts were obtained in saturated solutions. There was also prepared a standard solution of pure sulphuric acid containing one gramme of acid to the litre of solution. And, finally, a standard solution of litmus was provided which was made as nearly neutral as possible.

Into a series of four inch test tubes were introduced $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, etc., c.c. of this standard acid and each of them then diluted with the litmus solution to ten c.c. The tubes consequently gave a series of red tints from neutral purple to decided red. Tube No. 1 contained $\frac{1}{10000}$, gramme of H_2SO_4; No. 2, $\frac{2}{10000}$; No. 3, $\frac{3}{10000}$, etc. These quantities were added to amounts of litmus solution which were nearly the same, being the quantity necessary to complete the volume to 10 c.c. (The litmus actually ranged from 9 c.c. to 9.9 c.c.)

Saturated solutions of each of the following salts were examined:

$ZnSO_4$, $CuSO_4$, $PbSO_4$, $AgSO_4$, $CaSO_4$, Na_2SO , $FeSO_4$, $HgSO_4$, $MnSO_4$, $Al_2(SO_4)_3$, $Fe_2(SO_4)_3$, $ZnCl_2$.

In each case 1 c.c. of the salt solution was introduced with a test tube and enough litmus solution added to make 10 c.c. The resulting color was compared with the scale of colors made with



the sulphuric acid by looking across the test tubes. 10 c.c. of litmus solution were now added and the resulting color again recorded in terms of the color scale. Successive dilutions up to 100, and in some cases up to 400 c.c., were made and the colors noted.

Let 1 c.c. of some salt solution be diluted with litmus to 10 c.c., and let the color produced be the same as that of tube No. 7. Evidently $\frac{7}{10000}$ gramme of H_2SO_4 is equivalent to the salt in its reddening effect on litmus solution. If now we dilute the salt solution to 40 c.c. and find that it gives the same tint as tube No. 5, it is evident that $\frac{20}{10000}$ gramme H_2SO_4 would be required to produce the same reddening effect as that produced by the salt; for $\frac{5}{10000}$ gramme H_2SO_4 is needed to make 10 c.c. equally red, and consequently the 40 c.c. of the diluted salt represent four times the reddening effect of $\frac{5}{10000}$ gramme of acid in 10 c.c. of litmus. In this way the number of $\frac{1}{10000}$ gramme of H_2SO_4 required to produce the same reddening effect for various dilutions of each salt is found and represented as ordinates in the curve.

In the diagram, the numbers on the horizontal axis indicate the number of cubic centimetres to which one centimetre of salt solution has been diluted with the litmus solution. The numbers on the vertical axis indicate the number of $\frac{1}{10000}$ grammes H_2SO_4 which could be substituted in place of the salt in any given case and have the same reddening effect.

$CaSO_4$ and Na_2SO_4 acted like neutral salts and did not change the litmus solution, either with low or high degrees of dilution.

Ag_2SO_4 was the only salt found to change the solution *blue* instead of red.

$Fe_2(SO_4)_3$ and $FeSO_4$ gave no reliable results on account of a dirty precipitate which prevented all color tests. Both, however, made the litmus distinctly red, and $Fe_2(SO_4)_3$ seemed to be as acid as $Al_2(SO_4)_3$.

$HgSO_4$ was made by drying the crystals of $HgSO_4$ in free H_2SO_4 upon filter paper and dissolving as much as possible in water. After the basic mercuric sulphate had settled out, the clear solution above was used in the test. Probably the curve is simply that of free H_2SO_4.

When largely diluted $Al_2(SO_4)_3$ gave a precipitate which interfered somewhat with the tests. The same is true of $CuSO_4$. $ZnCl_2$ could not be examined quantitatively for the same reason, although it reddened the litmus.

Much difficulty was experienced at first from the fact that the various color tubes gradually changed back to a neutral tint after standing a day or two. If a tube, to which acid had just been added and mixed by stirring, were strongly shaken for one or two minutes, it would be found to have changed sensibly towards a blue. Owing to this instability of color the standard tints were prepared afresh every day for each day's work, and the amount of stirring was made as nearly as possible alike for all experiments.

This apparent loss of acid, shown by a real loss of red coloration, is probably due to oxidation which seems to regenerate the blue color. By working with freshly boiled solutions and making the mixtures under a film of paraffine oil, the change of color was rendered much less, and, therefore, we have little doubt that the complete exclusion of dissolved oxygen would have entirely prevented the change of tint.

It is perhaps hardly legitimate to draw general conclusions from a limited number of experiments, especially of work like this, where there are several unknown factors, such as the possible differential attraction of the coloring matter for the several bases, etc. Still, taking the results as they stand, this provisional conclusion may be drawn.

First. Sulphates of the alkali metals, except silver, seem to be strictly neutral under all degrees of dilution.

Second. Sulphates of the RSO_4 type, where R is a dyad metal, show an amount of dissociation which is in proportion to the degree of dilution, and hence their curves are represented by a nearly straight line.

Third. Aluminic sulphate and other double triads are not neutral when concentrated. When diluted they rapidly become strongly acid, and when the dilution exceeds a certain limit they seem to lose acid at a decreasing rate. Hence their curves are not uniformly straight lines.

SUGGESTIONS FOR COMPUTING THE SPEED OF CHEMICAL REACTIONS.

By ROBT. B. WARDER of North Bend, Ohio.

[ABSTRACT.]

THIS paper urges a thorough discussion of data upon the subject indicated in its title, for the following reasons: 1. To discover and investigate the causes of certain discrepancies between published observations and current theories. 2. To obtain more definite information as to the nature of certain reactions and the conditions determining their speed. 3. To afford numerical data for a fuller study of relations between the speed of reactions and other physical constants. 4. To suggest fruitful lines for further research in chemical dynamics.

As instances of the need of such discussion, in the determinations by Professor Menschutkin, of the speed and limits of the etherification of the several alcohols and acids, numbers are given for the "initial speed" of reaction, which express the average speed during the first hour, and are not proportional to speeds during the first minute. Prof. L. Meyer in his *Dynamik der Atomen* passes very lightly over both the theory and the observations of speed during a reaction.

The prevalent theory of the action of mass is expressed by the equation, $\frac{du}{dt} = kuv$, in which the differential expresses the rate of change in any substance, u and v represent the masses taking part in the change, and k is a constant. Observations by Ostwald and others indicate that some modifications of this theory are needed. Determinations of the speed of reaction require special care, both to measure time in relation to mass, and to control temperature and other conditions. The chemical section of the Ohio Mechanics' Institute has recently undertaken some work of the sort, and invites coöperation.[1]

The following provisional system of units is suggested: for volume, one cc.; for mass, the chemical equivalent expressed in mg.; and for time, one hour. The unit of speed would be the transformation of unit of each active body per unit of volume and time. Possibly the comparison of the constants of speed or of chemical affinity with those of heat, electricity, etc., could be better made from the unit of one second or 1,000 seconds. At least two observations of time and two of mass are required; and preferably several, to determine the limits of error. Determinations which do not accord with the hypothesis that the speed and the product of active masses vary in the same ratio, need special investigation. In reciprocal reactions, some of the ratios derived from observations on chemical equilibrium, may be combined with constants of speed already determined, to deduce other constants. By bringing all the facts into systematic order, these data can be made of use for comparison in other fields of physical science.

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[1] Sci. Proc. O. M. I. **2**, 51.

For the convenience of those who may be interested, a table is here given of some of the papers containing or discussing determinations of speed. References are given to (JB.) Jahresbericht der Chemie; (Ber.) Berichte deut. chem. Gesell.; (C. S. J.) Journal of the Chemical Society; (P. M.) Philosophical Magazine; (J.p. C.) Journal für prak. Chemie; (A. C. J.) American Chemical Journal; (Sci. Proc.) Scientific Proceedings of the Ohio Mechanics' Institute; and (I. A. R.) Indiana Agricultural Report. Some of the papers referred to have only an indirect bearing on the speed of reactions. They are grouped according to author and subject, and placed as nearly in historical order as consistent with this arrangement.

Wittwer	JB. 1855, 172, 174	Decomposition of water by chlorine.
Bunsen (and Roscoe)	" 1855, 173	H + Cl = HCl under the influence of light. "Photochemical induction" is discussed.
	" 1856, 185	
	" 1857, 37, 43, 49	
	" 1859, 32	
A. Baeyer	" 1857, 48	Br and milk sugar.
Draper	" 1857, 51	Reduction of ferric oxalate.
Berthelot (and St. Gilles)	" 1861, 591	Etherification, etc.
	" 1862, 386	
	" 1863, 458, 463	
	" 1877, 25, 88, 89	
	" 1878, 10, 14, 15, 513	
	" 1880, 83, 85	
van't Hoff	Ber. **10**, 669	" "
Menschutkin	JB. 1877, 321	" "
	" 1878, 513, 538	
	" 1879, 313, 314	
	" 1880, 381, 600, 752, 816	
	" 1881, 15	
	Ber. **15**, 162, 248, 721, 1445, 1572	
	" **15**, 1615, 2237, 2502	Acetanilide. [Compare L. Meyer, Ber. **15**, 1977.]
	" **15**, 2512, 2738	Tertiary amyl acetate. [Compare Warder, Sci. Proc. **2**, 2.]

Harcourt (and **Esson**)	JB.	1864, 9	Oxidation of $C_2 H_2 O_4$ by $K_2 Mn_2 O_8$ and of HI_2 by $H_2 O_2$.
	"	1866, 10	
	"	1867, 28	
Miss Reed	"	1875, 14	
Guldberg and **Waage**	"	1866, 15	General theory, with numerous illustrations and deductions.
	"	1879, 22	
Gladstone (and **Tribe**)	"	1871, 13, 15, 16	Replacement of metal in salts.
	"	1875, 14	Metathesis of salts.
Lemoine	"	1871, 117	
	"	1875, 92	H + I = HI and the reverse.
Buchanan	"	1871, 115, also	Halogens, and carbon compounds. [Compare Urech.]
	Ber.	**4**, 863	
Jackson	JB.	1879, 340	
	"	1880, 482	
	A.C.J.	**3**, 252	
G. C. Thompson	JB.	1879, 594	
Hell (and **Urech**)	"	1880, 753	
	Ber.	**15**, 273, 987	
	"	**16**, 1144	
Urech	JB.	1880, 386, 697	Br etc. Isobutyl aldehyde.
	"	1880, 1021	Inversion of cane sugar. [Compare Warder.]
	Ber.	**15**, 2130, 2457	
	"	**16**, 762	
	"	**14**, 340	
	"	**15**, 2685	$Cu S O_4$ and glucose.
Leeds	JB.	1876, 165, 166	Reduction of $Ag NO_3$.
Lunge	"	1876, 13	Action retarded by indifferent bodies. [Compare Hood.]
Dunn	"	1877, 30	
Boguski and **Kajander**	"	1876, 12	Solution of $Ca CO_3$ etc. in acids.
Kajander	"	1880, 11	
	"	1881, 24, 211	
Pawlewski	"	1880, 11	
Ostwald	"	1875, 11	$Bi Cl_3 + H_2 O$. [Compare Muir.]
	"	1876, 23	Volume-chemical studies
	"	1877, 28, 1033	
	"	1878, 27	
	"	1879, 24, 25	
	"	1880, 8	
	"	1881, 11, 12	
	J.p.C.	[2] **25**, 1, 452	
	"	[2] **26**, 384	Decomposition of acetamide. Abstract in Sci. Proc. **2**, 44.
	"	[2] **27**, 1	
Muir (and others)	JB.	1877, 19, 27	
	"	1879, 21	$Bi Cl_3 + H_2 O$. [Compare Ostwald.]
	"	1880, 10	
	Ber.	**14**, 858, 860	
	"	**15**, 394, 395, 921, 958	
	C.S.J.	**39**, 21	
Wright (and others)	JB.	1877, 91	
	"	1878, 232, 234	
	"	1879, 27	
	"	1880, 7, 85, 151	Chemical affinity and electromotive force.
	"	1881, 91	

Hood	JB. 1878, 16	
	" 1879, 21	
	P.M. [5] **13**, 419	Retarding influence of salts. [Compare Lunge and Dunn.]
Mills (and others)	P.R.S. **28**, 268, 270, 273, or	
	C.S.J. **38**, 437, 438, 458	
Potilitzin	JB. 1879, 29	
	" 1881, 12, 15	
	Ber. **15**, 918, 5099	Metathesis. [Compare Beketow.]
Beketow	JB. 1881, 14	
	Ber. **16**, 775	
Villiers	JB. 1880, 381, 595	Etherification of mineral acids.
Beamer and **Clarke**	" 1880, 906, or A.C.J. **2**, 329	Decomposition of organic salts by heat.
Warder	Ber. **14**, 1361	$C_2H_5\cdot O\cdot C_2H_3O + NaOH$.
	A.C.J. **3**, 55, 340	
	Proc. A.A.A.S. **30**, 70	
	Sci. Proc. **1**, 105, 166, 167	
	" **2**, 2,	
	Science **2**, 176	Dissociation of brass.
Riban	JB. 1881, 665, 666	Decomp. of formates and acetates.
Huston	I.A.R. 1882, 230	Citrate on reverted phosphate. See Sci. Pro. **2**, 134.
Calm	Ber. **15**, 609	Naphtylamine and dinaphtylamine.
Benz	" **16**, 8	
Spindler	" **16**, 1252	Nitrification of benzol.

AMERICAN BUTTERS AND THEIR ADULTERATIONS.

By H. W. WILEY of Washington, D. C.

[ABSTRACT.]

A SERIES of elaborate experiments and analyses of various samples of butter, oleomargarine, tallow, and lard, has been made by the author. The paper, which will be printed in full in the report of the Commissioner of the United States Department of Agriculture, contained a description of the methods employed by the author. He takes a weighed quantity of the butter, puts it in a sand-bath, and dries for two hours at 100°. The curd or caseine is determined by ignition: five grammes are used for the purpose. Dry combustion in a tube is difficult and unsatisfactory: he therefore uses the moist-combustion method, with permanganate and nesslerizing. The amount of salt he considers important. It is usually determined by ignition, and weighing the residue; but he found that so much chlorine was thereby lost, that the result was not trustworthy. He washes the butter by shaking it in a separating funnel with hot water, and then determines the chlorine with standard silver nitrate and potassium bichromate as an indicator.

Several novelties were devised for these analyses. One is for ascertaining the melting-point. The butter is packed in a U-shaped tube, of which one leg is longer than the other. The tube is placed upright in a vessel containing sufficient mercury to overflow the top of the tube. This vessel is placed in another containing water, and heat is applied beneath. The water, heated, in turn heats the mercury surrounding the tube, until the contents of the tube are melted. As soon as the melting takes place, the melted material leaves the tube, and floats on the surface of the mercury. Another method consisted in laying platinum wires upon the sample of butter, etc., heating the wires, and noting the heat required to cause them to disappear by sinking into the sample. These methods determined not only the melting-point of samples of butter, oleomargarine, tallow, and lard, but also of the fatty acids. But the variations in the melting point of genuine butter are so wide, that no certain conclusion can be arrived at by comparison with melting-point of oleomargarine, etc., to test the question of genuineness. Thus it was found that first-rate butter from an Alderney cow at one time, owing to special feeding, had a higher melting-point than oleomargarine; while a few weeks later, with different food, the same cow supplied milk from which was made butter with a lower melting-point than oleomargarine.

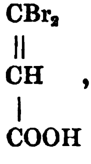
In regard to other tests, concerning which full details were given in the paper, it may be briefly stated, that, as a general rule, the amount of caseine present in pure butter is much greater than in oleomargarine. The specific gravity of genuine butter is higher. The saturation coëfficient for the insoluble acids in the genuine butter is low, in the imitations it is high. The author placed more reliance on tests for saturation coëfficient than on other methods. The soluble fatty acids in pure butter range from three to five per cent; while in oleomargarine, tallow, etc., they are either absent, or show a mere trace. The author also called attention to polarization tests. The genuine butter gives a uniform field in polarized light: oleomargarine gives a field with mottled and crystalline structure. He had made no analysis of butter known or suspected to be adulterated by mixture. He considered it unwise to decide the question of genuineness from any one of the constituents or conditions of a sample; believing that all the different tests should be brought to bear. He presented elaborate tables of analyses of different kinds of butter, etc.; specifying for each the place of purchase, name sold by, price, color, percentage of water, of caseine, of salt, specific gravity at 40°, melting and solidifying points, percentage of soluble and of insoluble acids, and the melting and solidifying points and the saturation equivalent of the insoluble acids.

ON γ-DICHLORDIBROMPROPIONIC AND γ-DICHLORBROMACRYLIC ACIDS.

By C. F. MABERY of Cleveland, Ohio, and H. H. NICHOLSON of Lincoln, Neb.

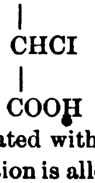
[ABSTRACT.]

WHEN dry chlorine is passed through β- dibromacrylic acid, the reaction is easily accomplished, and the product may be purified without difficulty by crystallization from carbonic disulphide. This acid is very sparingly soluble in water, more soluble in hot than in cold carbonic disulphide and chloroform. It melts at 100°. Its salts were carefully studied, but the silver salt was found so unstable that it could not be prepared in a state of purity. Since β-dibromacrylic acid has, without doubt, the form



$$\begin{array}{l} \mathrm{CBr_2} \\ \| \\ \mathrm{CH} \\ | \\ \mathrm{COOH} \end{array}$$

, the chlorine addition product would have the form



$$\begin{array}{l} \mathrm{CBr_2Cl} \\ | \\ \mathrm{CHCl} \\ | \\ \mathrm{COOH} \end{array}$$

. This acid is entirely decomposed when heated with an excess of any alkaline hydrate. If, however, the reaction is allowed to progress in the cold, keeping the hydrate in slight excess, the elements of hydrobromic acid are easily removed, with the formation of the corresponding dichlorbromacrylic acid. In order to distinguish this from two other products which have already been obtained, it will be called the γ-acid. It is prepared by the action of baric hydrate upon γ-dichlordibrompropionic acid, and the reaction proceeds so rapidly that it is difficult to keep the solution alkaline. Upon acidifying the baric hydrate solution with hydrochloric acid, γ-dichlorbromacrylic acid is precipitated partly as a crystalline solid, and is easily purified by crystallization from hot water. It is sparingly soluble in cold, readily in hot water, and readily soluble in alcohol, ether, carbonic disulphide, and chloroform. It crystallizes in pearly white scales, which melt at 78° to 80°. For further identification, the acid was analyzed, and its salts submitted to careful study.

ON THE FORMATION AND CONSTITUTION OF CHLORDIBROMACRYLIC ACID. By C. F. MABERY of Cleveland, Ohio, and RACHEL LLOYD of Louisville, Ky.

[ABSTRACT.]

IN a paper published in the Proceedings of the American Academy of Arts and Sciences, vol. xviii, p. 41, F. C. Robinson and one of us (C. F. M.) mentioned certain experiments in which we had tried to obtain an addition-product of brompropiolic acid with chlorinemonobromide. Although the results then obtained were not of an encouraging nature, it nevertheless seemed possible to obtain the desired product. On again taking up the subject the importance of obtaining chlorinemonobromide absolutely free from any excess of bromine was evident, inasmuch as a small quantity of free bromine would form tribromacrylic acid which could not be separated from the product by crystallization. After several trials it was found that, if chlorine was passed into bromine cooled to 0° until the latter was saturated, the product dissolved in chloroform and this solution saturated at 0°, no appreciable amount of tribromacrylic acid was formed when brompropiolic acid was added to it.

The acid was added slowly, keeping the chlorinemonobromide in excess, and this solution allowed to stand for some time. Evaporation of the chloroform left a solid residue which was easily purified by crystallization from hot water.

This substance melts at 104°, and is sparingly soluble in cold, more so in hot, water and carbonic disulphide, and quite soluble in chloroform. By slow evaporation it separates from a solution in carbonic disulphide in triclinic prisms which have been submitted to a careful study by Mr. O. W. Huntington.

The composition of this acid was determined by analysis, and it was further characterized by a study of its salts and a determination of its solubility in cold water.

The following salts were examined:

Argentic chlordibromacrylate, $AgC_3ClBr_2O_2$.
Rhombic plates, permanent in diffuse daylight.
Baric chlordibromacrylate, $Ba(C_3ClBr_2O_2)_2 \cdot 3H_2O$.
Calcic chlordibromacrylate, $Ca(C_3ClBr_2O_2)_2 \cdot 2\frac{1}{2}H_2O$.
Potassic chlordibromacrylate $KC_3ClBr_2O_2$.

The potassium salt was so deliquescent that its crystal water could not be determined. As a mean of two determinations the solubility of the acid in water at 20° was found to be expressed by the percentage 5.74.

Although previous experiments have shown (Proc. Amer. Acad., vol. xvii, 209, and vol. xviii, 45) that chlortribrompropionic acid, like other substituted propionic acids containing several halogen atoms, is readily decomposed when heated with baric hydrate in aqueous solution, when the action takes place in the cold in a slightly alkaline solution, the elements of hydrobromic acid are easily removed with the formation of a chlordibromacrylic acid. We, therefore, took advantage of this reaction to compare the resulting product with the addition-product above mentioned of brompropiolic acid. The object of this comparison was to establish the constitution of the chlordibromacrylic acid and thus determine the manner in which the elements of chlorinemonobromide united with brompropiolic acid. Chlortribromproprionic acid was allowed to stand for two days with baric hydrate in aqueous solution, sufficiently in excess to give a distinct alkaline reaction. On acidifying with hydrochloric acid, an oil was precipitated which soon solidified and more of the acid was obtained by extracting the solution with ether. The product was purified by crystallization from hot water in which it is much more soluble than in cold. From carbonic disulphide it crystallizes in rhombic prisms which melt at 99°. Its identity was established by analyses, and several of its salts were made and analyzed.

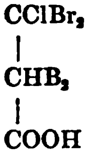
The silver salt proved to be so unstable that it could not be prepared in a form sufficiently pure for analysis.

Baric chlordibromacrylate, $Ba(C_3ClBr_2O_2)_2 \cdot 3H_2O$.
Calcic chlordibromacrylate, $Ca(C_3ClBr_2O_2)_2 \cdot 4H_2O$.
Potassic chlordibromacrylate, $KC_3ClBr_2O_2$.

The latter salt was so deliquescent, its water of crystallization could not be determined.

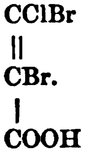
Two determinations of the solubility of this acid in water at 20° gave as a mean the percentage 2.60.

A difference in structure of the acid obtained by the two methods seems to be fully established by the different melting points, the wide difference in stability of the silver salts, and the different degrees of solubility in cold water. If the structure



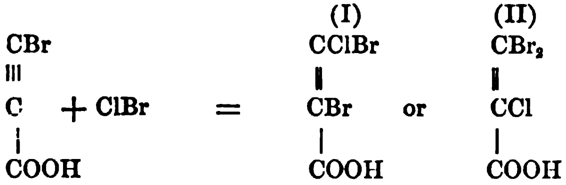
$$\begin{array}{l} CClBr_2 \\ | \\ CHB_2 \\ | \\ COOH \end{array}$$

assigned to chlortribromacrylic acid (Proc. Amer. Acad., vol. xviii, 46) is correct, eliminating the elements of hydrobromic acid must of necessity give an acid of the form



$$\begin{array}{l} CClBr \\ \| \\ CBr. \\ | \\ COOH \end{array}$$

Chlorinemonobromide can combine with brompropiolic acid in one of two ways:



$$\begin{array}{lcccccc} & & & (I) & & (II) \\ CBr & & & CClBr & & CBr_2 \\ \equiv & & & \| & & \| \\ C & + ClBr & = & CBr & or & CCl \\ | & & & | & & | \\ COOH & & & COOH & & COOH \end{array}$$

Brompropiolic acid. Chlordibromacrylic acid

The first form (1) is identical with that of the acid obtained from chlortribrompropionic.

Since the two products have been shown to be essentially different in their properties, the second form (II) seems more probable for the addition-product obtained with chlorinemonobromide. If this view is correct, it follows that in this reaction the weaker halogen unites with the terminal carbon atom

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ON ORTHOIODTOLUOLSULFONIC ACID. By C. F. MABERY of Cleveland, Ohio, and G. M. PALMER of Cambridge, Mass.

[ABSTRACT.]

WHEN sulphuric anhydride in small excess is added gradually to orthoiodtoluol, chemical reaction immediately ensues as shown by a rapid development of heat.

This reaction is probably expressed by the equation:

$$C_6H_4I_0CH_3 + SO_3 = C_6H_3I_0CH_3SO_2OH.$$

The resulting product was converted into the barium salt by treating it with an excess of baric carbonate after diluting with water, the solution filtered and concentrated by evaporation. On cooling, the barium salt crystallized in clusters of flat prisms or needles which were rather sparingly soluble in hot water, much less so in cold. It was purified by successive crystallizations from hot water and the water of crystallization determined at 100°. A determination of barium in the anhydrous salt gave the formula $Ba(C_7H_6ISO_3)_2$, or for the hydrous salt $Ba(C_7H_6ISO_3)_2.1\frac{1}{2}H_2O$. On evaporating the mother liquors of the several crystallizations a salt was deposited which differed somewhat in form from the first, but it was found to contain the same percentage of crystal water.

The free acid was prepared from the barium salt by precipitation with sulphuric acid. When the water was removed by evaporation, and the residue allowed to stand over sulphuric acid, the sulphonic acid became a thick syrupy liquid which refused to crystallize.

From the free acid the following salts were prepared.

Plumbic orthoiodtoluolsulphonate, $Pb(C_7H_6ISO_3).2H_2O$.

Calcic orthoiodtoluolsulphonate, $Ca(C_7H_6ISO_3).2\frac{1}{2}H_2O$.

Argentic orthoiodtoluolsulphonate, $AgC_7H_6ISO_3$.

We did not succeed in identifying a second acid, either in the mother liquors of the barium salt or in the free acid itself.

THE COMPOSITION OF AMERICAN WHEAT AND CORN. By CLIFFORD RICHARDSON of Washington, D. C.

[ABSTRACT.]

THIS paper gave an account of results obtained by the author in his work for the U. S. department of agriculture, and the paper will appear in full in the Report of the Commissioner of the Department. More than 200 analyses of wheat, and 100 of corn, have been made during the last two years under his supervision. It appears that while our wheats are of somewhat lighter weight, they contain less water, about the same ash, more oil, less fibre, and less albumen, than the foreign wheats. Among our wheats, only those from Colorado, Dakota, and Minnesota, equal the European in albuminoids and in size of grain. The wheats of the Atlantic states are poor in nitrogen. Corn, compared with wheat, contains twice as much oil, less starch, more water and fibre, and less of albuminoids. The following table gives a condensed statement of the analyses :—

AVERAGE COMPOSITION OF AMERICAN CORN (MAIZE).

	Weight of 100 kernels.	Water.	Ash.	Oil.	Carbhydrates.	Fibre.	Albuminoids.	Nitrogen.	Weight of largest 100 kernels.	Weight of smallest 100 kernels.	Highest albuminoids.	Lowest albuminoids.	No. of analyses.	No. of weights of 100 kernels.	Analyst.
	Grams.	*Pr. ct.*	*Pr.ct.*	*Pr.ct.*	*Pr. ct.*	*Pr.ct.*	*Pr. ct.*	*Pr.ct.*	*Grams.*	*Grams.*	*Pr. ct.*	*Pr. ct.*			
America	36.910	10.04	1.52	5.20	70.69	2.09	10.46	1.67	52.679	23.605	18.65	7.00	114	61	
Northern States ...	37.320	9.98	1.54	5.11	71.32	1.41	10.64	1.70	44.147	30.610	13.65	7.53	30	3	
South	....	8.96	1.37	4.94	72.06	1.72	10.95	1.75	....	....	11.03	10.88	2	......	
Middle West	32.457	12.33	1.43	4.97	68.16	2.22	10.80	1.70	....	....	12.00	9.29	15	1	
Far West	37.528	9.50	1.55	5.30	70.75	2.47	10.43	1.67	52.679	28.348	12.78	8.23	53	53	
Pacific slope	27.900	9.78	1.48	6.40	72.13	2.07	8.14	1.30	....	....	8.40	7.88	2	1	
Mexico	....	9.58	1.58	5.48	71.34	1.68	10.34	1.65	....	....	10.67	10.15	8	......	
New Hampshire ...	....	10.10	1.58	5.48	70.15	1.11	11.67	1.85	....	....	13.65	10.50	11	......	Department of Agriculture.
Vermont	....	8.64	1.45	5.63	72.76	1.38	10.14	1.62	....	....	....	....	1	......	Do.
Connecticut	....	10.84	1.54	4.70	70.98	1.46	10.48	1.69	....	....	11.63	9.19	13	......	S.W. Johnson, W.O. Atwater.
Pennsylvania	37.320	7.96	1.43	5.22	74.54	1.07	8.88	1.42	44.147	30.610	10.15	7.53	5	8	Department of Agriculture.
North Carolina	....	8.96	1.37	4.94	72.06	1.72	10.95	1.75	....	....	11.03	10.88	2	......	Johnson and Dept.
Kentucky	32.457	7.70	1.50	5.33	73.47	2.20	9.80	1.57	....	....	....	....	1	......	Department of Agriculture.
Tennessee	....	7.58	1.23	5.09	74.16	2.65	9.29	1.48	....	....	....	....	1	......	Do.
Indiana	....	11.29	1.28	4.87	70.16	1.90	10.50	1.68	....	....	....	....	1	......	R. C. Kedzie.
Michigan	....	13.20	1.45	4.05	67.05	2.21	11.14	1.78	....	....	12.00	9.88	12	......	Do.
Missouri	38.411	8.18	1.68	5.28	72.00	2.23	10.54	1.68	52.679	30.674	12.78	8.23	26	26	Department of Agriculture.
Kansas	33.971	11.94	1.49	4.87	69.39	2.16	10.15	1.62	36.687	28.348	10.68	9.10	6	6	Do.
Colorado	34.120	10.50	1.50	5.60	70.19	2.35	9.80	1.00	....	....	....	....	1	1	Do.
Texas	37.838	10.44	1.42	5.45	69.52	2.77	10.40	1.66	43.709	30.808	11.08	9.80	20	20	Do.
Oregon	....	9.25	1.46	7.08	73.07	1.26	7.88	1.26	....	....	....	....	1	......	Do.
Washington Terr..	27.900	10.30	1.50	5.73	71.19	2.88	8.40	1.34	....	....	....	....	1	......	Do.
Mexico	....	9.58	1.58	5.48	71.34	1.68	10.34	1.65	....	....	10.67	10.15	8	......	Do.

AVERAGE COMPOSITION OF AMERICAN WHEATS.

No. of analyses.		Weight of 100 grains.	Water.	Ash.	Oil.	Carbhydrates.	Fibre.	Albuminoids.	Nitrogen.	Heaviest 100 grains.	Lightest 100 grains.	Highest albuminoids.	Lowest albuminoids.
		Grams.	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Per cent.*	*Grams.*	*Grams.*	*Per ct.*	*Per ct.*
260	United States and Canada..........	3.638	10.27	1.84	2.16	71.98	1.80	11.95	1.91	5.924	1.830	17.15	8.05
108	Atlantic and Gulf States..........	3.464	10.42	1.75	2.17	72.61	1.72	11.33	1.81	5.079	1.830	15.58	9.45
47	The Middle West....................	3.607	10.51	1.76	2.01	71.67	1.90	12.15	1.94	4.902	2.138	16.63	10.15
97	West of the Mississippi............	3.806	10.01	1.99	2.22	71.12	1.87	12.76	2.04	5.924	2.561	17.15	10.15
8	The Pacific coast..................	5.044	9.74	1.84	2.08	76.18	1.56	8.60	1.37	(5.745)	(4.253)	9.47	8.05
6	Canada.............................	3.325	9.74	1 56	2.29	73.87	1.67	10.87	1.74	3.686	2.964	14.70	9.45
32	Pennsylvania.......................	3.373	10.72	1.67	2.05	72.45	1.73	11.38	1.82	4.658	2.085	15.58	9.45
9	Maryland...........................	3.597	10.52	1.75	2.00	72.25	1.74	11.65	1.80	5.079	3.075	14.53	9.80
11	Virginia...........................	3.343	10.34	1.70	2.21	71.87	1.71	12.71	1.95	4.208	1.830	14.00	10.15
7	Georgia............................	3.579	10.00	1.96	2.30	72.24	1.72	11.78	1.89	4.627	2.834	14.00	9.45
22	North Carolina.....................	3.776	10.03	1.59	2.25	73.94	1.76	10.48	1.67	4.628	2.780	12.43	8.93
17	Alabama............................	3.314	10.94	2.03	2.21	71.84	1.62	11.86	1.79	4.647	2.011	13.65	9.80
	Michigan (Kedzie)	....	11.28	1.73		74.97		12.02	1.92	....	....	13.78	9.13
22	Michigan...........................	3.969	10.71	1.64	2.06	72.12	1.80	11.67	1.87	4.902	3.402	15.23	10.50
8	Kentucky...........................	3.454	10.83	1.75	1.87	70.37	2.03	13.15	2.10	3.666	3.146	14.53	11.90
14	Tennessee..........................	3.150	10.19	1.89	2.00	71.33	2.02	12.51	2.00	3.990	2.138	16.63	10.15
12	Missouri	3.502	9.80	1.92	2.19	72.36	2.17	11.56	1.86	3.867	3.098	14.00	10.50
9	Minnesota..........................	3.354	10.60	1.71	2.03	70.96	2.04	12.66	2.03	3.828	3.116	17.15	10.85
10	Kansas.............................	3.204	11.80	1.64	1.98	71.35	2.08	11.15	1.78	3.424	2.881	12.25	10.50
19	Texas..............................	2.847	10.03	1.81	2.11	70.85	2.06	13.14	2.10	3.937	2.561	15.23	10.68
45	Colorado	4.682	9.57	2.21	2.38	70.91	1.62	13.31	2.13	5.924	3.851	15.94	11.19
8	Oregon.............................	5.044	9.74	1.84	2.08	76.18	1.56	8.60	1.37	5.745	4.253	9.47	8.05

THE SOTOL, A MEXICAN FORAGE PLANT. By CLIFFORD RICHARDSON of Washington, D. C.

[ABSTRACT.]

THIS plant, *Dasylirion Texanum*, grows wild and extensively on the borders of the Rio Grande and elsewhere in Texas, and in Mexico, on a rocky and gravelly soil. The plains covered with it look like a vast cabbage-field. Sheep feeding on it go without water for many weeks. Only the bulb is eaten. It is split open by the shepherd, who carries a knife for the purpose. Mexicans eat the bulb after roasting or baking it in bits. Also a liquor is obtained from it, by fermenting and distilling after roasting, called "sotol mescal," and possessed of highly intoxicating powers.

The plant is described in Watson's Revision of the North-American Liliaceae. About 18 per cent of sugar can be obtained from the outer husks; in the interior, more than 10.5 per cent exists; and in the whole head of the plant there is probably more than 15.5 per cent of sugars. No starch seems to be present.

A proximate analysis of the soft interior of the head gave 17 per cent sugars: 62 per cent of this soft substance in the head, when fresh, is water.

As a food-plant in dry districts, the sotol is of great value; as a fibre-producing plant, it will not be of any importance, owing to the shortness of the cells.[1]

TWELVE MONTHS OF LYSIMETER RECORD AT THE NEW YORK AGRICULTURAL EXPERIMENT STATION. By E. L. STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

THE lysimeters were described. They are boxes of peculiar construction, containing the soil in its natural position. The relative percolation of rainfall through these different soils, and the evaporation, are determined by observations of the instrument. The results are summarized as follows: sod land allowed 11.68 of the rainfall to percolate; soil of which the surface was simply bared allowed 25.88 per cent percolation; the cultivated soil passed 37.93 per cent. The evaporation from the first of these was, of course, 88.32 per cent; from the second, 74.12; from the third, 62.07; the sum of percolation and evaporation being held to account for the entire rainfall. This paper will be printed in substance in the Annual Report of the New York Agricultural Experimental Station for 1883.

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[1] This paper will be printed in full in the Report of the U. S. Dept. of Agriculture.

NEW FORMS OF BURETTES. By W. H. SEAMAN of Washington, D. C.

ESTIMATION OF CARBON AND NITROGEN IN ORGANIC COMPOUNDS. By C. LEO MEES of Columbus, Ohio.

SECTION D.

MECHANICAL SCIENCE.

PAPERS READ.

REGULARITY OF FLOW IN DOUBLE-CYLINDER ROTARY PUMPS. By J. BURKITT WEBB of Ithaca, N. Y.

[ABSTRACT.]

THE author introduced his subject by exhibiting a number of models of these pumps from the cabinets of Cornell University, which has recently purchased copies (248 in number) of the models of the celebrated Reuleaux collection in Berlin. Class I of this collection is devoted to these pumps. He then produced and demonstrated a formula for the flow of these pumps, and showed that the regularity of flow depends upon principles differing from those which have been given for determining this point. The formula given for the flow (for unity thickness) was:—

$$\pi \left[R'^2 + R''^2 - (r'^2 + r''^2) \right] =$$

flow for one revolution, where R' and R'' (generally equal to each other) are the extreme radii of the two revolving wheels, and r' and r'' are the radii (often, perhaps generally, variable) from the point of contact between the wheels to their centres. It was shown that for regularity of flow we must have $r'^2 + r''^2 =$ constant. R' and R'' may be called the "piston radii," and r' and r'' the "valve radii." These pumps are called by Reuleaux "Kapselräderwerke," or chamber-crank trains, according to Kennedy.

IMPROVEMENTS IN SHAPING-MACHINES. By J. BURKITT WEBB of Ithaca, N. Y.

[ABSTRACT.]

IN the ordinary shaping-machine there are two defects, one of which is found also in the planer. The ram of a shaping-machine is a bar sliding in bearings, and carrying at one end the cutting-tool. If we represent by a the variable horizontal distance from the tool to the first bearing (or nearest end of the long bearing), and by b the variable horizontal distance from the tool to the second or farthest bearing (back end of long bearing), and by c the length of stroke, we shall have:—

$$\text{Maximum value of } a = (\text{minimum value of } a) + c$$
$$\text{Maximum value of } b = (\text{minimum value of } b) + c$$

In other words, the length of the ram is variable, and the spring of the ram from the work is variable, the tool springing away from its work more at the end of its stroke. This springing takes place mostly in the joint between the ram and its bearings, and cannot be wholly avoided without a change of construction. To remedy the defect, the author proposes a reversed construction of the sliding parts; the two bearings (preferable to a long bearing) to be formed on the ram, so as to make the distance a and b constant, and the long slide being part of the bed of the machine.

The second defect, which is also common to the planer, is in having a "drop-block" which fits but indifferently between the jaws and against the bottom of its seat. From the necessity of the usual construction, the tool attached to this block will have more or less spring. The remedy is to dispense with the drop-block, and introduce an automatic motion to lift the tool on the return stroke, as has been done, the author believes, on some large machines.

A Comparison of Terra-cotta Lumber with other Materials. By Thos. R. Baker of Millersville, Pa

[ABSTRACT.]

The investigation which formed the subject of this paper was to ascertain certain properties of terra-cotta lumber. This material is a kind of potter's clay, and is prepared by mixing the clay with sawdust and then burning out the sawdust. The permeability of the material to air was determined by forcing air through it by means of a column of water. It was found to be 875 times as permeable to air as pine and 135 times as brick. Other tests showed that it is four times as hard as pine, and that its grip on nails driven into it is about half that of pine. It was regarded as a valuable article for the purposes for which it is intended.

A METHOD OF TESTING LONG PLANE SURFACES, APPLICABLE TO THE ALIGNMENT OF PLANER-BEDS, LATHE-BEDS, HEAVY SHAFTING, ETC. By WM. A. ROGERS of Cambridge, Mass.

[ABSTRACT.]

THIS paper consisted of a statement of the difficulties of the problem, an account of four different methods tried by the writer, and the results obtained by the new method, which was described as "the method by two microscopes."

CENTRIFUGAL FORCE IN TURBINES. By R. H. THURSTON of Hoboken, N. J.

[ABSTRACT.]

A NOTE by Mr. J. P. Frizzell ("Journal of the Franklin Institute for June") especially interests the writer, as he has recently also noted the same defect in the accepted theory. It is so obvious as to require no proof that total centrifugal force, even where it is necessarily considered at all, depends for its measure upon the actual rotation of the mass of fluid, and not at all upon the movement of the wheel, except so far as the latter controls the former. In water-wheels, the two rotations are very different in their angular measures.

Plotting the path of the water in space, it will often be found that the line is so nearly rectilinear that the action of this force may be neglected as unimportant in practice. This is probably the case, for example, in the Vallet wheels. In the Whitelaw turbine, or "Scotch reaction wheel," as it is sometimes called, the spiral form given the arms may often be given such proportions that the path of the water may be nearly a straight line to the exit, and centrifugal force thus rendered so small that it may be neglected in the theory of that wheel. The writer has been interested in noting how much this method of treatment simplifies the theory of turbines, and how much more nearly, in some cases, the results accord with experiment. In the last form of wheel, for example, the expression for maximum efficiency becomes

$$E = \frac{1}{sec^2 \theta'},$$

and the speed of maximum efficiency becomes such that the orifice moves with the velocity due the head. The maximum effort is exerted, as it should be, when the wheel is held fast, and the effort becomes zero, when the speed of orifice becomes twice that velocity.

For the common case, in which the velocity of rotation of the water is a maximum at its entrance into the wheel from the guides, and becomes zero at exit from the wheel, varying in the interval by a uniform retardation, we easily produce expressions for the work done by or against the centrifugal force, and for the efficiency which will not here be given.

THE COMMERCIAL AND DYNAMIC EFFICIENCIES OF THE STEAM ENGINE.

By ROBERT H. THURSTON of Hoboken, N. J

[ABSTRACT.]

THE writer, at the meeting of 1882 of the American Association for the Advancement of Science, in a paper on a newly discovered "Absolute Limit to Expansion in the Steam Engine"[1] gave what he has called the "General Equation of Steam Engine Efficiency."

In that equation it was assumed that the annual expense chargeable to, or variable with, the cost of engine maintenance and operation could be reckoned per unit of volume of steam cylinder without serious error. Although this is often, and with large engines generally, true, and although it may not lead to appreciably erroneous results, it may yet be sometimes advisable to adopt the more general formula about to be given and which may be made as general and as exact as may be desired.

In the equation above referred to, an approximate form was adopted—making the denominator variable with variation of the waste by cylinder condensation and leakage, instead of the second term in the numerator—for the purpose of obtaining a more manageable, although less precise expression.

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[1] Proceedings of the American Association for the Advancement of Science, 1882; Journal of the Franklin Institute, 1882.

The equation to be here given may be used when greater precision and a wider range of application is thought desirable than could be secured with the equation already given for general use in earlier papers.[2] The mathematical treatment may be found in the Franklin Institute Journal Sept., 1883, and by means of the analysis there given the "Maximum Efficiency of a Given Plant" can be easily determined.

VELOCITY OF A PISTON IN A CRANK ENGINE. By C. M. WOODWARD of Washington University, St. Louis, Mo.

[ABSTRACT.]

IN the case of a practically uniform angular motion of the crank, the maximum linear velocity of the piston is greater than the linear velocity of the crank pin. The points in the path of the pin, for which the piston velocity is a maximum, are 70° or 80° from the nearer dead-point, the exact position depending upon the relative length of the crank and connecting rod. This result was exhibited graphically and shown to be consistent with the obvious fact that more than half of the stroke is made when the crank has turned through 90° from the nearer dead point.

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[2] Transactions of the American Society of Mechanical Engineers, 1882; Journal of the Franklin Institute, February to July, 1882.

SECTION E.

GEOLOGY AND GEOGRAPHY.

ADDRESS

BY

PROFESSOR C. H. HITCHCOCK,

VICE-PRESIDENT, SECTION E

THE EARLY HISTORY OF THE N. AMERICAN CONTINENT

THERE is a special appropriateness in the association of geography with geology, as indicated in the assignment of sciences to section E; for the latter gives us an account of the origin of every topographical feature of the earth's surface, whether island, continent, mountain, plateau, valley, or oceanic depression. If we would properly understand the significance of the earth's contours, we must unravel the mysteries of geology; so a knowledge of topography is essential to the complete comprehension of the geological features of any country. If a geologist were taken by a balloon to an unexplored part of the earth, he would instantly recognize, from their topographical outlines, volcanic and granitic cones, limestone hills, elevated plateaus of basalt or horizontal sandstones, and special types of orographic structure. Hence the modern geologist first draws the contours of his district before applying the colors of geological age. The existing relief features of the earth have been produced one by one in successive periods; and it is the task of the geologist to discover what were the characteristic physical developments of the several ages. He can delineate a connected historical sketch of the beginning, growth, and completion of a continent. Such histories are rare, because attention has but recently been turned into this direction. One of the first American geologists to frame such an outline is Prof. J. D. Dana, to whom we owe the enunciation of this fundamental truth,— that the first formed land has always remained above water, and has been a nucleus about which zones of sediment have accumulated. We can now recognize this primitive continent, with all its successive stages of growth, upon every geological map.

Time would fail us to present the entire physical history of our continent; and we will therefore confine our attention chiefly to its earlier chapters, noting those points which are under discussion. As we are endeavoring to *advance* science, we must touch upon debatable topics, and hope by friendly discussions to become wiser.

We must assume the correctness of the commonly received opinions concerning the earliest history of our planet,—that it passed through the condition of a nebula, and then of a burning sun, the period of igneous fluidity. By subsequent refrigeration it has become either partially or wholly solid. Not until a crust had formed, and the earth had cooled enough to allow water to remain permanently, was it possible to talk of dry land and ocean. With these premises allowed, it seems to us evident that the material of the earth must be disposed in concentric zones, arranged according to density, the heaviest being at the centre. If the various elements were free to move, as is the case in all natural or artificial igneous fluids, we must expect to find the heavier metals situated beneath the others; and, following the analogy of extra-terrestrial bodies, the central nucleus may be principally iron, like the heavier meteors. Zones corresponding to stony meteors, lavas, the trap family, and granites would naturally succeed in order, the last named being at the surface. This outer zone is also characterized by the presence of much silica and oxygen. The primeval ocean came from the vapors surrounding the igneous sphere, condensed to liquidity as soon as water could remain upon the solid crust without immediate vaporization.

This original crust may have been essentially a plain, and consequently entirely covered by water; for if the land were now levelled off, the ocean would submerge every acre of the continents. As refrigeration progressed, ridges and valleys would form in accordance with that fundamental principle that the outer envelope must conform to the shrunken nucleus; and this contraction gives rise to that tangential force or lateral pressure which has acted through all time. Whether these earliest ridges rose above the ocean would depend upon the amount of elevation. Some authors argue that these ridges follow the course of great circles. If there are causes adequate to produce such results,—or any other world-wide arrangement,—they must have commenced to operate at the very beginning of contraction. Most authors maintain that the very thick strata of the older rocks have been formed just like modern sediments, having been broken off the ledges, and transported into oceanic basins in horizontal attitude. If so, there must have been great mountainous elevations, deep oceanic depressions, and extensive aqueous action; since the thickness of the crystalline schists is greater than that of the strata in the fossiliferous ages. The amount of distortion, crumpling, and faulting of the crystalline rocks is also greater. These same authors hold that the original strata were in all respects like modern sands, gravels, and clays, and that their present crystalline structure is due to metamorphism. No one has yet discovered any uncrystalline pre-Cambrian beds; nor have the original foundation rocks been pointed out, since the oldest known layers are stratified, and cannot therefore have constituted part of the original unstratified crust.

Professor Dana thinks the primitive land originated because of a difference in the rate of conduction of heat during the process of refrigeration. Cooling would be fastest where the heat was conducted most rapidly. The first areas to cool would be the first to solidify. The first solidified crust was heavier than the adjacent liquid; so it sank until it found a fluid as dense as itself. Then the liquid above this crust would in turn become solid, and sink; and this process is supposed to have continued until a permanent shell had become fixed in the earth's circumference, which constantly increased in breadth and thickness, becoming continents. Meanwhile the other portions remained liquid; and their surfaces must have stood at the same level with the first-formed crust till that congealed, and became depressed because of the diminution of volume in solidification. These depressions became the ocean's beds. From this beginning down to the present time the processes of growth have consisted in the thickening of the continents and the settling-down of the oceanic depressions, while the chief force employed has been the lateral pressure derived from contraction. LeConte and Pratt express the process thus far described by the term "unequal radial contraction." The total gravity of the particles of matter along each radius is supposed to be the same; and hence, if the heat is conducted most rapidly over the shorter radii composed of denser minerals the ocean-basins would cool first. These two views thus demand a different arrangement of the lighter and denser materials, as pointed out by Mr. W. O. Crosby; the one necessitating that the continents, and the other the depressions, were first to congeal. Both, however, make the gratuitous and unproved assumption, that the surface was not uniform in composition; the difference being probably like that between granite and trap. The principle stated above — that, where all the particles are free to move in a liquid, the lighter elements must rise to the surface, and the heavier minerals sink down in proportion to their specific gravity — is at variance with this assumption. Fortunately it is not essential to a right theory of continental growth. There is no reason, therefore, to doubt that the original crust had essentially a uniform thickness over the whole earth. Contraction would originate ridges and valleys in the normal way, most likely of similar dimensions. There must soon burst forth ejections of igneous matter, owing to tidal attraction; and these would show themselves along the weakest lines. At the outset it is difficult to assign reasons why either the elevations or depressions would be the weaker; and hence we should look for a multitude of locations of igneous overflow, both over the future continents and oceans. There may be no better reason for the eventual enlargement of certain of these volcanoes than that circumstances only very slightly favored them; but, this favor being continued, they would exist and enlarge at the expense of the others, affording us another illustration of the "survival of the fittest."

It seems to us there is now afforded an opportunity for reviving in a modified form the view of Poulett Scrope in regard to the origination of the earlier crystalline deposits. Suppose we say, that, besides the original unstratified igneous granitic material, the oldest stratified crystalline rocks are derived from volcanic ejections; being the continued enlargement in size, and reduction in number, of the early indeterminate vents. The several ejections would increase in size till they became islands, either gneissic or granitic; and, if an archipelago is allowed us, we can easily show how continents would accumulate, using only the universally acting forces of lateral pressure and sedimentary accumulation.

Of other theories relating to the origin of the earlier crystalline beds I may mention two. The first is that advocated by Lyell, who termed these rocks *hypogene*. After the solid granitic crust had been formed by refrigeration, "the hot waters of the ocean held in solution the ingredients of gneiss, mica-schist, hornblende-schist, clay slate, and marble,—rocks which were precipitated one after the other in a crystalline form" (Lyell's Principles of Geology, tenth ed., i, 142). In such a menstruum, life could not have existed. A very similar view was advocated by Dr. T. Sterry Hunt in his presidential address before this association in 1871.

The second is the view more commonly entertained,—that, after the solidification of the crust, sedimentation accumulated stratified systems from the granitic foundations, as ordinary sand, gravel, and clay, which were subsequently acted upon by thermal and aqueous influences, termed *metamorphic*, and thus converted into crystalline schists. The widespread and powerful action of metamorphism is conceded; but it is a more appropriate adjunct to volcanic than sedimentary accumulation.

A few of the considerations favoring our theory will now be presented.

1. Considering the igneous origin of the earth, volcanic energies would naturally continue their action as soon as there was a crust to be broken through, and immense piles of melted rock would ooze from the numerous fissures. Up to Laurentian times all admit the universality of igneous outflow, while but few have ventured to speak of anything like volcanic action, except as it has been manifested in the formation of dikes in these early periods. There has been a tendency to class the ancient granites and porphyries with rocks of sedimentary origin, and consequently to restrict the action of igneous agencies to phenomena of slight importance. Several English writers, and, in our country, Dr. Selwyn of Canada, have been calling our attention to the existence of a volcanic group in later Huronian or early Cambrian times. These are the rocks so largely developed about Lake Superior, New England, and the Province of Quebec, consisting of stratified schists, diorites, diabases, amygdaloids, and felsites, identical in composition with true eruptive masses of the same name. Investigation shows that oftentimes these schists are disposed like the lavas ejected from one series of volcanic vents. Suppose, for example, that Etna or Vesuvius should become extinct. In the course of ages the rains would obliterate the craters, and reduce the lavas to a rounded dome of greater or less regularity. We should recognize the volcanic origin of the mountain in the absence of craters, from the lithological similarity of the rocks to those

known to have been melted and ejected from vents, while the disposal of the material in a conical attitude shows us that it might once have been covered by craters. So we find in our eastern country many domes of diabasic or protogenic schists, whose volcanic origin may be predicated, both from their lithological character and physical aspect.

Now, this volcanic group of Huronian times indicates the existence of a greater degree of igneous activity than has been described for the paleozoic ages, even in Great Britain; and consequently this is an indication pointing significantly towards the predominance of thermal influences in the still earlier periods. In the Laurentian age the fires should have been yet more vigorous, because the time of universal igneous fluidity was less remote.

2. A careful study of the crystalline rocks of the Atlantic slope indicates the presence of scattered ovoidal areas of Laurentian gneisses. Those best known have been described in the Geology of New Hampshire. Instead of a few large synclinal troughs filled to great depths with sediments, the oldest group is disposed in no less than twenty-two areas of small size, scattered like the islands in an archipelago. In a chapter upon the physical history of the state, I have proposed the theory that the earliest land within its limits consisted of this series of islands, not packed together as closely as now, in an area of perhaps three thousand five hundred square miles, but as much more widely separated as would be determined by smoothing out the various anticlinals and synclinals that were formed later. By reference to our maps in Maine, Massachusetts, New Jersey, Pennsylvania, Virginia, North Carolina, and Georgia, many similar ovoidal Laurentian areas may be specified, usually larger than those of New Hampshire. This may be due partly to a less thorough knowledge of the exact areas occupied by this older gneiss, and partly to the existence of a greater number of volcanic vents, giving rise to a more widely spread and thicker mass of ejected material. Over the Atlantic slope and Canadian Highlands these primeval islands have, in later periods, been cemented together by a subsequent deposition of material; but in Missouri, Arkansas and Texas, we recognize, even now, these early islands.

3. The lithology of the Laurentian and other crystalline rocks is very like that of igneous ejections. It is proper at this point

to recall the proper signification of the term Laurentian. As originally defined by Logan, it included every formation that antedated the Huronian. The subdivisions of lower, middle and upper Laurentian are often used by the Canadians. Logan spoke of the gneiss of Ottawa and of Trembling mountain as being decidedly older than most of the series. In his report for 1845 he distinguished the groups by the absence of limestone in the lower and its presence in the upper series. This distinction was again alluded to by him in 1857 but never made prominent afterwards. Several years later Logan defined an upper series, characterized by the presence of triclinic feldspars, and it was regarded as overlying unconformably the gneisses holding limestone. The names Labrador and Norian have been applied to this upper series. Inasmuch as these upper rocks are largely eruptive, Selwyn and others think that the institution of a Labrador system is a mistake, and hence that the lower and middle. or the Ottawa and Grenville series represent the whole of the Laurentian.

In the report upon the geology of Canada for 1877–1878, Selwyn proposes to restrict the Laurentian outcrops to "all those clearly lower, unconformable, granitoid, or syenitic gneisses in which we never find interstratified bands of calcareous, argillaceous, arenaceous, and conglomeratic rocks." The Hastings and Grenville series, and all the schists containing the eozoon, are excluded from the Laurentian by this definition, as well as the Bethlehem, Lake Winnipiseogee, and Montalban groups of the Atlantic slope. The lower Laurentian is azoic, the other groups eozoic; and, unless newer distinctions are to be made hereafter, it looks as if we might claim these various azoic Laurentian islands as the first-formed dry land, as they certainly are the nuclei of the existing continents.

There are no minerals in these Laurentian islands that do not occur in eruptive granite; and the schistose structure is often so faint that the field geologist need not be blamed if he acknowledges his inability to detect it. Likewise we discover the same fluidal inclusions and the vacuoles that pertain to granite. If we follow Sorby and Clifton Ward in saying that granite has been formed beneath a pressure equivalent to a weight of forty thousand feet of strata, the same must be said of the early gneisses. With this general assertion of the identity of gneiss and eruptive granite, we must be satisfied at present, without entering into detail.

4. The analogy of the origin of oceanic islands at the present day suggests the igneous derivation of the Laurentian areas. Most of the high islands of the Pacific are composed of lava, with the volcanoes frequently in action.

The so-called lowlands are likewise of volcanic origin; since coral polyps have built up reefs upon the igneous area after the disappearance of the fire, and the Hawaiian areas are encircled by reefs. After the volcanoes have become cold, loose material would be worked in between them, coral reefs would grow, and, in various ways, the land area would be enlarged, and finally an archipelago may become a large island. It needs only time and a repetition of these constructive agencies to make a continent out of a series of archipelagoes.

5. The oceanic volcanic islands are not inferior in size to the crystalline Laurentian areas upon the present continents. Hawaii, of the Hawaiian group, may illustrate their position and shape. Its area above the water-line is 4,210 square miles, and its cubical contents above the sea-level are about the same with those of New Hampshire. It rises from a plateau over 16,000 feet deep, thus forming a cone 30,000 feet high, whose cubical contents must be twenty times greater than the portion making dry land. The length of the entire series of islands, all of similar character, is 350 miles, and the area of the base of the lava must be about 100,000 square miles. These cones have been built up by the accumulation of lava ejected from the interior of the earth, and they are entirely isolated, the nearest land being 1,000 miles distant. The ground-plan of this volcanic mass is that of two elliptical areas, either of which is like some of our Laurentian islands, and is certainly as large as any of these ancient lands south of the St. Lawrence. The land area of the Hawaiian Islands is less than that of Massachusetts, but their base must be equal to the whole of New England and New York combined. Surely it cannot be avowed that volcanic areas are too small to be compared with the space occupied by our oldest formation.

There are two points requiring explanation in this connection, —first, the supposed deeply seated localities where granite is produced; and, second, the origin of foliation in the schists.

We should naturally expect that the earlier igneous rocks would have been derived from reservoirs quite near the surface, because of the thinness of the crust. With this notion agrees the presence of cavities containing liquid, and of hydrated minerals, which are more common in the older eruptive rocks, and have led to the aqueo-igneous theories of the origin of granite. Water would be scarce at great depths, and hence these rocks ought to originate near the surface where moisture was abundant. It seems to us that this consideration should more than balance the arguments usually cited in favor of the origin of granite at enormous depths, as it is difficult to see how both can be true.

Mr. H. C. Sorby has led the way in studies of the mineral constituents of eruptive rocks. He measures the included cavities in the component minerals, and calculates how much the contained substances must have contracted in cooling, allowing for an increase in the temperature of the point of vaporization under pressure. By assuming the temperature to be correctly determined, he ascertains the amount of pressure indicated by mathematical formulæ, and finds it to be the equivalent of a thickness of 40,000 feet of overlying rock in Cornish granites, and of 60,000 feet in Scotch granites. Later writers seem to have regarded this pressure as certainly produced in the way thus suggested, and that its appearance at the surface has been due to an enormous erosion which has denuded the overlying blanket. This conclusion is not necessary; for, *first*, an enormous pressure would result from the tangential force of contraction, which would be entirely adequate to have produced the cavities. *Second*, the necessity of an erosion of 40,000 feet over all the granites in every part of the world cannot be maintained. In North America, for example, it would necessitate the supposition that nearly eight miles' thickness of rock had been removed from one-fourth of the surface since the Laurentian, for the blanket removed would have equalled in dimensions the crystalline areas. The mere statement of the amount of denudation required refutes the theory. *Third*, by reference to existing volcanoes, it is plain that a column of lava will often be adequate to exert the needed pressure. Teneriffe rises 12,000 feet above the ocean, and its cone descends 18,000 feet more to the submarine plateau. When the crater is full of melted lava, there must be a pressure of 30,000 feet at the base of the cone: hence the lava from the reservoir supplying Teneriffe might exhibit the vacuities produced by a pressure of 30,000 feet without any weight above the peak.

When molten lava pours down the side of a crater, the included vapors and liquids must disappear because of the removal of the pressure, but, after a substantial crust has formed, the peculiar markings imprinted at the great depth would remain: hence we can understand how it is that the vacuities are to be seen both in granites and lavas that have been subjected to great pressure. At the Boston meeting of this association I endeavored to show that there are mountain masses of granite in New England possessing all the physical characteristics of volcanic cones. The material must have been liquid, hot, ejected from a vent, and flowed over a plateau, building up a cone, and indurating the underlying floor. It was claimed that such phenomena could be explained only by supposing the granite to have been erupted just like lava. This granite contained the usual vacuities indicative of great pressure just as they are also found in the lava of Monte Somma or the trachyte of Ponza.

When one examines the interior structure of modern lava-flows, he is surprised to find beds nearly as well defined as the foliation of schists. Around vents like Vesuvius or Etna the lava accumulates naturally in quaquaversal sheets, no one eruption being very extensive. When steam and hot water are copiously supplied from the caldron, there may be flows of hot mud and tufa. The closing phases of eruptions are usually showers of ashes falling upon the cone or beyond. If the vent is beneath the ocean-level, the lava is minutely subdivided and the deposit will be like sand or gravel. Between the igneous flows the ordinary aqueous agencies will wear off excrescences, and scatter the fragments down the slope. These various agencies will produce a concentric stratiform arrangement in the whole mass. Where the eruption is massive, a similar set of layers will be formed.

This mass of volcanic material will be very susceptible to metamorphic influences when placed under the proper conditions of heat and pressure. As the result, new minerals will be formed, arranged in foliated beds or schists. Thus briefly stated may be the origin of foliation. So long ago as 1825, Poulett Scrope advocated essentially this doctrine for the arrangement of the crystalline particles in the crystalline schists, having found an analogous structure in certain volcanic accumulations.

Sufficient has now been said in advocacy of our doctrine that the first land consisted of volcanic islands. This was the Laurentian or azoic accumulation. Cartographers have not yet distinguished the several crystalline deposits, so that it will not be practicable at present to point out the supposed volcanic areas of the Hastings, Grenville, Montalban, Huronian, and other eozoic periods. Sedimentation would also act so that in this age many beds must be referred to an aqueous derivation. By the close of the eozoic age the continent was outlined; or at least the framework of the future superstructure was put into position. The broader patches about to be mentioned had their origin in the earlier numerous islands cemented by detrital accumulations.

The more important areas developed in the eozoic must have been Greenland, Canada east of Lake Winnipeg, the Atlantic district, the Rocky Mountains, the Sierra Nevadas, and numerous buttes over the Cordilleras. The three great depressions of Hudson's Bay, the Mississippi valley, and the Salt-Lake and Nevada basins commenced to sink very early, and the future growth of the continent consisted largely in filling them up with marine sediments. An inspection of a map drawn upon a correct scale will dissipate the fancied resemblance to the letter V, in the Canadian dominion, so often insisted upon. Neither has the development of the land been in bands parallel to the northwest and southeast arms of this supposed angle. A better conception would find three great basins, excluding the unknown regions of Mexico and Alaska, in each of which operations were conducted independently. The best known is that of the interior of the United States, or the Mississippi hydrographic basin. This depression was nearly encircled by a crystalline border of high land. Beginning at Alabama, we follow it to New England, thence by a slight gap to the Adirondack promontory, thence across the Lakes to the Dakota promontory. In Minnesota and Dakota the schists are more or less covered by cretaceous clays and tertiary sands; but they evidently constitute the floor for the later deposits, as seen in the Black Hills. Thus we may connect the Dakota and Rocky Mountain crystallines. From Wyoming southerly the granites are again conspicuous into New Mexico. Thus the circuit is not complete: it is like a horseshoe, with the lower Mississippi valley in the gap; yet this may have been filled in the Cambrian age, since Laurentian islands are found in Texas, Arkansas, and Missouri.

PAPERS READ.

EVIDENCE FROM SOUTHERN NEW ENGLAND AGAINST THE ICEBERG THEORY OF THE DRIFT. By JAMES D. DANA of New Haven, Conn.

IN presenting to the Association evidence from southern New England with regard to the insufficiency of the iceberg theory of the drift, I shall have to say some things that have often been said before, and by various investigators. But I may claim for what is here brought forward, that it is, in my own mind, the fortified conclusion of long-continued investigation.

The arguments on the subject are derived from three sources:—

I. The scratches and groovings over the rocks.
II. The transported bowlders and other materials.
III. The facts as to the relative level of the land and sea.

I. The scratches or grooves over the rocks.

Under this head there is, first, the old argument based on the universal distribution of the scratches over the region of all New England. These effects of abrasion are to be found everywhere beneath the soil, each fresh exposure of the rocks bringing them to light. This was said years ago; and the conviction of its truth has been gaining force with every year of additional observation.

a. In view of this fact, it is urged rightly that only an abrading agent that pressed heavily against the broad rocky surface could have produced the effects; and such is not an occasionally grounding iceberg, or a succession of them. Neither is it the still more locally acting shore-ice.

b. Floating ice would have found little bare rock over the sea-bottom to be abraded. Like the bottom of existing seas, and eminently those of the continental borders, the submerged region would have had for the most part a bottom of detritus, its former detritus, and additional detritus from later depositions. The removals would have been local, and relatively of small area. Consequently, the drifting ice would rarely have reached down to the rocks. Shore-ice carried along by the currents would have had a better chance, and yet a poor one, for the work to be done.

c. The character of the groovings and ploughings is, to a great extent, such as floating ice could not have produced. As has been often said, the close uniformity of direction and parallelism over large areas, which so generally prevails, is not a possible result of iceberg action. The needed pressure and steadiness of movement are wanting.

Troughs in hard granite even six inches deep are the work of one and the same moving tool for a long period; and one year would be long for the steady action of an iceberg. If grounded, it would do almost nothing; if floating free, absolutely nothing; and a nice adjustment to depth would be required for any steady abrasion, much nicer than would have long continued anywhere over the uneven bottom.

In the Triassic sandstone of East Haven, Conn. (just east of New Haven), at a place where the sandstone is a very firm, thick-bedded, gritty rock, the ploughing ice ploughed out a piece of moulding, somewhat like the *ogee* of the carpenter, which was 8 feet deep, 25 feet wide, and over 120 feet long, and perfectly even in surface as well as direction.

d. The currents that would have borne along the icebergs over submerged New England, in case of a submergence sufficient to cover the highest striated surfaces,—3,000 to 5,500 feet,—would have been those of the present ocean, the Labrador current, and Gulf stream; and, with less submergence, the same in part, modified by the courses of the valleys and the tides. It is to be noted, that the New Haven region, in Connecticut, is the southern extremity of the Connecticut valley. The mean trend of this valley in Connecticut is about S. 15° W., and, in southern Connecticut, S. 18° W. Now, the numerous scratches over the *eastern* portion of the New Haven region average in direction S. 16° W.; but along its *western* border, where the rapidly rising slopes give the region rather an abrupt limit 150 to 350 feet high, the scratches have an average course of S. 33° W., the extreme being S. 27° W., and S. 55° W.; and S. 33° W. is the almost uniform trend over the undulating surface of the country for six to nine miles west. It is, as far as I can see, impossible that the valley stream should have had on its west side so wide a divergence from the direction of the Connecticut valley: all the features of the region oppose it. The scratches are well exposed over the metamorphic rocks in many places; and large and perfect examples of *roches moutonnées* here occur.

Again: over the higher lands of western Connecticut (and of New England generally, according to the observations of Prof. Edward Hitchcock, Prof. C. H. Hitchcock, and others), the direction of the scratches is southeastward. To have produced them, if icebergs were the agent, the submergence should have exceeded 2,500 feet, and this would have given a chance for the full play of the oceanic currents; and yet the above direction does not correspond with that of either of the great currents.

II. Distribution of the drift.

Bowlders of trap, from 50 to 1,000 tons in weight, are numerous in the New Haven region, especially along its western border. All are Connecticut-valley travellers; for the trap ridges of the valley—400 to 1,300 feet in height—are the only possible source. They were gathered up by the ice from these trap ridges, and were carried 15 to 60 miles down the valley. It is mechanically impossible that the larger bowlders should have been taken up, or gathered in any way, by floating ice; either shore-ice, where the water was but 1,000 feet deep and less, or by that of icebergs, where the depth was greater. It is well known, that the distance of drift transportation is in general less than 20 miles. Hills of but 100 feet often have their long trails. A moving glacier would easily gather and carry along the material from hills, high or low, wherever loose or detachable masses of rock or gravel existed to be gathered; while floating ice would be very poor at gathering, and hence inefficient in distributing.

III. Relative level of the land and sea.

I have examined carefully along southern New England for proofs of the Quaternary submergence which the iceberg theory assumes to have existed in the glacial era. I thought at one time that I saw evidence about New Haven of a submergence of 45 to 50 feet. But the terrace that afforded the evidence was situated six miles back from Long Island Sound, adjoining the rivers; and on further examination I found that the deposits had precisely the structure of those along the river-valleys farther north, and that, in fact, they were nothing but fluvial formations. The highest terraces on or near the shores of the sound, in the vicinity of New Haven, have a height above mean tide of 23 to 26 feet; and on Milford bay, nine miles west, a similarly situated terrace has a height of 30 to 33 feet. Along the hills facing the waters, and the southern extremity of the valleys, no traces of any higher level exist. Twenty-five to thirty-five feet is the greatest amount of submergence the facts sustain. Seaborder deposits exist at a higher level on the coast of Maine and on the shores of the St. Lawrence, and show what was the position of the shoreline in those regions. But the level along southern New England is not proved by the facts there gathered, neither is it established by the demands of the iceberg theory.

In conclusion, if icebergs, or floating masses of ice, were not capable of covering with scratches great continuous areas, and would have had a chance for little rock-abrasion on account of the covering of detritus; if they could not have made, in their hitching and swinging way of action, when touching bottom, scratches over great areas, that had the even course and parallelism characterizing those of drift regions, or could not have ploughed out the deep furrows; if they could not have gathered the great bowlders for transportation; and if the sea along the sound did not cover the land, in any part of the era of ice, to a greater depth than 30 or 35 feet,—the iceberg theory of the drift may be reasonably pronounced unsatisfactory for southern New England; and similar facts show that it is equally unsatisfactory for the rest of New England.

THE ANCIENT GLACIATION OF NORTH AMERICA: ITS EXTENT, CHARACTER, AND TEACHINGS. By J. S. NEWBERRY of New York.

[ABSTRACT.]

WHILE the glacial area on our continent has not been fully explored, there is abundant proof for the following propositions: 1. Glaciers covered most of the elevated portions of the mountain belts in the west as far south as the 36th parallel, and in the eastern half of the continent to the 40th parallel of latitude. 2. The ancient glaciers, which occupied the area above described, were not produced by local causes, but were evidences of a general climatic condition. 3. They could not have been the effect of a warm climate and an abundant precipitation of moisture, but were results of a general depression of temperature.

The traces of glaciation are similar in kind, and apparently in date, over the whole area: they are therefore effects of general, not of local, causes. East of the Mississippi, the evidence is even more widespread and impressive than in the far west. The area bearing marks of ice action, and strewn with drift, extends from New England westward, parallel with the Canadian highlands, in a belt five hundred miles wide and over two thousand miles long. Its northern extension has not been traced beyond Winnipeg; but there are reasons for believing that it reached to the Arctic ocean, and that the great lakes are preglacial river-valleys, scooped out and modified by ice. Fully half the continent north of the 36th parallel was glaciated. So far as we now know, the glaciation was synchronous.

The iceberg theory was opposed on the following grounds: it postulated a water-line with irregularities of level that are irreconcilable. The direction of the scratches, and the lines of derivation of the bowlders, require that the northern portion of the continent should have been all submerged, leaving no land for the origin and starting-point of icebergs. If icebergs could have been formed and floated, an incomprehensible tangle of ocean-currents would be required to account for their movements. The evidence of sea-covering, in the form of marine shells, is totally absent from the great drift area of the interior, while they are found abundantly in the Champlain and bowlder clays of the coast. Finally, the inscription left by the eroding agency is characteristic and *sui generis*.

The record of the ice period on our continent is far more extensive and impressive than has been represented. The phenomena were due to extraneous and cosmical causes, not to anything local or even telluric.

The question here passes from the geologist, and must be addressed to the astronomer. The author briefly recapitulated some of the theories which have been suggested by Croll, Newcomb, and others, to account for the glacial epoch.

THE ERODING POWER OF ICE. By J. S. NEWBERRY of New York, N. Y.

[ABSTRACT.]

THE object of this essay was to enter a protest against the theories of certain geologists who claim that glacial ice has not played an important part in the erosion. Several heresies have been advanced in regard to this subject: 1. The denial that there ever was a glacial period. 2. The assertion that if there was an ice period it was a warm and not a cold one. 3. That the phenomena usually ascribed to glacial action in the record of the ice period were generally due to icebergs. 4. That ice has little or no eroding power, and that glaciers have never been important geological agents.

In refutation of these theories the author gave the result of his extended study of glacial action in the Alps, and in many different parts of the United States and Canada. The conclusions to which he had been led by his observations were: 1. That the glacial period was a reality, and that its record constitutes one of the most striking and interesting chapters of geological history. 2. That this was a cold period. 3. That ice has great eroding power, and that in regions which they have occupied, glaciers have always been important and often preponderating agents in effecting topographical changes.

The facts on which these conclusions were based may be summarized as follows: 1. All the mountain chains and highlands of the west have been simultaneously covered with ice or snow fields, for moraines, excavated lake-basins, rock-surfaces marked with the peculiar inscription of glaciers, rounded hillocks, hills and mountains attest the former presence of ice in the Rocky Mountain belt, in the Wasatch and in the Sierra Nevada from the 36th parallel as far northward in British America as explorations have been carried. 2. On the Atlantic coast the glacial topography, glaciated rock-surfaces and glacial debris (till, moraines, kames and Champlain clays), stretch continuously from New York to Maine, and, as we learn from many observers, to Greenland, where continental glaciers covering thousands of square miles and apparently some thousands of feet in thickness are doing the work of which the record is left farther south. 3. The highlands north of the St. Lawrence, once an elevated mountain range, have been planed down to an undulating surface, the topography resulting from ice action, and everywhere showing ice marks. 4. A belt of country parallel with the Canadian Highlands reaching from Maine west and northwest to and beyond Lake Winnipeg, an area 2000 miles by 500, is glaciated and covered with glacial debris to an average depth of 30 to 50 feet. 5. The basins of the Great Lakes were certainly once occupied by glaciers, which moved approximately in the direction of their longest diameters, and have been in part, at least, excavated by ice action. 6. Similar records reported by Forbes and Whitmell in Norway, Ramsay, Geikie and others in the British Islands, Penck in Bohemia, Agassiz, Guyot and others in Switzerland, Godwin-Austen in India, Darwin and Agassiz in South America, confirm the testimony of the rocks here, and establish beyond question at least one ice period as a marked chapter in geological history, and prove the enormous eroding power of ice.

The erosion of ice is effected by sand, gravel, and bowlders frozen into its under surface and carried forward in its motion; where the glaciers were 1000 feet in thickness the grinding material was pressed upon the underlying rock with a force of 48,700 pounds to the square foot. The ancient glaciers of New England were from 3000 to 5000 feet in thickness, exerting a pressure of from 150,000 to 250,000 pounds to the square foot. The weight, grinding material and incessant motion, make glaciers over the area they occupy far more powerful eroding agents than water. This is proved by the relative amounts of sediment carried by the streams which drain glaciers and those which do not. The latter are made turbid only by occasional and temporary rains, while the former are always opaque from the quantity of sediment they transport; and yet this is only the finer material ground up by the glacier, the coarse being left behind. The quantity of transported matter has in some instances been measured: for example, the stream which drains the Aar glacier brings down 280 tons per day, and the Justedal glacier of Norway wears down 69,000 cubic metres of solid rock annually; and these are measurements of the eroding power of two small glaciers

THE LIFE-HISTORY OF THE NIAGARA RIVER. By JULIUS POHLMAN of Buffalo, N. Y.

[ABSTRACT.]

IN preglacial times the valley of the Tonawanda was occupied by a lake whose waters found their way into the Ontario valley across the upper rapids at Niagara Falls, thence along the present river gorge down to the whirlpool, and from here by the way of the buried St. Davis channel. After the ice period this outlet was closed. Lakes Eric and Ontario drained simultaneously, as testified by beaches around both lakes, until both stood on a level with the Lewiston heights. From this time Lake Ontario drained slowly with many resting places and the waters of Lake Erie flowed over the mudflat along the preglacial river valley between Buffalo and Lewiston. We must therefore reject any theory of falls at Lewiston; contrary to hitherto accepted data, the waters could, at their best, form only a rapid over the escarpment; for the Niagara limestone here is only thin, and it was a comparatively easy task for the immense volume of water to excavate the soft strata of rock between Lewiston and the whirlpool, where it met the bed of the preglacial river up to the present site of the falls. This fact explains why the river turns almost at right angles opposite the whirlpool. I therefore conclude that the time necessary to excavate the gorge of the Niagara below the falls has always been overestimated, for that portion which would take the longest time, between whirlpool and fall, was already excavated in preglacial times and the three miles between whirlpool and Lewiston were composed almost entirely of shales.

RESULT OF EXPLORATIONS OF THE GLACIAL BOUNDARY BETWEEN NEW JERSEY AND ILLINOIS. By G. F. WRIGHT of Oberlin, Ohio.

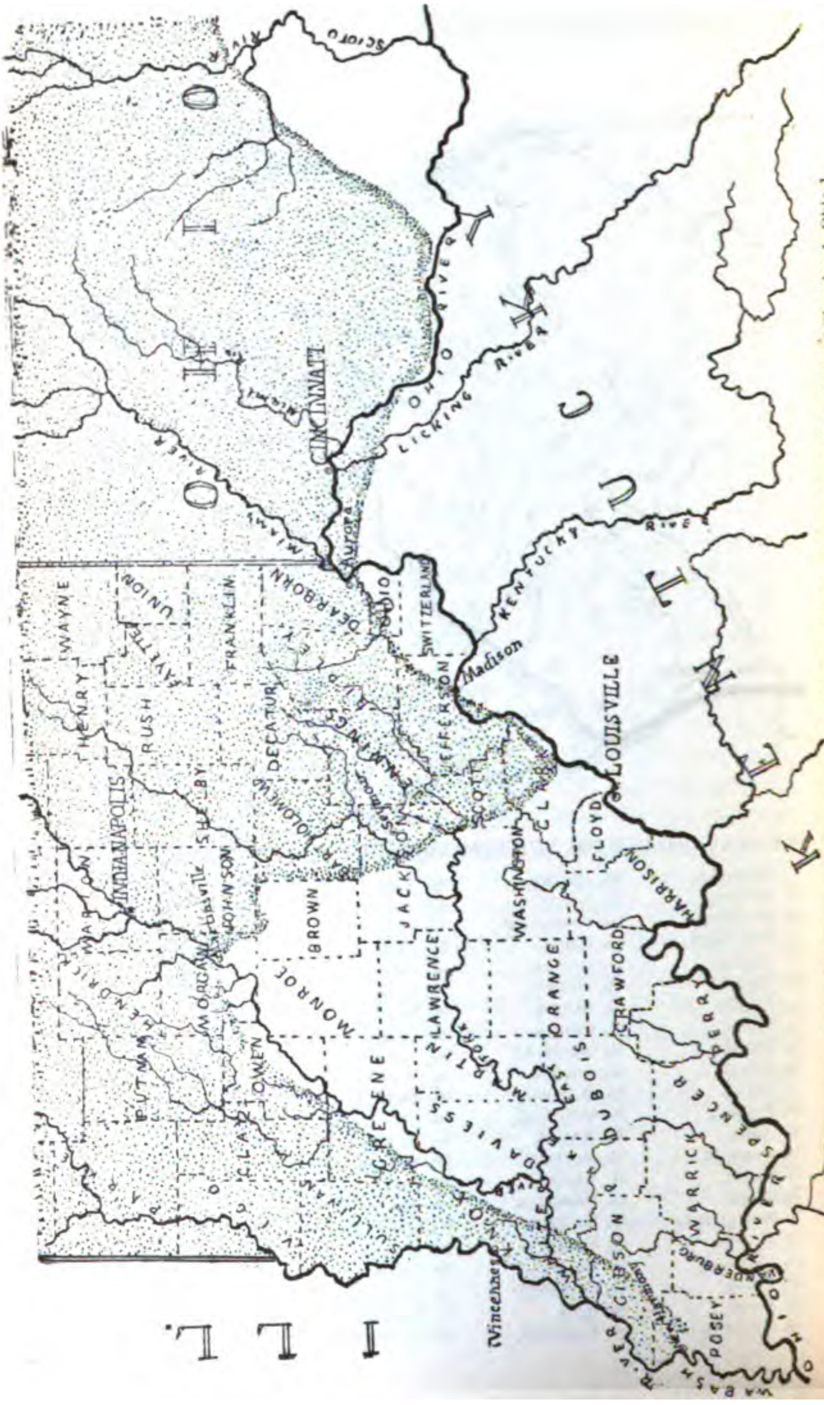
[ABSTRACT.]

AFTER citing reasons for desiring a careful *résumé* of the subject, —the observations being scattered in the works of different explorers,— the author proceeded to name those who had determined, for different regions, the southern boundary of the glaciated area.

Starting at the eastern coast, President Edward Hitchcock was the first to intimate that the backbone of Cape Cod was a part of the terminal moraine if the theory of Professor Agassiz were true. Clarence King made a similar assertion as to accumulations near Wood's Holl and on the Elizabeth islands. Professor Charles H. Hitchcock declared that the backbone of Long Island was the foot of a terminal moraine. Warren Upham went over this field, from the end of Cape Cod to Brooklyn, to verify the hypothesis. Professors Cook and Smock traced the moraine across the state of New Jersey. Professor Lesley commissioned Professor H. Carvill Lewis and the author of the paper to continue the exploration across Pennsylvania. In Ohio, Professor Newberry had approximately outlined the boundary; but in Ohio, Indiana, and Illinois, the survey was carried on by a number of different persons before the most distinctive glacial features were fully understood.

The chief indications on which reliance can be placed to determine glacial action are striated rocks, striated stones, bowlders, and till. Rocks near the margin are often so deeply covered with till that their markings are not apparent. The softer rocks do not always retain their striæ; this has often been the case in Ohio. In certain situations, stones might be striated by a landslide, or the grounding of an iceberg; but the area over which striated stones are found is too vast for such explanation of their presence. The bowlders are of granite and metamorphosed rocks from northern Canada and the shores of Lake Superior; their presence is relied upon only when they are in such situations as to preclude the likelihood of their having been transported by the agency of rivers. Till is spread over the whole area: it is defined as an unstratified deposit, containing striated stones of various sizes,— fragments of rock foreign to the locality. Its composition varies, through mixture with underlying material. It covers and gives fertility to northern Ohio, Indiana, and Illinois. Till has been well characterized by Professor Newberry as the grist of the glacier.

Briefly told, the boundary-line of the glaciated area, so far as now accurately known, is as follows: beginning on the island of Nantucket, it runs through Martha's Vineyard, No Man's Land, Long Island from east to west, across Staten Island, entering New Jersey at Perth Amboy, and after bending north-



Southern Boundary of Glaciated Area in Indiana. [From G. F. Wright's Report to the Hist. Soc. of Cleveland, Ohio.]

are sufficient proof. For instance, the line is near sea-level in New Jersey; in Pennsylvania it rises over Mount Kittatinny to a height of 1,600 feet, then descends 800 feet into a valley, and, again rising, reaches the summits of mountains 2,000 feet above sea-level. Crossing the valley of the Susquehanna at an elevation of only 500 feet, the line mounts the Alleghenies diagonally, and runs over them at a height of 2,100 feet.

The paper proceeds to describe certain marked features of glaciated areas. South of New England, the terminal line is characterized by a series of glacial hills, 100 to 300 feet high. These are also observable in New Jersey, near Plainfield and Menlo Park.

Among the most interesting results of the author's survey in Ohio, was the demonstration of the existence of an ice-dam across the river at Cincinnati. The line bounding the glacial accumulations crosses the Ohio river into Kentucky near the boundary between Campbell and Pendleton counties, about twenty-five miles above Cincinnati, and recrosses it near Aurora, Ind., about twenty-five miles below Cincinnati, thus filling the channel for about fifty miles of its course. The Ohio, it should be said, occupies, throughout nearly its whole extent, a narrow valley of erosion, not often more than a mile wide, and from 300 to 500 feet deep. Emptying into the main channel there are subordinate channels all along, of smaller dimensions, but of nearly equal depth. The proofs that the ice bodily crossed the river at the points indicated are, that till and granitic bowlders are found on the Kentucky hills south of the river to a certain distance, and not beyond it.

To the question, Why is the boundary of the glaciated area so crooked? the author replied at some length; assigning as a principal cause, aside from differences of level, the probability that unequal amounts of snow fell over different regions to the north. The effect of such differences of accumulating snowfall, in determining the extension of the glacial outline, is illustrated by supposing that two loads of sand are placed in one pile, and one load in an adjoining pile, when the sand will flow downward to unequal distances upon a level.

A little reflection will show that the glacial theory will not make extravagant suppositions as to the amount of ice required. The ice was indeed 6000 feet deep over New England, and, very likely, of an equal depth over the area to the west; but it is not necessary to suppose a great increase of this depth to the north. All that is necessary is to suppose great accumulations of ice to the north of the granitic hills of Canada, starting a movement past them to the south. This movement may have been kept up toward the margin by fresh accumulations of snow upon the spreading glacier. An accumulation of snow over the glacier in any part of it would spend its effective force in giving impetus to the movement of the front along the lines of least resistance.

GENETIC CLASSIFICATION OF THE STONY DRIFT CLAYS. By T. C. CHAMBERLIN of Beloit, Wis.

[ABSTRACT.]

IN a somewhat extensive study of the drift east of the Rocky mountains, the importance of a more complete genetic classification of the drift had been felt by the speaker. In so far as it is possible to do so, it is important to determine the origin of any given drift deposit by its *inherent* characteristics, for in the closer distinctions which the progress of this department of science now demands, stratigraphical and other extraneous characters often fail to present decisive criteria. This is especially true of the pebble and bowlder clays. Certain of them seem to be plainly of glacial formation, while others are as certainly to be assigned a different origin. The discrimination between them on the basis of inherent constitution is one of extreme difficulty, and only partial success can be expected, but the attempt is none the less important.

1. One characteristic difference between the classes of drift clays is the manner in which pebbles and bowlders are embedded in them. In subaqueous clays, lines of lamination, in some portions of the deposit, at least, are to be expected, and these are often seen to be depressed beneath the enclosed pebbles or bowlders, as if the latter had been dropped upon a yielding surface of clay,

while the laminæ above curve over the erratics conformably to them, indicating subsequent deposition. This is regarded as a decisive criterion, though not universally applicable, since all subaqueous drift is not distinctly laminated.

2. Again in subaqueous clays, the pebbles and bowlders are found to be commonly in that position in which they would be likely to strike on falling through water upon soft clay. In the true glacial clays, on the contrary, the rock fragments have been observed to have no definite or uniform position. This again, where applicable, is an important discrimination.

3. The distribution of bowlders through the two clays is another means of discrimination. In subaqueous clays the bowlders are observed to be distributed with such uniformity as the law of hazard would prescribe. In the glacial clays proper, on the other hand, there is a manifest tendency to a clustering of the bowlders. Currents would of course modify the distribution of bowlders in subaqueous clays. Other agencies also might operate to effect the same result, but the general lack of uniformity of distribution of bowlders through the one class and of clustering through the other is of wide application.

4. The surface contours of the glacial and subaqueous clays afford a still further means of distinction. The latter are, in general, characterized by plane topography, while the former generally present several distinct phases of irregularity. Aside from the peculiar surface aspects of the moraine ridges, the lenticular or elliptical hills present a distinct type of glacial accumulation. Allied to these two diverse classes, there are two prevalent systems of linear ridging of drift, the one parallel to glacial movement and the other transverse to it; the former, perhaps due to the influence of preëxisting irregularities of surface and of rock-resistance, determining a tendency to flow in channels, the latter to secular and seasonal advances of the ice. Both of them grade away into unclassifiable irregularities. Other means of discrimination, less specific, or less capable of brief statement, may be found; but combining the testimony of all, there are still certain pebbly clays that as yet elude positive identification by means of their inherent natures, whatever judgment of them may be based upon external considerations.

The gradations of one type into another present grave difficulties.

THE CHARACTER OF THE OUTER BORDER OF THE DRIFT. By T. C. CHAMBERLIN of Beloit, Wis.

[ABSTRACT.]

WEST of the Scioto valley in Ohio the extreme outer border of the drift is not usually marked, so far as the writer's observations have extended, by any definite moraine, applying that term only to ridged aggregations of drift due to the forcible action of glacier ice. East of that point on or near the margin, definite morainic ridges prevail. Outside of these is the so-called glacial fringe, a border of thin drift or scattered bowlders.

In the western interior, however, an analogous phenomenon attains a much more important development. The drift margin here graduates away to a vanishing edge. In some portions, especially in Dakota, bowlders constitute almost the entire portion positively referable to drift. These repose upon residuary material of local origin. This border of thin discontinuous drift reaches its greatest extension in Dakota, west of the Missouri river. In this region glacial clay and gravel are quite limited in quantity, the peripheral drift consisting very largely of archæan bowlders, there being but few of palæozoic limestone. These bowlders are notably clustered, the groups being gathered mainly on prominences, the intermediate depressions and even many of the eminences being comparatively free from them. The clusters lie at unequal altitudes and repose sometimes on summits and sometimes on slopes. West of the Missouri river there is little or no evidence of glacial ploughing, the topography being mainly of the drainage type. East of the river, in general, the erosion topography is noticeably subdued. The Missouri does not, however, accurately mark the transition from the one type to the other.

West of Pierre there are but few bowlders, but on the east bluffs there is an extraordinary accumulation of them. At other points, where studied, the proximate border was found marked by an unusual accumulation of bowlders, beyond which there was a scattered distribution for a considerable distance.

THE TERMINAL MORAINES OF THE LATER EPOCH. By T. C. CHAMBERLIN of Beloit, Wis.

[ABSTRACT.]

THE marginal accumulations thus far discussed mark the extreme limit of glaciation, and, in the interior regions, belong to the earlier glacial epoch.

Turning to the border of the later drift, we may recall that in 1873, an investigation of a peculiar line of drift-hills in eastern Wisconsin, locally known as the Potash Kettle Range, which it had been suggested represented an old beach line, was undertaken.

Investigation soon developed evidence, that, instead of a beach line, the range is a moraine. The region affords exceptional advantages for securing decisive data, and results essentially demonstrative are attainable. These show that the Kettle Range is an interlobate moraine formed jointly by glacial lobes occupying the valleys of Lake Michigan and Green Bay respectively. It was found that this moraine bifurcates in southern Wisconsin, one portion continuing southward, nearly parallel to the shore of Lake Michigan, while the other curves to the west and then north forming a broad loop outlining the glacial tongue that then occupied the Green-bay-Rock-river valley.

From this initial region the observations of the writer have extended along these two branches eastward and westward, and, in connection with the work of others, have shown an essentially continuous morainic chain to the Atlantic on the one hand, and the British Possessions on the other. The looped disposal of the moraine, accompanied by reëntrant angles and interlobate projections, signifying an extremely lobed ice margin, and divergent internal ice-movements, has been found to prevail throughout the interior. The distribution of these great loops was traced by the writer upon the map, and their significant relations to the great valleys indicated. The irregularities of the development of the moraine were referred to and associated with topographical relations.

In response to interrogation as to the time relations between the older and the newer drifts, Professor Chamberlin stated that one of the best bases of estimation was to be found in the amounts of erosion suffered by the regions of older and younger drifts respectively. Attention was called to the fact that the lakes are almost entirely confined to the region north of the terminal mo-eastward twenty miles, turns southerly at Brown's Valley, and enters the northwest end of Big Stone lake. Here and in its whole extent thence to its mouth, the Minnesota river occupies a very remarkable valley, the origin of which was first explained in 1868 by Gen. G. K. Warren, who attributed it to the outflow from an ancient lake that filled the basin of the Red river and Lake Winnipeg. This valley or channel begins at the northern part of Lake Traverse, and first extends southwest to the head of this lake, thence southeast to Mankato, and next north and northeast to the Mississippi at Fort Snelling, its length being about two hundred and fifty miles. Its width varies from one to four miles, and its depth is from one hundred to two hundred and twenty-five feet. The country through which it lies, as far as Carver, about twenty-five miles above its junction with the Mississippi, is a nearly level expanse of till, only moderately undulating, with no prominent hills or notable depressions, excepting this deep channel and those formed by its tributary streams. Below Carver it intersects a belt of terminal moraine, composed of hilly till. Its entire course is through a region of unmodified drift, which has no exposures of solid rock upon its surface.

Bluffs in slopes from twenty to forty degrees, and rising one hundred to two hundred feet to the general level of the country, form the sides of this trough-like valley. They have been produced by the washing away of their base, leaving the upper portion to fall down and thus take its steep slopes. The river in deepening its channel has been constantly changing its course, so that its current has been turned alternately against the opposite sides of its valley, at some time undermining every portion of them. In a few places this process is still going forward, but mainly the course of the Minnesota river is in the bottomland. Comparatively little excavation has been done by the present river. As we approach its source it dwindles to a small stream flowing through long lakes, and we finally pass to Lake Traverse, which empties northward; yet along the upper Minnesota and at the divide between this and the Red river, this valley or channel and its enclosing bluffs are as remarkable as along the lower part of the Minnesota river. It is thus clearly shown to have been the channel of outflow from a lake formerly extending northward from Lake Traverse along the Red river valley. The existence of this body of water is believed to have been due to the barrier of the receding continental ice-sheet, obstructing the natural course of drainage northward as at present to Hudson bay; and it is therefore called *Lake Agassiz*, in memory of Professor Louis Agassiz, the first prominent advocate of the theory that the drift was produced by land-ice. The shore-line of this lake, when it stood at its highest level, marked by a distinct beach of gravel and sand, upon a surface which on each side of this beach is chiefly till, has been traced continuously 175 miles, from Lake Traverse easterly to Herman and thence northerly to Maple lake, twenty miles east-southeast from Crookston. The height of this beach has been ascertained at the same time, by levelling along this entire distance.

The Minnesota valley in many places cuts through the sheet of drift and reaches the underlying rocks, which have frequent exposures along its entire course below Big Stone lake. This excavation shows that the thickness of the general drift-sheet upon this part of Minnesota averages about one hundred and fifty feet. The contour of the old rocks thus brought into view is much more uneven than that of the drift. In the hundred miles from Big Stone lake to Fort Ridgely the strata are metamorphic gneisses and granites, which often fill the entire valley, one to two miles wide, rising in a profusion of knolls and hills, 50 to 100 feet above the river. The depth eroded has been limited here by the presence of these rocks, among which the river flows in a winding course, crossing them at many places in rapids or falls. From New Ulm to its mouth the river is at many places bordered by Cretaceous and Lower Silurian rocks, which are nearly level in stratification. These vary in height from a few feet to 50 or rarely 75 or 100 feet above the river. From Mankota to Ottawa the river occupies a valley cut in Shakopee limestone underlain by Jordan sandstone, which form frequent bluffs upon both sides, 50 to 75 feet high. After excavating the overlying 125 to 150 feet of till, the river here found a former valley, eroded by preglacial streams. Its bordering walls of rock, varying from one-fourth of a mile to at least two miles apart, are in many portions of this distance concealed by drift, which alone forms one or both sides of the valley. The next point at which the river is seen to be enclosed by rock-walls is in its last two miles, where it flows between bluffs of Trenton limestone underlain by St. Peter sandstone, 100 feet high, and about a mile apart. This also is a preglacial channel, its further continuation being occupied by the Mississippi river. The only erosion effected by the Minnesota river here has been to clear away a part of the drift with which the valley was filled. Its depth at some earlier time was much greater than now, as shown by the salt-well on the bottomland of the Minnesota river at Belle Plaine, where 202 feet of stratified gravel, sand and clay were penetrated before reaching the rock. The bottom of the preglacial channel there is thus at least 165 feet lower than the mouth of the Minnesota river.

Heights of the bluffs, which form the sides of the valley, composed of till, enclosing layers of gravel and sand in some places, and frequently having rock at their base, are as follows, stated in feet above the lakes and river: along Lake Traverse, 100 to 125; at Brown's Valley and along Big Stone lake, mainly about 125, the highest portions reaching 150; at Ortonville, 130; at Lac qui Parle and Montevideo, 100; at Granite Falls, 150; at Minnesota Falls, 165; thence to Redwood Falls, Fort Ridgly and New Ulm, 165 to 180; at Mankato, 200 to 225; at Saint Peter and Ottawa, 220 to 230; at Le Sueur and Henderson, 210 to 225; at Belle Plaine and Jordan, about 230; and at Shakopee, 210 to 220. The morainic hills through which this valley extends below Shakopee are 225 to 250 feet in height. The expanse of till through which this channel is eroded slopes from 1125 feet above the sea at Big Stone lake to 975 at Mankato, in 140 miles; and thence it descends to 925 at Shakopee, in 50 miles. This channel, or valley, of the Minnesota river, lies nearly midway between the belt, on its northeast side, of medial and terminal moraines, that extends from Lake Minnetonka 150 miles northwest to the Leaf hills, and the Coteau des Prairies on its southwest side; toward each of which, some fifty miles distant from this river, there is a gentle ascent, sufficient to cause drainage to follow this central line.

The height of Lake Traverse is 970 feet above the sea; the lowest point in Brown's valley between this and Big Stone lake is only three feet above Lake Traverse; Big Stone lake is 962 feet above the sea, or eight feet below Lake Traverse; and the mouth of the Minnesota river is 690 feet above the sea, the descent from Big Stone lake to the mouth of the river being 272 feet.

Lakes Traverse and Big Stone are from one to one and a half miles wide, mainly occupying the entire area between the bases of the Bluffs, which rise about one hundred and twenty-five feet above them. Lake Traverse is fifteen miles long; it is mostly less than ten feet deep, and its greatest depth probably does not reach twenty feet. Big Stone lake is twenty-six miles long, and its greatest depth is reported to be from fifteen to thirty feet. The portion of the channel between these lakes is widely known as Brown's valley. As we stand upon the bluffs here, looking down on these long and narrow lakes in their trough-like valley, which extends across the five miles between them, where the basins of Hudson bay and the Gulf of Mexico are now divided, we have nearly the picture which was presented when the melting ice-sheet of British America was pouring its floods along this hollow. Then the entire extent of the valley was doubtless filled every summer by a river which covered all the present areas of flood-plain, in many places occupying as great width as these lakes.

Gen. Warren observed that Lake Traverse is due to partial silting up of the channel since the outflow from the Red river basin ceased, the Minnesota river at the south having brought in sufficient alluvium to form a dam; while Big Stone lake is similarly referred to the sediment brought into the valley just below it by the Whetstone river. Fifteen miles below Big Stone lake, the Minnesota river flows through a marshy lake four miles long and about a mile wide. This may be due to the accumulation of alluvium brought into the valley by the Pomme de Terre river, which has its mouth about two miles below. Twenty-five miles from Big Stone lake, the river enters Lac qui Parle, which extends eight miles, with a width varying from one-fourth to three-fourths of a mile and a maximum depth of twelve feet. This lake, as Gen. Warren suggested, has been formed by a barrier of stratified sand and silt which the Lac qui Parle river has thrown across the valley. He also showed that Lake Pepin on the Mississippi is dammed in the same way by the sediment of the Chippewa river; and that Lake St. Croix and the last thirty miles of the Minnesota river are similarly held as level back-water by the recent deposits of the Mississippi.

All the tributaries of the Minnesota river have cut deeply into the drift, because the main valley has given them the requisite slope. The largest of these extend many miles, and have their mouths level with the bottomland of the Minnesota river. The bluffs of all these valleys are also everywhere seamed and gullied by frequent rills and springs, many of which flow only after rains. Few of the large inlets have any great amount of sediment deposited opposite their mouths, showing that their excavation was mostly done at the same time with that of the main valley. The short ravines are more recent in their origin, and the material that filled their place is commonly spread in fan-shaped, moderately sloping banks below their mouths, which are kept at a height from 30 to 40 feet above the present flood-plain. The road from Fort Ridgely to New Ulm runs along the side of the bluff at the only height where a nearly level straight course could be obtained, being just above these deposits and below the ravines.

The valleys of the Pomme de Terre and Chippewa rivers, 75 to 100 feet deep along most of their course, and one-fourth of a mile to one mile in width, were probably avenues of drainage from the melting ice-fields in their northward retreat. Between these rivers, in the twenty-two miles from Appleton to Montevideo, the glacial floods at first flowed in several channels, which are excavated 40 to 80 feet below the general level of the drift-sheet, and vary from an eighth to a half of a mile in width. One of these, starting from the bend of the Pomme de Terre river, one and a half miles east of Appleton, extends fifteen miles southeast to the Chippewa river near the centre of Tunsburg. This old channel is joined at Milan station by another, which branches off from the Minnesota valley, running four miles east-southeast; it is also joined at the northwest corner of Tunsburg by a very notable channel which extends eastward from the middle of Lac qui Parle. The latter channel, and its continuation in the old Pomme de Terre valley to the Chippewa river, are excavated nearly as deep as the channel occupied by the Minnesota river. Its west portion holds a marsh generally known as the "Big slough." Lac qui Parle would have to be raised only a few feet to turn it through this deserted valley. The only other localities where we have proof that the outflow from Lake Agassiz had more than one channel are seven and ten miles below Big Stone Lake, where isolated remnants of the general sheet of till occur south of Odessa station and again three miles southeast. Each of these former islands is about a mile long, and rises 75 feet above the surrounding lowland, or nearly as high as the bluffs enclosing the valley, which here measures four miles across, having a greater width than at any other point.

Terraces and high plains of modified drift, found in many places along the valley of the Minnesota river from New Ulm to its mouth, show that it was once filled, doubtless at the close of the last glacial epoch, with stratified gravel, sand and clay, to a depth 75 to 150 feet above the present river. The remnants of this deposit include the plateau of modified drift, about a mile long and an eighth of a mile wide, upon which the west and highest part of New Ulm is built; a terrace in section 27, Courtland, opposite the southeast part of New Ulm, more than a mile long and about an eighth of a mile wide; a larger terrace, four miles long and a half mile wide, lying also in Courtland, four to eight miles southeast from New Ulm, upon which Courtland depot is situated; a terrace extending about three miles northwest from near Minneopa falls, and varying from a few rods to a third of a mile in width; a terrace three miles long east and south of Kasota; the "sand prairie," about four miles long and averaging a mile wide, west and north of Saint Peter; Le Sueur prairie, six miles long and from one to three miles wide, beginning east of Ottawa and reaching to Le Sueur; the plain five miles long and a mile wide, near the middle of which Belle Plaine is built; Spirit hill and "sand prairie," southwest and northwest of Jordan; a terrace eight miles long and varying from a few rods to two miles in width, extending through San Francisco, Dahlgren, and Carver; and Shakopee prairie, eight miles long and averaging one mile wide. The height of these terraces and plains at New Ulm is about 115 feet above the river; in Courtland, and near Minneopa falls, 125 to 150 feet; at Kasota, Saint Peter and Le Sueur, about 150 feet; at Belle Plaine, about 135; and at Jordan, Carver and Shakopee, about 125. Wells on the "sand prairie" near Saint Peter and on Le Sueur prairie go through sand and gravel, sometimes with layers of clay, to the depth of 75 or 100 feet, finding till below. At Belle Plaine the sand and gravel are about 50 feet deep, underlain by till. Shakopee prairie has 40 or 50 feet of this modified drift, lying upon limestone. The principal remnant of these deposits seen below Shakopee was a terrace about 75 feet high, an eighth to a third of a mile wide and four miles long, extending through Eagan in Dakota county, its north end being about two miles south of Fort Snelling. This valley was first excavated in till, which rises in continuous bluffs on each side 50 to 100 feet above these high plains and terraces of modified drift. It was afterward filled along this distance of one hundred miles next to its mouth with fluvial deposits 75 to 150 feet thick, sloping about two feet per mile, through which the channel has been cut anew.

With this general view of the valley of the Minnesota river as it exists to-day we are prepared to inquire more particularly what its history was during the vicissitudes of the ice age and from the close of that period until now. Its condition at the beginning of the reign of ice first claims consideration. During what ages was its preglacial rock-walled channel formed? This is in part answered by deposits of Cretaceous clay found in waterworn hollows of the Shakopee limestone forming the walls of this channel at numerous places in Blue Earth, Le Sueur and Nicollet counties, and by the occurrence in this valley in Courtland and New Ulm and near Fort Ridgely and Redwood Falls, of Cretaceous sandstone, clay and shale, occasionally containing lignite. It is thus known that before the Cretaceous age, when western Minnesota and the region of the upper Missouri were depressed and covered by the sea, a deep channel had been cut by some river in the Lower Magnesian strata of the Minnesota valley. The slopes from each side toward this channel appear therefore to have been partly like those of the present day. The direction of drainage may even have been the same as now, for the northern part of the Mississippi river and valley has probably existed since the middle of Palæozoic time.

Scanty exposures of Cretaceous strata are found in many parts of Minnesota, enclosing sometimes marine fossils, sometimes impressions of leaves, and at a few places thin layers of lignite. The western two-thirds of the state were probably covered until the glacial period by deposits of this age which have now been mainly eroded, with much from the underlying Palæozoic rocks, and constitute a part of the drift, irrecognizably mingled with detritus and boulders that have been brought from Laurentian and Huronian areas far to the north and northeast. Excepting its partial submergence by the sea in the Cretaceous age, Minnesota seems to have stood wholly above the level of the ocean from the beginning of the Carboniferous period till the present day.

Before the ice age the rocks had been long subjected to the ordinary disintegrating agencies of rain and frost. Granite and gneiss were thus generally decomposed in place to considerable depths. The loose material resulting from such decay was doubtless spread somewhat evenly over the surface, collecting to the greatest depth in valleys. Except where it had been transported by streams and consequently formed stratified deposits, the only fragments of rock held in this mass would be from underlying or adjoining rocks.

During the ice age the Cretaceous strata, mostly unconsolidated, which covered western Minnesota, the superficial deposits and decomposed rock produced by weathering, and the alluvial beds of sand and gravel along the river-courses, were mostly ploughed up by the ice and thoroughly kneaded with each other. Much detritus was also added from erosion of the harder rocks beneath. Large blocks and boulders, often already formed by the process of weathering, were borne away, and the surface of the bed-rock was worn and striated by boulders and pebbles, which were rolled and dragged along under the vast weight of ice, breaking up and grinding themselves and the underlying rock into gravel, sand, and even the finest clay.

Ledges of decomposed gneiss and granite, found in the Minnesota valley at many points from Minnesota Falls to Fort Ridgely, with their surface changed to a soft, earthy or clayey mass, resembling kaolin, and the Cretaceous beds before mentioned, both of which would readily yield to eroding agencies, show that the moving ice-sheet did not everywhere plough up all the loose material under it. A considerable depth, however, has probably been removed; and these may be scanty remnants of thick beds which covered this region generally before the glacial period. More commonly the ice-sheet removed all such material, and gathered a part of its drift from the underlying solid rocks; as is shown by their being frequently rounded, smoothed, and marked with parallel furrows and scratches, called striæ. Similarly scratched pebbles and boulders are found in the glacial drift. These were the graving tools by which the bed-rock was worn and striated. They were held firmly by being frozen in the bottom of the ice and were pushed forward by its current, which thus recorded its direction. Our observations of glacial striæ in the Minnesota valley are as follows: one to three miles southeast from the foot of Big Stone lake, S. E.; near Odessa, S. E.; at Granite Falls, in several places, S. 45°-50° E.; at the dam of O K mill, Beaver

Falls, S. 60° E.; one and a half miles west from Fort Ridgely, S. 60° E.; at Redstone, one and a half miles southeast from New Ulm, S. 25° E.; at Jordan, noted by Foss, Wells & Co., in quarrying and on the site of their mill, S. E.

Within the till are frequently found layers of sand or gravel, which yield the large supplies of water so often struck in digging wells. Probably many of these veins of modified drift were formed by small subglacial streams and therefore cannot be regarded as marking divisions of the glacial period, nor even any important changes in the overlying ice. It appears, however, by shells, remains of vegetation, and trees, found deeply buried between glacial deposits in this and adjoining states, that the ice age was not one unbroken reign of ice, but that this retreated and reädvanced, or was possibly at some times nearly all melted upon the northern hemisphere and then accumulated anew. Thus periods of ice alternated with interglacial epochs, in which animal and vegetable life spread again northward, following close upon the retreat of the ice-fields. By each new advance of the glacial sheet much of the previous surface would be ploughed up and redeposited; hence we find only few and scanty remnants of fossiliferous beds in the glacial drift. At the disappearance of the last ice-sheet these drifted materials, seldom modified by water in their deposition, formed a mantle 100 to 200 feet thick, which throughout the basin of the Minnesota river almost universally covered the older rocks.

Two principal glacial epochs can be especially distinguished, each subdivided by times of extensive recession and reädvance of the ice, as shown by features of the drift in this state. In the first glacial epoch, when the ice attained its greatest area, all of Minnesota except its southeast corner was deeply covered by the continental ice-sheet, and its border was several hundred miles south of this district, in Nebraska, Kansas, Missouri, and southern Illinois, Indiana and Ohio, extending somewhat beyond the Missouri river, but terminating north of the Ohio river, except in the vicinity of Cincinnati, where it reached a short distance across that river into Kentucky, as recently proved by Professor Wright. In the later very severely cold epoch, the ice-fields were of less extent, and terminated in the central part of the United States from fifty to three hundred miles within their earlier limit, covering all the basin of the Minnesota river, but not enveloping a large tract in the southwest corner of Minnesota and leaving uncovered a much larger area than before in the southeast part of the state. The terminal moraines which form conspicuous belts of rolling and hilly drift in Wisconsin, Minnesota, Iowa and Dakota, were accumulated at the boundaries of the ice of the last glacial epoch. Between these epochs the ice was melted away within the basins of the Minnesota and Red rivers, and probably from the entire state. The greater part of the till appears to have been deposited by the earlier ice-sheet; and during its retreat this till was overspread in some places, especially along the avenues of drainage, by beds of modified drift, or stratified gravel, sand and clay, washed from the material that had been contained in the ice and now became exposed upon its surface to the multitude of rills, rivulets and rivers that were formed by its melting.

In the principal interglacial epoch, this drift-sheet was channelled by water-courses till its valleys were apparently as numerous and deep as those of our present streams. The interglacial drainage sometimes went in a different direction from that now taken by the creeks and rivers; and the valleys then excavated in the drift, though partly refilled with till during the last glacial epoch, are still, in some instances, clearly marked by series of lakes, as in Martin county, in the south edge of Minnesota. More commonly the interglacial water-courses must have occupied nearly the same place with the valleys of the present time; and there seems to be conclusive proof that this was true of the entire valley of the Minnesota river. A long period intervened between the great glacial epochs; the earlier ice-sheet gradually retreated northward; a lake was formed in the Red river valley by the receding ice-barrier on the north; the outflow from this lake, and the drainage of the Minnesota basin itself, appear to have excavated the valley of the Minnesota river nearly as it now is; and the further recession of the ice-sheet probably even allowed the drainage of the Red river basin to take its course northward, as now, to Hudson bay, this being indicated by fossiliferous beds, containing the shells and vegetation of swamps, and trunks of trees, underlain and overlain by till, within the area that had been covered by this interglacial lake and was afterward occupied by Lake Agassiz at the close of the last glacial epoch.

Since all this, a severely cold climate again prevailed, accumulating a vast sheet of ice once more upon British America and the greater part of Minnesota. Beneath this glacial sheet the valley of the Minnesota river was partly refilled with till, but it evidently remained an important feature in the contour of the land surface. Perhaps it had been the pathway, along the lower part of its extent, of a subglacial river during the later epoch of ice. At the final melting of this ice-sheet, its waters, discharged in this channel, quickly removed whatever obstructing deposits of drift it had received and undermined its bluffs, giving them again the steep slopes produced by fluvial erosion. This partial excavation and sculpture anew were then immediately followed, during the retreat of the ice-sheet, by the deposition of the modified drift seventy-five to one hundred and fifty feet deep, remnants of which occur frequently as extensive terraces on the sides of this valley, from its mouth to New Ulm, and less distinctly beyond. Had not the great valley existed nearly in its present form through the last glacial epoch, it could not have become filled with this modified drift, which must belong to the era of melting of the last ice-sheet.

Modified drift, which was probably deposited during the recession of the ice of the earlier glacial epoch, was observed near New Ulm, forming a thick bed of stratified gravel and sand enclosed in the till. The section showing this is on the extension of Center street, a half mile west of New Ulm, where it rises to the top of the bluff, one hundred and eighty feet above the river, but only some one hundred feet above its old channel which lies between New Ulm and this bluff. The height here reached is the general level of the vast prairie of gently undulating till, through which the Minnesota valley is excavated. The grade cuts to a depth of about forty feet at the edge of the bluff, and thence ascends, with decreasing depth of cut, along a distance of about twenty-five rods, to the surface of the drift-sheet. This excavation exhibits two deposits of true till, separated by modified drift which is probably an interglacial formation, supplied at the time of final melting of the earlier ice-sheet and spread beyond its receding margin upon the unchannelled surface of the till that had been formed during that earlier part of the ice age. The upper bed of till, thus apparently representing the total thickness of the drift deposited here in the last glacial epoch, is sixteen to eighteen feet thick, and is an entirely unstratified yellowish gravelly clay, containing occasional rock-fragments up to six or eight inches in diameter, but showing only two or three of larger size, these being two to three feet in diameter. The bottom of this upper till, seen clearly exposed along a distance of about two hundred and fifty feet, is an almost exactly level line. Next below is the modified drift which is supposed to have had its origin from the melting ice-sheet of the earlier glacial epoch. Its thickness is also sixteen to eighteen feet, and consists of yellowish gravel and sand, containing pebbles up to six or eight inches in diameter, quite ferruginous in the lowest one to three feet, levelly stratified throughout, but having the horizontal layers often obliquely laminated, the dip of this lamination being to the east or northeast, toward the Minnesota river, and varying in amount from two or three to fifteen or twenty degrees. The underlying till was seen along an extent of one hundred feet, the greatest depth cut into it being about eight feet. Its upper line, separating it from the modified drift, is approximately level but undulating, with its highest points two or three feet above the lowest. This till, like the upper bed, bears no marks of stratification; and neither shows any interbedding or transition, but both are bounded by definite lines, at their junction with the intervening gravel and sand. The lower bed of till is dark bluish, excepting for about twenty feet from the face of the bluff inward, where weathering has changed it to the same yellow color that characterizes the modified drift and upper till. In other portions of the Minnesota valley, its bluffs frequently exhibit modified drift interbedded, sometimes in deposits of large extent and thickness, with the till, which makes up the principal mass of these bluffs and of the drift-sheet.

A peculiar stratification observed in several of the deposits of clay, which form part of the terraces of modified drift in the Minnesota valley in Scott and Carver counties, belonging to the time of departure of the last ice-sheet, appears to afford a measure of the rate of deposition. In Mr. Charles Rodell's excavation for brick-making at Jordan, the clay is bedded in distinct horizontal layers from three to eight inches thick, averaging six inches. These layers are dark bluish, often finely laminated, changing above and below to a nearly black, more unctuous and finer clay, which forms the partings between them. These divisions are clearly seen through the whole extent of this excavation, which reaches twenty-five feet below the top of the clay and is four rods long. The height of its top is estimated to be sixty-five feet above the river. The excavation of Nye & Co., at Carver, where the exposure is four rods long and fifteen feet high, with about the same elevation above the Minnesota river as the foregoing, exhibits the same stratification, except that here the layers all have a nearly uniform thickness of three inches. There is a tendency to split at the darker partings, which are seen to extend continuously, never passing one into another, and preserving a very constant width of three inches apart, through the whole of the section exposed. They are from an eighth to three quarters of an inch thick, gradually merging above and below into the less dark clay that makes up the principal mass of these layers. The bedding is nearly level, but dips one to two degrees away at each side. In this depth of fifteen feet there are thus about sixty layers, all closely alike. The alternating conditions which produced them were evidently repeated sixty times in uninterrupted succession. The only explanation of this which seems possible is that these divisions mark so many years occupied by the deposition of this clay. Layers nearly like those in the clay at Carver and Jordan are also seen in other clay-beds in this valley and in that of the Mississippi in this state. The principal mass of each layer is regarded as the deposition during the warm portion of a year, and the very dark partings as the sediment during winter when the glacial melting was less and the water consequently less turbid.

At Chaska, situated in the Minnesota valley two miles below Carver, the clay used for brick-making is modified drift of interglacial age. It varies from twenty to forty feet in thickness, being underlain by sand and covered by till from two to six feet thick, holding boulders of all sizes up to five or six feet in diameter, many of which are planed and striated. This till forms the surface, thirty to thirty-five feet above the river. The only fossils found here were fresh-water clam shells which occurred in considerable numbers upon a space four rods in diameter near the middle of Gregg & Griswold's excavation, lying in the upper foot of the clay, just beneath the till. This interglacial clay, overspread by till, testifies that an ice-sheet covered this region after the Minnesota valley had been eroded nearly as it now is.

Another observation which seems to give the same testimony, and to show that the modified drift forming high terraces and plains in this valley was deposited during the recession of the ice-sheet, is presented in the notably uneven surface of the broad part of the terrace of this valley drift in Carver county between Carver and Beven's creeks. On this tract, composed, below the soil, of stratified gravel and sand, extending about two miles in width and elevated one hundred and twenty-five feet above the river, are frequent depressions from ten to thirty rods in diameter and fifteen to forty feet in depth below the general level, often enclosed without outlet, and some of them containing lakelets and sloughs. Such hollows have not been seen elsewhere in our exploration of these terraces along the Minnesota valley, which instead have generally a smoothly level contour. Their origin must apparently be referred to sedimentation, while masses of ice occupied the places of these bowl-like depressions. Elsewhere the absence of such irregularities in the surface of the valley drift, as also the very rare occurrence of boulders in it, and the fact that no portion of it, excepting that just mentioned at Chaska, is known to be interglacial by having become covered with till, together show that the deposition of these beds of modified drift took place outside the limits of the retreating ice-sheet. The valley appears to have remained from excavation in an interglacial epoch, and to have become rapidly filled with sediments as soon as the ice by which it had been enveloped was melted away.

Alluvial beds fill the Minnesota valley at Belle Plaine, as shown by the section of the salt-well, to a depth about 150 feet below the present river at its stage of low water. This well, situated on the bottomland at nearly the same height with the depot, or approximately 30 feet above the river and 725 above the sea, is reported by Prof. Alexander Winchell to have passed through the following succession of deposits before reaching the bed-rock; soil and gravel, 9 feet; clay and gravel, 9 feet; sand and gravel, 18 feet; quicksand, 54 feet, having its base 90 feet below the surface; coarse sand, 1 foot; clay, 6 feet, in which was found, two feet from its top, a piece of grapevine with bark; sand, 38 feet, varying from quicksand to coarse sand, in which at 114 feet, inflowing water, under pressure from the bottom, filled the pipe twelve feet with sand, and a second time, at 125 feet, filled it five feet; then, gravel, quicksand, and coarse sand, 45 feet, having its base 180 feet below the surface, yielding water at 144 feet, which filled the pipe with sand ten feet, and containing another piece of grapevine at 168 feet; next, from 180 to 200 feet, blue clay, 7 feet, and rock-fragments, 13 feet, probably both boulder-clay or till; and, lastly, gravel, 2 feet; the whole depth of alluvium and drift being thus 202 feet, extending about 170 feet below the river.

At the railroad bridge which crosses the Minnesota river close to its mouth, borings were made to a depth of 60 feet below the river-level without reaching the bed-rock. In the deep well at Mankato, drift was found to extend 65 feet below the river.

A summary of the glacial history of the Minnesota valley, as recorded in its physical and geological features here described, is nearly as follows: This channel, excavated in the Lower Magnesian or Calciferous formations far below the bottom of the present valley, appears to have been eroded by a river during the later Palæozoic and earlier Mesozoic ages, before the Cretaceous subsidence which carried much of this state, with a large area farther west, beneath the sea. In the first principal epoch of glaciation, when the ice covered its greatest area, a thick drift-sheet, mostly unmodified, was spread over all this region, probably covering most of this preglacial valley with an unbroken, moderately undulating expanse of till. During the ensuing interglacial epoch, the drainage of this area cut a channel, which, because of the natural slopes of the basin determined by preglacial erosion, coincides along much of its lower part, where it crosses the nearly horizontal Palæozoic formations, with the old valley eroded in these strata long before the ice age. The preglacial, and probably also the interglacial river lay far below the present stream. The till of the later glacial epoch appears to have only partially blocked up this river-course along the greater part of its extent, and portions which may have been obstructed were soon channelled anew, and this valley from its mouth to New Ulm or beyond was filled with modified drift, to the height of its present terraces of this formation, during the recession of the last ice-sheet. After the departure of the ice from the Minnesota basin, this avenue of drainage continued through a long time to be the outlet of Lake Agassiz, whence it received an immense volume of water, supplied from the melting ice-fields of northwestern Minnesota and of a vast region reaching far to the north and northwest over the basin of Lake Winnipeg and the Saskatchewan river. As long as streams poured into this valley directly from the melting ice-sheet, its modified drift, gathered from the ice in which it had been held, continued to increase in depth; but when the great glacier had retreated beyond the limits of the basin of the Minnesota river, the water discharged here from Lake Agassiz brought no modified drift, and was consequently a most powerful eroding agent. By this mighty river the valley drift, so recently deposited, was mostly swept away, and the channel was excavated to a depth lower than the present river and perhaps quite to the bottom of the gravel and sand in this valley at Belle Plaine, which is 150 feet below the river there and about 140 feet below low water in the Mississippi at Saint Paul. Since the ice-barrier which had caused Lake Agassiz disappeared and that lake was drained northeastward to Hudson bay, the Minnesota valley and that of the Mississippi below, carrying only a small fraction of their former volume of water, have become considerably filled by the alluvial gravel, sand, clay and silt, which have been brought in by tributaries, being spread for the most part somewhat evenly along these valleys by their floods.

The changes produced by this postglacial sedimentation have been pointed out and ably discussed by Gen. G. K. Warren, who thus added much to our knowledge of the geological history of the Minnesota and Mississippi rivers. Lakes Traverse, Big Stone and Lac qui Parle occupy hollows in the outlet of Lake Agassiz due to inequalities of these recent deposits. At the mouth of the Minnesota river, the Mississippi has brought more sediment than its branch, which is thus dammed for a distance of thirty miles to Little Rapids, with a depth of twenty to twenty-five feet at low water. The current of this part of the Minnesota through the dry season is very sluggish or imperceptible, and its surface often becomes considerably covered with the green scum of cryptogamous vegetation characteristic of pools and lakes. The channel here is from fifteen to twenty-five rods wide, with no lake-like expansions; but lakes from one to four or five miles long, and from a quarter to a half mile wide, lie near the river and parallel with it at each side, upon the bottomland. Lake Pepin, having a depth of about sixty feet, according to General Warren, lies in the continuation of this valley which was deeply channelled by the outflow from Lake Agassiz, because it has become unequally filled below the foot of this lake by the deposition of alluvium from the Chippewa river. Two of the tributaries of the Mississippi from the east were similar outlets of floods supplied by glacial melting after they had become free from their modified drift by flowing through a lake. Lake Superior, held by an ice-barrier on the northeast at a level about 500 feet above its present height, overflowed at the head of the Bois Brulé river, by Upper St. Croix lake and the St. Croix river. The Mississippi valley at the mouth of this river, as in the case of the Minnesota river, has become more filled by postglacial deposits than its tributary, which is thus held as back-water twenty miles, to the head of Lake St. Croix, which is twenty-five feet deep. Lake Michigan, till the receding ice-sheet was melted from its present outlet at the north, similarly discharged southward by the Illinois river, which, like the foregoing, is obstructed at its mouth by the alluvium of the Mississippi. At low water the greater part of its length is dammed, and has a very slight and often imperceptible current through the two hundred miles from La Salle by Lake Peoria to its mouth. Major Long remarked: "This part of the river may, with much propriety, be denominated an extended pool of stagnant water." All these results of recent fluvial action show that the drainage from the final melting of the ice-sheet excavated these valleys to depths much lower than they have now, and make it very probable that the deposits penetrated by the first 180 feet of the Belle Plaine salt-well are wholly postglacial.

The exposures of rock over which the Minnesota river flows at Little Rapids, ten miles below Belle Plaine, do not forbid this conclusion, for the topography of the valley in that vicinity indicates that a much deeper channel than that now occupied by the river may have existed there since the ice age, passing a mile and a half east, and a mile northeast of the Little Rapids. This course of the river along which it is believed to have cut a channel in the easily eroded Jordan sandstone to a depth equal to that of the fluvial deposits penetrated by the well at Belle Plaine, extends northeastward diagonally across section five, Sand Creek; then northerly through the west part of section thirty-three, Louisville; and thence northwesterly through section twenty-nine. It thus leaves the present river a mile south of these rapids, and returns again to it about a mile south of Carver, after passing east and north of the island-like sandstone outcrops of section thirty-two, Louisville. The recent accumulation of sediments that fill this avenue to a height slightly above the Little Rapids, has turned the river that way, so that it has abandoned its former course and now flows over ledges of sandstone.

Four features of the glacial drift in Minnesota seem to me very impressive and even grand. These are, first, the great thickness of the drift and its extent over large areas where it conceals and deeply covers all the surface of the older bed-rocks; second, the terminal moraines; third, Lake Agassiz; and, fourth, the outlet from this lake, which has been the theme of the present paper. Professors J. D. Dana and J. W. Spencer, to the latter of whom we are indebted for valuable work in the Quaternary geology of the region of lakes Erie and Ontario, have expressed the desire, in which I heartily join, to recognize in the nomenclature of American geology the work of the engineer who in the years 1866 to 1869 made a survey of the Minnesota valley from Big Stone lake to its mouth, and from whose observations and writings, already several times referred to, this essay has received most important contributions and suggestions. Therefore it seems fitting to propose here for the ancient river which flowed in the Ice Age where lakes Traverse and Big Stone and the Minnesota river now are, this name, the *River Warren*, in honor and *in memoriam* of Gen. G. K. Warren, the author of the first adequate description of this valley.

CHANGES IN THE CURRENTS OF THE ICE OF THE LAST GLACIAL EPOCH IN EASTERN MINNESOTA. By WARREN UPHAM of Minneapolis, Minn.

THIS paper is based upon my observations as assistant on the geological and natural history survey of Minnesota, under the direction of Prof. N. H. Winchell, the state geologist. It has been previously read, May 8, 1883, before the Minnesota Academy of Natural Sciences.

When the ice of the last glacial epoch attained its maximum extent, it appears that the ice-current moving southwestward from lake Superior across the northeast part of Minnesota, spreading a reddish till containing boulders and pebbles peculiar to the region from which it came, had its limit at a line reaching from lake St. Croix southwesterly across the Mississippi and through the north part of Dakota county, thence bending to a northwest direction and continuing by lake Minnetonka and through Wright and Stearns counties. At the same time another portion of the ice-sheet was pushed from the region of lake Winnipeg and the Red river valley toward the south and southeast, meeting and opposing the ice-current from lake Superior along a line from Stearns county southeast by lake Minnetonka to Crystal lake in Dakota county, beyond which its eastern limit farther south was at the outer or eastern belt of the terminal moraine as traced in Rice, Steele and Freeborn counties and onward to central Iowa. The angle formed by the margins of these portions of the ice-sheet, moving respectively from the northeast and the northwest, was at Crystal lake and Buck hill in Burnsville, Dakota county; from which point northwesterly, along an extent of a hundred miles or more, these ice-currents were pushed obliquely against each other.

At a later date in this last glacial epoch, after the ice-border had for the most part retreated considerably from its farthest limits, it is known to have halted in its recession (and it even probably reädvanced), forming a second belt of morainic accumulations usually from five to fifteen miles back or inside from its former line of drift-hills. This second morainic belt is well exhibited from Rice county south to central Iowa, and at the west side of this lobe of the ice-sheet upon the Coteau des Prairies. At this time the ice-sheet that was pushed southwestward from lake Superior and northern Wisconsin probably terminated only a few miles back from its earlier limit east of lake St. Croix and in Washington, Dakota and Ramsey counties, and in the east edge of Hennepin county at Minneapolis. But farther northward the presence of bluish till, weathered next to the surface to a yellowish color, containing boulders and pebbles of limestone and of Cretaceous shale and other material brought from the northwest, overlying the red till with rock-fragments from the region of lake Superior, proves that the ice-current from the northwest was stronger and extended farther, the ice on this side of the great western lobe of the ice-sheet being therefore even deeper, at least in comparison with the thickness of the eastern ice flowing southwestward from lake Superior, than in the former part of this glacial epoch when these ice-fields covered their greatest area.

In that earlier part of this epoch the ice-currents from the northwest and northeast had met along a line drawn from Crystal lake and lake Minnetonka northwestward; but now, when elsewhere the border of the ice-fields had somewhat retreated and formed the second and inner terminal moraine, the ice flowing from the west extended eastward across Wright and Hennepin, Sherburne and Anoka counties, to the St. Croix river at the east side of Chisago county, and into the adjoining edge of Wisconsin, pushing back the ice-current that came from lake Superior, and covering the red till brought by that ice with the characteristic blue till brought by the ice from the northwest and west.

The cause of this changed course of the line at which the currents of the west and east portions of the ice-sheet met is to be found in the changed meteorological conditions of this time. During the increased ice-melting attendant upon the recession of the ice-fields from the outer to the inner terminal moraine, the prevailing westerly winds sweeping over the western side of the ice-sheet upon the Coteau des Prairies and eastward became more laden with moisture than in the earlier part of this epoch when there was comparatively little melting upon the surface of the ice; and the increased temperature enabled these winds to carry their moisture farther than when the ice had its greatest extent. Then the precipitation of rain and snow took place more upon the western side of the ice; but at this later time the precipitation, by reason of the causes here mentioned, probably became much greater than before upon the east part of the lobe of the ice-sheet that extended southeastward from the Red river valley to central Iowa. Before this, lake Minnetonka and central Wright county had been the limit where this ice-flow was stopped by the opposing ice-current from lake Superior; but now, because of the relatively and perhaps absolutely greater thickness of this part of the ice flowing from the northwest, due as shown to climatic changes, its current pushed back that opposed to it on the east, covering the red till brought by that ice with blue till containing plentiful limestone boulders and other material from the west and northwest.

The line where these ice-fields moving from the west and from the northeast now met, lies in the south edge of Mille Lacs, Kanabec and Pine counties, and even beyond the St. Croix river at the east side of Chisago county, fully seventy-five miles east of the line where these currents formerly met; but it scarcely reaches into Washington and Ramsey counties, which remained covered with ice that came from the northeast. This persistence of the ice-flow from the northeast near the margin of the ice-covered area, and also in Stearns and Morrison counties, north of the changed portion of the line of confluence of these ice-fields, proved by the character of the drift upon these counties, seems yet quite consistent with this explanation, by meteorological causes, of the change in source of the ice covering the intervening district, from Wright and Hennepin counties eastward to the St. Croix river and the edge of Wisconsin.

Details of sections in the drift showing the blue (or next to the surface yellow) till overlying the red till in this district, and more complete discussion of the glacial period, its ice-sheets, and their various drift deposits in this state, have been partially presented in the fifth, sixth, eighth and ninth annual reports of the geological and natural history survey of Minnesota, and will be fully exhibited in the final reports of this survey.

THE KAME RIVERS OF MAINE. By GEORGE H. STONE of Colorado Springs, Colorado.

[ABSTRACT.]

NEARLY all glacialists now agree that most of the kame gravels of the drift region were deposited by glacial streams. There remains a difference of opinion as to whether these streams were sub- or super-glacial; but for the sake of brevity no reference is here made to the literature of the subject. In my studies of the drift in Maine, I think I have found evidence of both kinds of streams.

During an exploration of Maine which continued for about five years. I had opportunity to examine many thousands of sections of the till throughout the greater part of the state. I have never found stratified or even water-classified material enclosed in this formation, except within a few miles of the coast, or so near to the surface as to leave the interpretation in doubt. Hundreds of miles of the beds of the inland streams have been examined by me, and if water-washed material exists in the till in the interior of the state, it must be rare. But near the coast, as for instance at Portland, one may see small masses of sand, nearly or quite surrounded by the unmodified till, the clayey till being in such contrast with the clean, silicious sand, that the difference in composition and mode of formation is seen at once unmistakably. These masses of sand and gravel in the till are often not now stratified, but all have been plainly classified by running water and not afterward so mixed with the clayey till as to lose their identity. If a ribbon or pocket of sand, not an inch in thickness, can be thus plainly distinguished from the amorphous till, surely the absence of such deposits from the great interior region is a strong indication that the conditions were there different from what they were near the coast. A somewhat positive indication of what this difference was is found in the existence of pot-holes near the coast in situations where no ordinary surface streams can ever have flowed. These pot-holes must be regarded as the work of glacial streams. In the interior no pot-holes have been found except in the valleys of ordinary streams.

Before interpreting these facts, let us consider the causes of the melting of a glacier. The white color and porous structure of the surface ice of a glacier show where most of the melting takes place. This superficial water flows along the surface, or it may wear and melt a deeper and deeper channel until it disappears down some crevasse and becomes a sub-glacial stream. Modern glaciers show a difference in their glacial streams. Near the lower extremity almost all the water is sub-glacial. The ice is so broken by crevasses that the melting waters almost immediately plunge to the bottom. But as we go backward toward the line of perpetual snow, where the ice is thicker and the melting less rapid we find streams upon the surface, the length depending upon the extent of ice free from crevasses. How far do these conditions represent those of the great glacier of eastern North America?

Without some limitation, it is doubtful if the analogies of any modern glaciers, even of Greenland glaciers, can be admitted as valid in the case of the great ice-sheet. The Greenland glaciers do not wholly overlap large areas like the great ice-sheet, but here and there peaks emerge from them which form crevasses and assist the waters to pass beneath the glacier. However, waiving these objections, and admitting for the time that the sub-glacial streams of our great glacier were as well developed as those of the Green land glaciers of to-day, at the same time we must admit that the surface streams were as well developed. Now the superficial streams of the Greenland glaciers are often of considerable size; therefore we must believe that large surface streams existed upon the ice-sheet at a proper distance north from its terminus. It should be noted that this is the condition of the Greenland glaciers during a time of comparative stability, or, quite probably, of their slow increase in extent and depth.

But what would happen during the final melting of the ice-sheet? All substantially admit that this melting was very rapid and that it took place upon a great width simultaneously, although the ice, being thicker toward the north and in a colder mean climate, would not become melted so quickly as farther south. As I have elsewhere pointed out, no southward motion would be possible over a large part of Maine, after the thickness of the ice was reduced to about five hundred feet, on account of transverse ranges of hills. Nowhere except in the White Mountains have I found traces of glaciers following local valleys. Apparently, almost all flow ceased after the melting had so far proceeded that the hills appeared above the ice. Under these conditions no new lines of crevasses would open in the thinning ice-fields, even after the hills began to emerge. The melting took place so rapidly, and the motion of the ice ceased so completely that there would be no time for the formation of sub-glacial channels. On the contrary, the exigency of glacial melting, supplemented by the summer precipitation, would cause a great extension of the superficial streams.

The presence of glacial pot-holes in Maine near the coast, and the masses of water-washed matter in the till, show that the glacial streams there reached the ground. The most of the area of sub-glacial streams must have lain south of the present coast-line. The glacier reached out into the Gulf of Maine, and off its front were probably numerous boiling springs of fresh water, like those at the submerged extremities of the Greenland glaciers. The ice over the great interior region of the state was for the most part drained by superficial streams. When the melting had so far proceeded that the bottoms of these streams reached the moraine stuff which was scattered through the lower part of the ice, then the kame-gravel began to gather on the bottoms of their channels. Several of these kame-rivers were a hundred or more miles long, as is shown by my map of the kames of Maine, published in the *Proceedings* of this Association for 1880. Most of the kame-rivers have been described by me as probably flowing in channels formed superficially on the ice, and that is my opinion still. Near the coast these channels may originally have been of sub-glacial origin. Northward there was a region where the drainage of the glacier was chiefly by surface channels which finally found their way beneath the ice at the southern limit of this region. The final melting of the great glacier took place largely at the surface, and this melting together with the large rainfall, caused great bodies of water to collect on the ice, finally wearing and melting deep channels. In some cases these streams probably followed previously formed sub-glacial channels for a part of their course, widening them until the ice-arch collapsed by its own weight.

We may grant as great a development of sub-glacial streams in the ice-sheet as anyone can reasonably consider is demanded by the Greenland glaciers, and yet I cannot see how this furnishes any adequate representation of the conditions during the final melting, when there was such a vast accession of surface water, and also when the ice-flow had so far ceased as to make it certain that few if any additional crevasses would be formed. Under these circumstances there could be no time to extend the previously formed sub-glacial channels far, and the sudden floods would find their way over the lowest parts of the ice just as they would over the ground. A great and sudden northward extension of the superficial streams would be the result.

The above reasoning applies to Maine only, and other regions are to be studied by themselves. Part of the reasoning will probably apply to most or all of the glaciated area of North America, but with many differences in the details. Indeed, as glacialists come to better appreciate the great variety of work which the ice-sheet performed, the less likely shall we be to fight for the privilege of imposing the local conditions, with which we are most familiar, upon the whole drift-covered area.

ON GLACIAL CAÑONS. By W. J. McGEE of Washington, D. C

[ABSTRACT.]

GLACIAL cañons are characterized by several peculiar features, of which the greater number suggest that glaciers are most effective engines of erosion, while others indicate that glacial erosion is inconsiderable. Neither the source of this apparent discrepance nor the full significance of the characteristic features of such cañons can be well determined empirically; but on approaching the subject from the analytical side, it appears, from detailed consideration of the several agencies involved in the development of the leading features, that the effect of *temporary* occupancy of a typical water-cut cañon by glacier ice will be to (1) increase its width, (2) change the original V to a U cross profile, (3) cut off the terminal portions of tributary cañons and thus relatively elevate their embouchures, (4) intensify certain irregularities of gradient in the cañon-bottom, (5) excavate rock-basins, (6) develop cirques, and, in general, transform such cañon into an equally typical glacial cañon.

CLAY PEBBLES FROM PRINCETON, MINN. By N. H. WINCHELL of Minneapolis, Minn.

[ABSTRACT.]

THIS paper was accompanied by an exhibition of specimens. The pebbles were of various shapes and sizes, most of them somewhat cylindrical. Outside, they are coated with fine sand and gravel; inside, they consist wholly of a fine sedimentary clay, such as is deposited by standing water, and contain no interior pebbles. Professor Winchell had compared these with pebbles of till found in deposits of gravel and sand and with various others, without finding anything exactly similar.

Professor Newberry examined the pebbles, and admitted that they were not exactly like any that he had seen, but he thought they bore a general resemblance to pebbles found throughout the range of the Carboniferous strata wherever there is a bed of sandstone capped by clay. Professor Claypole claimed to have seen similar specimens in the drift of Pennsylvania.

THE PRE-CAMBRIAN ROCKS OF THE ALPS. By T. STERRY HUNT of Montreal, Canada.

[ABSTRACT.]

THE writer began by reviewing the history of Alpine geology, and noticed first that speculative period when the crystalline rocks of the Alps, including gneisses, hornblendic and micaceous schists, euphotides, serpentines, etc., were looked upon as altered sedimentary strata of carboniferous or more recent times. He then traced the steps by which these views have been discarded, and more and more of the rocks shown to belong to eozoic or pre-Cambrian ages. In this connection the labors of von Hauer, Gerlach, Heim, Favre, Renevier, Lory, Gastaldi and others were analyzed, and reference was made to the great progress since the writer, in 1872, published a review of Favre's work on the geology of the Alps.

The sections by Neri, Gerlach and Gastaldi in the Western, and those of von Hauer in the Eastern Alps were described, and it was shown that all these agree in establishing in the crystalline rocks four great divisions, in ascending order: 1st. The older granitoid gneiss with crystalline limestones, graphite, etc., referred by Gastaldi to the Laurentian. 2d. The so-called *pietre verdi*, or greenstone group, consisting chiefly of dioritic, chloritic, steatitic and epidotic rocks, with euphotides and serpentines, including also talcose gneisses, limestones and dolomites, and regarded by Gastaldi as Huronian. 3d. The so-called recent gneisses of von Hauer and Gastaldi, interstratified with and passing into granulites and micaceous and hornblendic schists, also with serpentines and crystalline limestones. 4th. The series of argillites and soft glossy schists with quartzites and detrital sandstones, including also beds of serpentine with talc, gypsum, karstenite, dolomite, and much crystalline limestone. This fourth series, well seen at the Mount Cenis tunnel, is still claimed by Lory and some others as altered trias, but the present writer's view, put forth in 1872, that it is, like the preceding groups, of eozoic age, was subsequently accepted by Favre, and by Gastaldi, and is now established by many observations. To this horizon belong the crystalline limestones of the Apuan Alps, including the marbles of Carrara.

The writer next recalls the fact that he, in 1870, insisted upon the existence of a younger series of gneisses in North America, alike in the Atlantic states, in Ontario, and to the northwest of Lake Superior. These, in his address before the American Association for the Advancement of Science in 1871, he farther described under the name of the White Mountain series, and subsequently, in the same year, called them Montalban. These rocks were then declared to be younger than the Huronian, and to overlie it; though, in the absence of this latter, it was pointed out that in Ontario and in Newfoundland the Montalban reposes unconformably upon the Laurentian. When these newer gneisses and mica-schists were first described, in 1870, there was included with them an overlying group of argillites, quartzites and crystalline limestones, and for the whole the name of Terranovan was suggested provisionally; but in defining in the following year the White Mountain series, this upper group was omitted, and was subsequently referred to the Taconian series,— the Lower Taconic of Emmons, and the so-called altered Primal and Auroral of H. D. Rogers, in Eastern Pennsylvania.

The writer next described his own observations in the Alps and the Apennines in 1881. He affirms the correctness of Gastaldi in referring the groups one and two to Laurentian and Huronian, finds the third, or the younger gneiss and mica-schist group of the Alps, indistinguishable from the Montalban, and regards the fourth as the representative of the American Taconian. It was maintained by Gastaldi that these pre-Cambrian groups of the Alps underlie directly the newer rocks of northern and central Italy, forming the skeleton of the Apennines, reappearing in Calabria, and moreover protruding in various localities in Liguria, Tuscany and elsewhere. The serpentines, euphotides and other resisting rocks, thus exposed, have been regarded as eruptive masses of triassic and eocene time. The writer, however, holds, with Gastaldi, that they are indigenous rocks of pre-Cambrian age exposed by geological accidents.

The uncrystalline rocks of the mainland of Italy are chiefly cenozoic or mesozoic, and the only paleozoic strata known are carboniferous, the organic forms of the limestones of Chaberton having been shown to be triassic. Triassic, liassic, cretaceous, eocene and miocene strata are found in different localities resting on the various pre-Cambrian groups. In the island of Sardinia, however, all these are overlaid by a great body of uncrystalline lower palæozoic rocks, in which the late studies of Bornemann and Meneghini have made known the existence of a Lower-Cambrian fauna, including Paradoxides, Conocephalites and Archeocyathus; succeeded by an abundant fauna of Upper Cambrian or Ordovician age. In the island of Elba, according to Lotti, rocks containing the latter fauna repose directly upon the crystalline schists with serpentines.

The existence of the younger or Montalban gneiss in Sweden, and in the Harz and the Erzgebirge was noticed, and to it were referred the Hercynian gneisses and mica-schists of Gümbel. The presence both in Sweden and in Saxony of conglomerates, as described by Hummel, and by Sauer, wherein pebbles of the older gneiss are enclosed in beds of the younger series, was discussed, and the direct unconformable superposition of the latter upon the older gneiss, in the absence of the Huronian, was considered; evidences of the same relations being adduced from the Alps. The gneisses of the St. Gothard, as seen on the Italian slope, were also referred to the newer series, and the important studies of Stapff, in this connection, were discussed. It was declared that the views put forth by the author in 1870–71 on the relations and succession of the crystalline stratified rocks in North America, and then extended by him to Europe, have been fully confirmed by the labors of a great many European geologists, as already shown. Those of Hicks, Hughes, Bonney, Callaway, Lapworth and others in the pre-Cambrian rocks of the British islands were cited in support of these conclusions. It was said that whatever may have been the conditions under which these vast series of crystalline stratified rocks were deposited, there is evidence in the similarity of their mineralogical and geognostical relations, of a remarkable uniformity over widely separated regions of the earth's surface, as well as of long intervals of time, marked by great foldings and disturbance, and by vast and wide spread erosion of the successive series of rocks.

In conclusion, the writer took occasion to call attention to the important labors of the present school of Italian geologists, and their great zeal, skill and disinterested service, as shown in the memoirs of the R. Accademia dei Lincei, and in the work of the Royal Geological Commission, including the special studies, maps and memoirs prepared by it for the International Geological Congress of Bologna in 1881. The new Geological Society of Italy, founded at the same date, gives promise of a brilliant future, and has already published many important memoirs.

THE SERPENTINE OF STATEN ISLAND, NEW YORK. By T. STERRY HUNT of Montreal, Canada.

[ABSTRACT.]

THE serpentine of Staten Island appears as a north-and-south range of bold hills, rising out of a plain of mesozoic rocks. On the west side are triassic sandstones like those of the adjacent mainland, including a belt of intrusive diabase, and on the east the overlying and nearly horizontal cretaceous marls, which are traced south and west into New Jersey. The only rocks besides those here mentioned, seen on the island, are small areas of coarse-grained granite, having the characters of a veinstone or endogenous mass, and others of an actinolite-rock, both exposed among the sands on the northeast shore of the island.

Mather, who described this locality forty years since, looked upon the serpentine as an eruptive rock, like the parallel belt of diabase seen to the west of it, but Dr. N. L. Britton, of the New York School of Mines, who in 1880 published in the Proceedings of the New York Academy of Sciences a description and a geological map of the island, regarded the serpentine as similar to that which is stratified as a contemporaneous member in the ancient gneissic series of Manhattan island, and appears also at Hoboken; a view which is doubtless correct.

The appearance of isolated hills of serpentine rising among newer rocks is common in other regions, and is by the writer attributed to the fact that this insoluble magnesian silicate resists, to a great degree, the action of subaërial decay, which converts gneisses and other feldspathic rocks into a clay, that is easily removed; leaving the beds and lenticular masses of interbedded serpentine in relief. In many parts of Italy, where ridges or belts of serpentine protrude in the midst of tertiary strata, they have been des-

cribed as eruptive masses. The question of their geognostical relations is there often complicated by the fact that subsequent movements of the earth's crust have involved alike the underlying serpentines and the newer strata, and have given rise to faults and inversions, by which the younger rocks, overturned, are made to dip towards and even beneath the older. This condition of things the writer illustrated by references to localities of serpentine lately examined by him in Liguria, and in Tuscany, where the true geognostical relations of these rocks was first indicated by Gastaldi. The structure above described was further explained by reference to the similar inversions of strata to be seen along the western base of the Atlantic belt, from the Highlands of the Hudson southward, along the Appalachian valley.[1]

The writer stated that although serpentine, under subaërial influences, decays much less rapidly than the harder gneisses, it does not entirely escape this process. He showed that this rock on Staten Island is covered in parts with a decayed layer, including portions of limonite, separated by a process of segregation in pre-glacial times, since in the subsequent erosion it has been removed from many parts of the serpentine-belt. The details of this decomposition of the serpentine, and of its relation to glacial erosion have been discussed by the writer in an essay on rock-decay, to appear in the *American Journal of Science* for September, 1883. He acknowledged, in conclusion, his obligations to Dr. Britton for his careful descriptions, and for his personal guidance on Staten Island.

ON THE GENESIS AND CLASSIFICATION OF MINERAL VEINS. By J. S. NEWBERRY of New York, N. Y.

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[1] The history of the serpentine of Staten Island and of other American localities, as well as those of Italy, will be found treated at length in the author's paper entitled The Geological History of Serpentines, with notes on pre-Cambrian Rocks; which appears in the *Transactions of the Royal Society of Canada* for 1882-1883, Vol. I, pp. 165 - 215.

On the geological map of the state of New York, published "by legislative authority" in 1842, the serpentine of Staten Island, the diabase adjacent, and that of the Palisade range, are alike colored as "Trap Rocks."

ON THE HAMILTON SANDSTONE OF MIDDLE PENNSYLVANIA. By E. W. CLAYPOLE of Buchtel College, Akron, O.

[ABSTRACT.]

THE Hamilton sandstone is a peculiar local development in the middle of a group composed generally, in eastern North America, of soft shales. The middle of the Hamilton group begins to grow sandy near the Schuylkill River, the proportion of sand increasing to the southwest, to the Susquehanna, when it consists of a heavy bed of sandstone 800 feet thick and very solid. Hence its sand continues but grows somewhat less in quantity to its last appearance at twenty miles farther to the southwest. The breadth of this belt of sand is not great. It rapidly shales off to the northeast and is scarcely recognizable thirty miles from the Blue Mountains its most southerly exposure.

The Hamilton sandstone may consequently be regarded as an elongate fan-shaped mass of sand intercalated in the Hamilton shales and thinning off in all directions from a single point. So far as it is possible to determine it, this point is on or near the southeastern point of Perry County.

There is no doubt that this sandstone once extended farther to the southward but the enormous erosion that has taken place has removed all traces of it south of the Blue Mountains ; yet what remains is enough to enable us to form an idea of its extent when deposited.

The question naturally arises, what was the source of this vast mass of sand deposited in this place and in this form?

The distribution does not resemble that of the other sandstones in the county. It is comparatively small in extent and far from uniform in thickness and consistency. The obvious cause that first suggests itself is that we have the deposit of a large river of that day which brought down from a continent a quantity of sediment whereof the coarser part was dropped close or near to shore and the finer carried off to a greater distance. In that view of the case the river must have been near the site of the present Susquehanna but must have flowed in an opposite direction. And the source of the material must have been a large tract of land lying to the south or southeast of Perry County, the mouth of the ancient stream being near the present site of Harrisburgh. Such a tract of land may have consisted of the area of primal sandstone in York and Lancaster counties if at that day exposed, which it would be difficult to disprove, or else the great area of gneiss ranging southwest from Philadelphia. In either case the supply of sand would be ample.

Several facts may be quoted in support of the opinion above advanced. In the southeast of Perry County the geological column is incomplete as compared with other parts. There the Hamilton sandstone lies on the top of the Onondaga red shale, all the intermediate rocks being absent, namely, the following:

Hamilton lower shale	400 feet.
Marcellus black shale	100 "
" iron ore	100 "
" limestone	
" shale	
" lower ore	
Oriskany sandstone	40 "
Limestone shale	200 "
Limestone	100 "
	940

with part of the upper beds of the Red Shale. It is a fair inference that the part of the county over which these beds do not occur was dry land during the latter part of the interval expressed by their absence, and that during all this latter part erosion of the beds deposited during the former part must have taken place. Supposing that some geographical change toward the close of the dry land period had the effect of throwing the drainage over this district by diverting or developing the river above suggested, we have a sufficient cause for the production and deposition, of the sand. Subsidence is the usual accompaniment of deposition and as the accumulation of sand and silt increased the region subsided extending the area covered until finally the water became again deep, the deposit of sand ceased, and was succeeded by a deposit now forming the Hamilton upper shale.

It is not necessary to assume any elevation of the land as a cause of the dry ground spoken of above in the southeast of Perry County, etc. Some irregularity in the subsidence which all now admit accompanied the enormous Appalachian deposit may have drawn the water down leaving certain districts of which this was a portion above sea level for a time.

This part of Perry County is the most ancient dry land which geology has yet indicated in the district. All else to the earliest accessible times was open sea.

THE EQUIVALENT OF THE NEW YORK WATER-LIME GROUP DEVELOPED IN IOWA. By A. S. TIFFANY of Davenport, Iowa.

[ABSTRACT.]

THE horizon of the even-bedded magnesian buff limestone of the Le Claire and Anamosa quarries has been a question of uncertainty and dispute and for more than twenty years organic remains have not been found. Prof. C. A. White, Vol. I, p. 182, says:

> In the former report of the Geology of Iowa, two distinct formations of the Niagara Group were recognized and described, namely, the Niagara limestones and the Le Claire limestones, the latter resting upon the former. Subsequent stratigraphical and palæontological examinations fail to show any good reasons for referring any of the Upper Silurian rocks of Iowa to any other than the epoch of the Niagara limestones; therefore they are all regarded as belonging to that formation alone. Messrs. Worthen and Meek, of the State Geological Survey of Illinois, long since came to the same conclusion, and in the recently published State reports, they reaffirm it.

Organic remains not having been discovered by these several geologists they were left with only lithology and superposition to determine the horizon. The superposition cannot be studied successfully at Le Claire.

There are, however, many places in Scott, Cedar, Clinton, and Jones counties where the superposition shows that it is superior to the Niagara. The Niagara formation in Iowa had been land, and consequently subject to denuding agencies, for a long period before the formation under consideration had begun to be deposited. The Niagara deposit had been deeply scored and grooved by the channels of drainage.

During this period many hundreds of feet of Upper Silurian rocks were deposited in the state of New York. When subsidence had brought the Niagara below the ocean again, the deposition of the rocks under consideration took place, filling the interstices in the eroded Niagara group. Being thus protected by the older and harder Niagara rocks, they have escaped subsequent erosion, while the higher portions, which once connected these numerous outliers, have been swept away.

Prof. Hall, in the New York report of the Geology of the Fourth District, p. 142, Fig. 57, in describing the Water-lime group, figures some acicular crystals of sulphate of baryta which are numerous in this group of rocks at Anamosa.

The writer had been diligently seeking for organic remains for more than twelve years, and was at last rewarded in February, 1882, by finding great numbers of fossils in the stone from the base of the Anamosa quarries, all of which are of the type of the Water-lime group, among which the following were determined:[1] *Avicula* (*Pteronites*) *rugosa* Conrad; *Meristella sulcata* Vanuxem; *Leperditia alta* Conrad; Geol. of the N. Y. Fourth District, p. 142. The fossils were exhibited.

THE "EARTHQUAKE" AT NEW MADRID, MO., IN 1811, PROBABLY NOT AN EARTHQUAKE. By JAMES MACFARLANE of Towanda, Penn.

[ABSTRACT.]

AFTER dwelling upon the fact, that the locality of the alleged earthquake was not the seat of any apparent volcanic action, the author proceeded to state his view that the event in question was due to a different cause. He believed that what took place was a subsidence, due to the solution of underlying strata. He alluded to the descriptions afforded by Humboldt and Lyell, the latter having visited the locality, and given it a careful examination. The inhabitants described it as a convulsion, taking place at intervals during three years; creating new lakes and islands, changing the face of the country. The graveyard was precipitated into the Mississippi river; forest trees were tilted in all directions; vast volumes of sand and water were discharged on high.

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[1] Since the above was written the *Spirifera Vanuxemi* Hall has been collected from the above formation.

The author presented the following among other evidence that the phenomena proceeded from mere subsidence, and not from earthquake shocks. Their long continuance, from 1811 to 1813; their progression from place to place, ending nearly one hundred miles east of where they began; that they were never repeated in the same spot; that none of the ordinary phenomena of earthquakes occurred, except subsidence; that no great alluvial region, like this, was ever visited by an earthquake; that earthquakes do not occur so far from the seashore; and that the falling in of the Wade farm of 500 acres on the shore of Reel-Foot Lake, in January, 1883, was a continuation, on a small scale, of the supposed earthquakes of 1811–13.

In respect to the geology of the region, he stated that New Madrid and its vicinity rested on tertiary or quaternary strata. Sub-carboniferous formations are represented near the borders of the depression. Many of the older formations, by which this quaternary delta must be deeply underlaid, are of a soluble character, as is shown where they are exposed in the interior of Kentucky. Similar subsidences have evidently taken place in nearly every limestone country; the remarkable thing about this being that it occurred in modern times and before eye-witnesses.[1]

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[1] For discussion of this paper see Science for Sept. 7, 1883.

COMPARATIVE STRENGTH OF MINNESOTA AND NEW ENGLAND GRANITES. By N. H. WINCHELL of Minneapolis, Minn.

[ABSTRACT.]

HAVING had recent occasion to test the qualities of the building stones of Minnesota, the author subjected them to the usual tests of crushing, using for this purpose specimens of two-inch cube. The specimens included sandstones, limestones, granites and trap-rocks, and numbered about 100. Great care was taken in preparing them accurately. They were sent to Gen. Gillmore at Staten Island and there subjected to the tests, which were applied by crushing the samples, one in the direction of the schistose structure and one across it. The following were the results with twenty samples of Minnesota granites:

Kind of stone.	Location of quarry.	Position.	Strength in pounds.	
			Of sample.	Per cubic inch.
Dark trap-rock, massive melaphyr	Taylor's Falls, Chisago county	On bed . On edge,	105,000 105,000	26.250 26,250
Dark trap-rock, from a dyke.	Tischer's creek, near Duluth, St. Louis county,	On bed . On edge,	105,000 105,000	26,250 26,250
Gray gabbro, massive, fine.	Rice's Point, Duluth, St. Louis county	On bed . On edge,	109,000 105,000	27.250 26,250
Red, fine sienite	Beaver Bay, Lake county	On bed . On edge,	106.000 103,000	25,000 25,750
Red quartzose sienite	Watab, Benton county	On bed . On edge,	103.000 103,000[1]	25,750 25,750
Red quartzose sienite	East St. Cloud, Sherburne county	On bed . On edge,	112.000 105,000	28,000 26,250
Red quartzite	Pipestone City, Pipestone county	On bed . On edge,	111,000 108,000	27,750 27,000
Massive gray quartzose sienite	East St. Cloud, Sherburne county	On bed . On edge,	105.000 103,000	26.250 25,750
Fine-grained gray sienite.	East St. Cloud, Sherburne county	On bed . On edge,	112.000 105,000	28.000 26,250
Fine-grained gray sienite.	Sauk Rapids	On bed[2]. On edge,	86,000 100,000	21.500 25,000
Average of	twenty samples	....	104,800	26,675

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[1] Estimate.

[2] Probably an imperfect sample.

Allowing for eleven per cent difference between processes of crushing between steel-plates and between wooden cushions, this gives an average for Minnesota granites of 23,318 pounds per square inch.

The following are the records of tests of twenty New-England granites: —

Kind of stone.	Location of quarry.	Position.	Strength in pounds.	
			Of sample.	Per cubic inch.
Blue	Staten Island. N. Y. . .	On bed,	89.250	22,315
	Fox Island, Me. . . .	— —	59,500	14,875
	Dix Island, Me. . . .	— —	60.000	15,000
Dark	Quincy, Mass.	— —	71,000	17,750
Light	Quincy, Mass.	— —	59.000	14,750
Flagging	Hudson River, N. Y. . .	— —	53,700	13,425
	Cape Ann, Mass. . . .	On bed,	59.750	14.937
Porter's rock, ...	Mystic River, Conn. .	On bed,	72.500	18.125
Gray	Stony Creek, Conn. . .	On bed,	60.000	15.000
Gray	Fall River, Mass. . . .	On bed,	63,750	15.937
Bluish-gray	Keene, N. H.	On bed,	41,000	10.250
Bluish-gray	Keene, N. H.	On bed,	51,500	12.875
	Millstone Pt., Conn. . .	— —	64,750	16,187
	Greenwich, Conn. . . .	— —	45,200	11,300
Niantic river, ...	New London, Conn. . .	— —	50,000	12,500
Niantic river, ...	New London, Conn. . .	On edge,	56,700	14,175
	Vinalhaven, Me. . . .	— —	52,600	13,150
	Vinalhaven. Me. . . .	— —	67,000	16,750
	Westerly, R. I.	On bed,	58,750	14.689
	Westerly, R. I.	On edge,	59,750	14.937
Average of	twenty granites . . .	. . . .	59,785	14.946

After discussing several supposable causes of error, and showing that they could not have applied to the present case, the author proceeds to suggest causes why the Minnesota granites may be stronger than those of New England. He thinks those of the west may have been less changed by decay. The lateness of the glaciation to which they were exposed may have left them comparatively fresh through the recent removal of a considerable thickness. On this point we shall be more certain when the glacial moraines have been fully traced from west to east, and the eastern analogues are determined.

THE SINGING BEACH OF MANCHESTER, MASS. By H. C. BOLTON of Hartford, Conn., and A. A. JULIEN of New York, N. Y.

[ABSTRACT.]

THE authors describe the acoustic phenomena observed by one of them at Manchester-by-the-Sea, Mass., and on the island of Eigg, Hebrides, together with the results of microscopical examination of the sands. The beach at Manchester forms a small crescent about three-quarters of a mile long, and is terminated at each end by bold promontories of granite, rich in feldspar, and intersected by numerous dykes of igneous rocks among which porphyritic diorite is noticeable. The beach sand resembles at first sight ordinary sea-shore sand, but when struck by the foot, or stroked by the hand, yields a peculiar sound which may be likened to a subdued crushing; the sound is of low intensity and pitch, and is not metallic nor crackling. This phenomenon is confined to that part of the beach lying between water-line and the loose sand above the reach of ordinary high tide. Some parts of the beach emit a louder sound than others. The sounding sand is near the surface only; at the depth of one or two feet the acoustic properties disappear, probably owing to the moisture. Only the dry sand has this property. The sounds occur when walking over the beach, increase when the sand is struck obliquely by the foot, and can be intensified by dragging over it a wooden pole or board. A slight noise is perceptible upon mere stirring by the hand, or upon plunging one finger into the sand and suddenly withdrawing it.

Similar beaches occur in various parts of the world. One of us visited the island of Eigg, Hebrides, in July and found the acoustic phenomena quite similar to those of Manchester. The Eigg locality has been described by its discoverer Hugh Miller. One of the most famous localities in the world is on the island of Kauai, of the Hawaii Islands. There the sounds are said to be so loud as to resemble distant thunder, when any great weight is dragged over it. As elsewhere dampness prevents the sound. The sand is almost wholly calcareous and has been examined by Dr. Blake of San Francisco.

In other places sonorous sand is associated with high dunes or steep hills; of these the most famous is Jebel Nakous on the Gulf of Suez, in Arabia Petræa, which has been described by many European travellers. A similar hill exists near Cabul in Afghanistan. The sand of these hills is silicious. In Churchill Co., Nev., a somewhat similar phenomenon is described, the sound being likened to that produced by telegraph wires when the wind blows through them.

Microscopical examination of the sands from several localities shows them to be unlike in constitution, in form, and in structure. The sand of Manchester is about fifty per cent feldspar, that of Kauai is calcareous, that of Eigg silicious. The latter is peculiar in containing dark granules of chert (to about three or four per cent of the whole) which has a cellular structure. The quartz is present in various sizes, some rounded and others angular.

To explain the sonorous properties of these sands several theories are considered. That of equality of size of the grains is rejected. Resonance due to cellular structure probably accounts for the sound in the sands of Kauai and of Eigg, but this structure does not occur in other cases. Effervescence of air between moistened surfaces does not apply to the Manchester sand. Sonorous mineral or rock, such as clinkstone, is not present. There is no evidence of electrical phenomena being concerned. The hypothesis adopted is based upon the structure of the sand which, instead of being composed of the usual rounded particles, is made up of grains with flat and angular surfaces. In the Manchester sand, the plane surface of feldspar is apparent in many of the grains. Probably a certain proportion of quartz and feldspar grains is adapted to give the sound, while less or more of either component would fail of the result.

It is concluded that different conditions are concerned in the production of the sound in different localities; as, for example, cellular structure, intermixture of grains having cleavage planes, parallel arrangement, slight cementation of the grains, etc.

The paper, which is to be regarded as a preliminary notice, ends with a table showing the physical structure of the sands from many localities. The authors add an urgent request for the contribution of similar facts and additional material from other places at which the phenomena may be observed.

THE EARTH'S OROGRAPHIC FRAMEWORK: ITS SEISMOLOGY AND GEOLOGY. By RICHARD OWEN of New Harmony, Indiana.

[ABSTRACT.]

I. *Its Orography.*

SEVERAL writers[1] have already pointed out the almost uninterrupted successions of important mountain-chains, from the Cantabrians easterly to China.

The present paper calls attention to the connection of that so-called "Stony Girdle of the Earth" with two important G. C's[2] of seismic activity, and with geological changes of comparatively recent date.

If we bring to the zenith the region of land centre, the P. V. will coincide with the main ranges of the great east and west orographic framework, at least from the Cantabrian mountains nearly to the Hindu Cush, and Pamier plateau (or "Roof of the World"), whence the P. V. crosses the basaltic Deccan to Sumatra, where it is 90° *east* of the B. M. In the same way, continuing west from the Cantabrians, we find the P. V. pass through the Azores and the Lesser Antilles, reaching Caraccas to follow the eastern cordillera of the Andes to the region of the volcanic plateau near Quito, with its three active, five dormant and twelve extinct volcanoes, exactly 90° *west* of the B. M.

A plane projected through our globe, coincident with the B. M. would divide the earth into an eastern and western hemisphere, having respectively the regions of Quito and Sumatra for the centre of each, exhibiting a duality, not so apparent when we divide the hemispheres, as in most geographies, about mid-Alantic. The unabbreviated paper points out many evidences of this duality, and indeed of a symmetrical distribution of land and water, seismic phenomena and magnetic intensity.

The Andean, and subsequently the Rocky Mountain "Backbone," or continuous chain of nearly 8000 miles (if we estimate from Patagonia to Alaska) intersects the P. V. at Quito; while the somewhat interrupted chain from Behring's Straits through

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[1] Manual of Modern Geogr. by Dr. MacKay, also the "Abode of Snow" by A. Wilson.

[2] The abbreviations used in the Montreal paper "Contributions to Seismology" are here continued. B. M. (brazen meridian) is also used to indicate the meridian of land centre, where Switzerland is brought to the zenith. If an international prime meridian is ever adopted this B. M. would appear to have the strongest claims to that distinction.

Kamchatka, the Kuriles, Japan, etc., intersects the P. V. not only near Java, in the eastern hemisphere, but also at Caraccas in the western hemisphere.

II. *The Seismology of the "Stony Girdle."*

The intersections of the P. V. at Quito, Caraccas and Java are near the regions of most internal activity, as proved by the Tomboro and other outbursts,[3] the thirty-eight volcanoes in Java, etc., on the one hand, as well as by the Cotopaxi-Sangai eruptions, and the Riobamba, Caraccas and other earthquakes of that proximity, added to the Central American Coseguina, Izalca, etc., seismic action, on the other hand.

The law seems to be that the active volcanoes often form part of the "Stony Girdle," while earthquake regions usually display their activity within areas enclosed by the great orographic links. Orton, in his "Andes," p. 161, speaking of earthquakes there, says: "Most damage is done where the sedimentary plains abut against the hard, upturned strata of the mountains."

The great arctic and antarctic volcanic chain from Kamchatka and the Kuriles, along Japan and across Sumatra, thence through the southern Pacific to South America, and along the Andean and Rocky Mountain backbone of America to Behring's Straits again, has its nearly two hundred linear volcanoes, to a considerable extent *on* the orographic main-chain; whereas when we trace the P. V. as a belt, we find its seismic regions chiefly *between* the mountain ridges which make up that belt or girdle.

Beginning at the Cantabrians, we may follow the Pyrenees, Alps, Carpathians, etc., east to the Caucasus; again we may trace a nearly parallel chain several hundred miles, south of the first, running from the Spanish Sierra Nevada, in an easterly direction through the Balearic Islands and northern Sardinia, close to Ischia, thence to the highest Balkans (some nearly 10,000 ft. high) and along Anti-Taurus to Armenia. A third parallel range may be followed from the Atlas mountains to Sicily, the Morea, and Taurus, including Candia and Cyprus, also to Armenia. There all three unite to form the Persian Caucasus, from Demavend, along the Paropamesan range to the Hindu Cush, as already incated.

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[3] To which we may now add the recent catastrophe in the Straits of Sunda, and at the southwest extremity of Java, produced by Karakas.

Either on, or within, this tri-partite belt, we find the extinct Auvergne volcanoes, the active mountains or other regions, such as Etna, Vesuvius, submarine "Graham," the Lipari Islands, Santorin and four other volcanic islands[4] in the Grecian Archipelago, the mud volcanoes of Perekop, the Chimæra in Lycia,[5] the Baku "Field of Fire;" also the extinct volcano of the Caucasus (Elburuz); those of Armenia (Ararat, Kasbek and Savalan Dagh; besides the active Demavend, in the Elburz or Persian Caucasus.

We have, further, within this area, the highly seismic regions of Lisbon, Switzerland and Italy, especially Calabria; also of Agram, Cattaro, and Ragusa, Negropont, Scios, Smyrna, Antioch, Tabriz in Persia, Schamaki, and numerous places, besides the Cutch, on the route to Sumatra, with its four active and fifteen extinct volcanoes.

Tracing the P. V. *west* from the Cantabrians, we have two volcanoes in the Azores and ten in the West Indies, all nearly on the P. V. line to Quito. This P. V. thus followed through the land-hemisphere, if traced through the oceanic-hemisphere, will be found to pass through volcanic New Zealand.

It is also worthy of note that when the P. V., or one of its collateral stony furcations, intersects any one of the five equidistant continental eastern trends (described in the Montreal paper on "Seismology") there are usually found important orographic and seismic results.

III. *The Geology of the "Stony Girdle."*

A close inspection of Murchison's Geological Map of Europe, as well as of D'Orbigny's "Extension Géographique," especially for the "Néocomien" and "Suessonien;" also of Dana's and Geikie's latest text-books, besides many other works detailing the Cretaceous and Tertiary areas, can scarcely fail to satisfy us that we can trace both Cretaceous and Tertiary along almost the whole line of the P. V.

The conclusion forced upon us seems to be that the Alps, the Carpathians and Balkans, the Euxine-Caspian Caucasus, as well as the Persian Caucasus, and Sivalik Hills or sub-Himalaya range, also the more southerly Atlas-Taurean strike, together with the western extension of the P. V. through the Azores and West Indies to Chimborazo, *all* owe their *present* elevation partly to the orographic impulse about the close of the Mesozoic, and partly to the earlier Tertiary, especially Nummulitic Eocene, and Oligocene: slowly attaining nearly their maximum eminence during the Miocene; since which little addition seems to have been made, as the Pliocene and Quaternary are usually undisturbed, except probably in the Andes. Illustrative details, which could readily be given, have of course to be omitted in an abstract.

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[4] Dana's Manual, p. 703.

[5] Cosmos, Vol. iv (Sabini's trans.), p. 251.

Recapitulation.

It appears, then, that a great mountain-framework, in several parallel ranges, extends nearly east and west, from the centre of land, near the "Furka" watershed of great rivers (Rhine, Rhone, tributary of Danube, etc.) to the equator, respectively at Quito and Sumatra. Strengthening braces run from the elevated Asiatic plateau region, and sources of great diverging rivers (Brahmaputra, Ganges, Indus, with tributary Sutlej, etc.) in at least four grand ramifications through Asia; while America is similarly strengthened by its continental arctic-antarctic "backbone," whence flow the mighty waters of the Missouri, Amazon and other streams.

The east and west "Framework," and the arctic-antarctic Belt, indicate intense reaction from the interior to aid this recent, as well as earlier, mountain-making: the five equidistant continental trends often marking Paleozoic belts[6]; while the east and west "Stony Girdle" is of Mesozoic age, with its highest finish in Cenozoic times, leaving a belt of almost continuous fire, at space-intervals sufficiently near to promise a continuance of erupted products, during at least Quaternary history: namely, through the five ever active regions mentioned by Humboldt, Izalco, Masaya and Sangai; Fogo, Stromboli and Chimæra, besides others, scarcely less active such as Santorin and the Malay Archipelago

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THE CONTINENTAL TYPE, OR NORMAL OROGRAPHY AND GEOLOGY OF CONTINENTS. By RICHARD OWEN of New Harmony, Indiana.

[ABSTRACT.]

IN each one of the five great continents may readily be traced a general plan of construction, or physiography, connected with their appearance as land above the ocean; also a similarity in their geognostic history and present geological areas.

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[6] See Murchison's map of Paleozoic regions, in "Siluria," to face p. 575.

In order to make this apparent, let us construct a cross-section of each continent, taking the ocean as our base line, and running from W. S. W. through the geographical centre[1] of the continent to E. N. E.

In each case, we find a seismic belt, near at least one rim of the continent, usually of both, either on the shore border, or just outside in the ocean. Orographically, each continent is more or less basin-shaped, being comparatively low in the central area, with its chief river drainage; and low near the ocean borders; rising in an eastern and western main range, with usually several parallel subordinate ridges. These eastern and western mountains generally converge southerly, thus assuming a somewhat triangular form enclosing often on the west some table-land.

The eastern rim is usually Paleozoic with perhaps some Mesozoic on the flanks, and Cenozoic on the ocean border. The western elevation is more commonly Mesozoic in its main range, and Cenozoic on the flanks, or subordinate ridges.

A section running north and south (instead of easterly and westerly) through the three northern continents would successively expose Archæan, Paleozoic, Mesozoic and Cenozoic cuts, which formations would generally increase in area as we go south.

1. Beginning on the west with *N. America*, we find (on traversing the continent from the region of California, through the continental centre, near the west end of Lake Superior to Maine) first the seismic belt running along the west coast of N. America (marked "D′" in the Montreal paper on Seismology) then comes the Cenozoic in the coast range and on the flanks of the Sierra Nevada; subsequently the Rocky Mountains, chiefly Mesozoic; but, by their extensive basaltic outflows, forming tablelands of Tertiary areas affording land and fresh-water fossil animals, sometimes of gigantic proportions, also lignitic coal; then follows a gradual slope to the great valley of central drainage. Soon after passing the height of land, we encounter the White Mountain and Katahdin reëxtension of the Paleozoic Appalachians, with a considerable amount of Mesozoic on the eastern slopes, and some Cenozoic, rather south of our line at Cape Cod and Martha's Vine-

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[1] This is usually in about lat. 48°–49° N., for northern continents; on the equator, for Africa; and on the Tropic of Cancer, for S. America and Australia; as shown in detail at pp. 437, *et seq.*, of Boston Vol. of Proc., in paper on "Law of Land-Forming."

yard. The section closes near the meridian of the seismic belt, running from Central America, through the West Indies to Iceland: viz. Great Circle "F′."

To supplement the above east and west sections, we may trace N. America from north to south, which will give us Archæan, as far down as Lake Superior; then Paleozoic, as we go south to Tennessee; then chiefly Mesozoic (except in the extreme centre of erosion) to the state of Mississippi; and thence Cenozoic, to the Gulf of Mexico, as well as in Texas and Mexico.

2. Let us take *Europe* next, in cross-section from northern Spain, through Tatra (the centre of the continent) to the Caspian, its eastern termination, in a depression 84 ft. lower than the ocean. We find on the west a seismic belt (G. C. " C.", passing over the submarine volcanic areas of the Cape Verd and Canary Islands, besides Lisbon); then some Cenozoic in north Spain; then chiefly Mesozoic with some Cenozoic in the Pyrenees, Alps and Carpathians; then a valley of great drainage: the Danube, Dniester, Dnieper, Don and other rivers flowing into the Black Sea; besides the Volga, etc., into the Caspian.

On or near this section line, we have trap, trachyte and other ancient eruptive rocks, flowing out at various points in France, Switzerland and Austria; then we traverse the tablelands of Hungary and Transylvania. Although the Caucasus is chiefly Mesozoic with Cenozoic on the flanks, yet a little south of our section line, Murchison shows a considerable area of Metamorphic, and some Paleozoic, along the anti-Taurus mountains, as they trend toward Armenia; while north of our section, we have the Paleozoic of Perm and the Urals, with outflows of trap, extending nearly the whole length of that range; and lastly we encounter the highly seismic regions around the Caspian, which form part of G. C. "D.," the same which passes along our Appalachians.

3. *Asia.* In a cross-section from the Caspian through the continental centre, near Lake Baikal, to Japan, starting from the low, seismic region, we cross portions of the radiating ridge from the Hindu Cush and Himalayas; then traverse the comparatively low Lake Baikal region, and meet the Khingan mountains on our route to Japan.

The high Himalayas are at least partly Mesozoic, with Cenozoic on their flanks, in the Sivaliks; the central portion of Asia is also partly Mesozoic, partly Cenozoic; in the Provinces between the south extremity of the Khingans and the eastern ramifications of the Kuen Lun, namely, in Hunan and Szechnan,[2] the Paleozoic affords true Carboniferous coal; while in Japan with its numerous volcanoes, there is much Cenozoic.[3]

4. *S. America.* The centre of this continent being on the Tropic of Capricorn, our section may run from the region of N. Chili, through Brazil, a little north of Rio. In this section, we have the Cordilleras in double range, and find Sorata and Sahama, the highest mountains in the western hemisphere, somewhat north of this line. The mean height of the Brazil tablelands is from 2000 to 3000 ft.; but the eastern Sierra Montequeira, north of Rio, attains the altitude of 10,000 ft.

The great centre is drained north by the Madera river, running into the Amazon; south by the Paraguay and Parana flowing into the La Plata.

With reference to the geology of this region, Darwin, D'Orbigny, Professors Hitchcock and Orton[4] mention considerable areas of Paleozoic in the Bolivian Andes and in eastern Brazil, with some intermediate Oolitic, Cretaceous and Tertiary; the latter extending, with Quaternary, south over much of the Argentine Republic and all Patagonia. They think that this Cenozoic occupies the area of wide straits, which formerly separated the Andes from Brazil. Some Paleozoic rocks are also found in Chili, along with its enormous mountains of trachyte and active volcanoes. Erupted porphyries are likewise very abundant. On each side of the continent there are seismic belts; one on the east partly submarine (G. C. "C"), the other on the west, running along the Andes, etc., (G. C. "D'") indicating the normal type.

5. In *Africa*, as the centre of land is near St. Thomas's Island, it is more convenient to run the section a little farther north, say from Liberia, nearly through Lake Tchad to northern Abyssinia, so as to give land, instead of water, on the western half. The law here holds good; showing us the elevations of the Kong mountains to some 9000 ft., and at the north, in the distance, the Atlas range attaining over 11,000 ft., while the more eastern ranges in Abyssinia reach 12–15,000 ft. We observe also the extensive central depressions, and the drainage south by the Niger.

The geology is not accurately known yet, but seems to be Cenozoic with some Mesozoic centrally (especially farther north in Sahara); and, outside the eastern basin rim, chiefly Cenozoic in Arabia and around the Persian Gulf. The Atlas mountains are Cretaceous, overlying Paleozoic, with some Cenozoic on their flanks. Along the Abyssinian Metamorphic mountains, with some Triassic, we trace, as also in Ethiopia and Egypt, dioritic and syenitic eruptions. The whole of the Pyramid of Cheops is of nummulitic limestone, but the lesser pyramid, beyond the Sphinx from Cheops, has its first layer of syenite, the next being nummulitic limestone. The Sphinx itself is carved from a red sandstone *in situ*, partly covered by the drifting sand, but still showing distinctly the beds of stratification.

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[2] See Thompson's China, p. 441. [3] Miss Bird's Japan. [4] "The Andes," p. 117.

Outside the continent of Africa, we have two great seismic belts; that on the west being G. C. "C," which embraces the volcanoes of Cape Verd, Canary Islands, etc. The seismic G. C. "B," on the east, besides traversing the Sandwich and Comoro Islands, runs near the Bourbon and the Red Sea volcanoes and crosses the volcanic and earthquake region of Central Asia. The existence of the Thian Shan volcanoes, notwithstanding the negative, opposing hearsay evidence of Schuyler, in his work on Turkestan that "no volcanoes had been seen by his informants," nevertheless receives strong positive testimony, in Chinese records; and has secured the sanction of Humboldt, Mallet, Guyot and other eminent scientists.

ON RHIZOCARPS IN THE PALÆOZOIC PERIOD. By J. W. DAWSON of Montreal, Canada.

[ABSTRACT.]

THE paper noticed the discovery of *Sporangites Huronensis* Dawson, in the Erian shale of Kettle Point, Lake Huron, and its description by the author in 1871,[1] when these organisms, abounding in a bed of bituminous shale twelve feet in thickness, were described as probably sporangia or macrospores of some acrogenous plant. Mention was then made of the subsequent discoveries of Professor Orton in the Erian shales of Ohio, and those of Prof. H. S. Williams and Prof. I. M. Clarke in New York, whereby it appeared that similar organisms are abundantly distributed in those states through shales 250 to 300 feet thick, and extending over wide areas. The late Professor Hartt had also found similar bodies in Brazil. A new light had been thrown on these facts by the discovery in the Erian shales of Brazil of these bodies, still contained in circular envelopes or sporocarps similar to those of *Salvinia* and other modern Rhizocarps. This discovery was due to Professor Orville Derby of the Brazilian Geological Survey, who also had sent specimens to the author.

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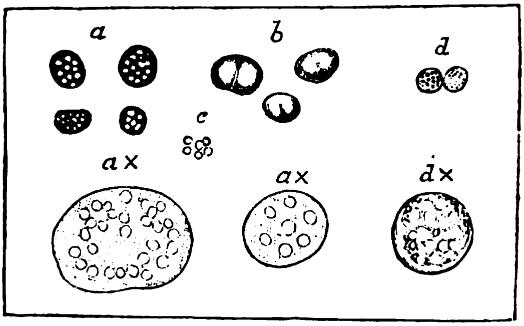
[1] American Journal of Science.

Mr. Derby's specimens contain *Spirophyton* and also minute rounded *Sporangites* like those formerly obtained by Professor Hartt. But they differ in showing the remarkable fact that these rounded bodies are enclosed in considerable numbers in spherical and oval sacs, the walls of which are composed of a tissue of hexagonal cells, and which resemble in every respect the involucres or spore-sacs of the little group of modern acrogens, known as Rhizocarps, and living in shallow water. More especially they resemble the sporocarps of the genus *Salvinia*. This fact opens up an entirely new field of investigation as to these interesting specimens, their probable affinities with modern plants and their probable relations to other palæozoic forms of vegetation found with them.

Mr. Derby's specimens are labelled as from Rio Trombetos and Rio Curuá. They occur in two kinds of matrix. One is a thinly laminated sandy shale, tinged red with peroxide of iron, and with occasional ferruginous laminæ. In this the spore-sacs are flattened and black, and show the structure of the walls under the

FIG. 1.



(*a*) *Sporangites Braziliensis*, nat. size. (*a* ×) same, magnified.
(*b*) *Sporangites bilobatus*, nat. size.
(*c*) Detached macrospores.
(*d*) Spore-cases of *Salvinia natans*. (*d* ×) same magnified.

microscope. The contained macrospores, when visible, appear as minute tubercles or sometimes as depressions on the wall of the envelope, or more frequently as round light colored spots, according to their state of preservation (fig. 1, *a*). The other kind of matrix is a gray dense shale in which the spore-sacs appear less flattened and destitute of carbonaceous matter.

The very numerous spore-sacs contained in these shales are extremely variable in size and form, and may have belonged to several species of plants. They resolve themselves, however, into two leading types, which may be provisionally named *Sporangites Braziliensis* and *S. bilobatus;* though I would suggest for them, in prospect of the discovery of their vegetative parts, the name *Protosalvinia.* They may be described as follows:—

Sporangites (*Protosalvinia*) *Braziliensis*, s. n. (fig. 1, *a*). Sporocarps thin, carbonaceous, having a structure of dense hexagonal cells and enclosing macrospores which vary in number from three or four to as many as twenty-five. Form circular, oval or reniform. Longest diameter from three millimetres to six millimetres. Some of the sacs which do not show included round bodies may have held microspores.

S. (*P.*) *bilobatus*, s. n. (fig. 1, *b*). Sporocarps oval or reniform, three millimetres to six millimetres in diameter, each showing two rounded prominences at the ends, with a depression in the middle, and sometimes a raised neck or isthmus at one side connecting the prominences. Some of the specimens indicate that each prominence or tubercle contained several macrospores. At first sight it would be easy to mistake these bodies for valves of *Beyrichia.*

The following remarks were made on the probable affinities of these bodies and their relation to other Erian plants:

There can be no question of the close resemblance of the Brazilian species of *Sporangites* with the spore-envelopes of modern Rhizocarps. Some individuals of the *S. Braziliensis* are scarcely distinguishable in form or contained macrospores from the sporocarps of *Salvinia natans* of the rivers of Europe (fig. 1, *d*). It is true that the analogy of *Salvinia* would lead us to expect other sacs containing microspores; but in ordinary circumstances the latter could not be preserved in a visible state, and on the Brazilian shales there are many specimens not showing macrospores, and which might have been filled with microspores which have been flattened into an undistinguishable mass.

If we compare the separate macrospores of the Brazilian sporocarps, and especially those which are found detached from their envelopes, with *Sporangites Huronensis*, we see a remarkable similarity in size, form and texture, sufficient to justify us in supposing that the latter may be of the same nature with the former, but deprived of their outer cases either by dehiscence or by decay, and this is the view which I am now disposed to take of their nature, and which better accords with their wide distribution in aqueous deposits and with their accompaniments than any other supposition. I may add that Professor Orton and Professor Clarke, in letters to the author, refer to grouping of the little rounded bodies and traces of enveloping membrane. In this connection I would also mention the sacs containing rounded bodies known as *Parka*, and which have been met with in the Erian beds both in Scotland and in Gaspé, and have been supposed to be ova of crustaceans. It is true that these are much larger than the sporocarps above referred to, but on examination of Gaspé specimens in my collection, I am disposed to suspect that they also may prove to be the fructification of Rhizocarps.

It remains to enquire, Are there any Erian plants known to us in their stems and foliage, to which such organs of fructification as those above described might have belonged?

A preliminary question would naturally be as to the vegetative organs of modern Rhizocarps. On reference to the descriptions, and to a somewhat extensive collection of specimens placed at my disposal by Mr. Poe Watt of Montreal, I find that these may be referred to three leading types. Some, like *Pilularia*, have simple linear leaves ; others, like *Marsilea*, have leaves in verticils and cuneate in form ; while others, like *Azolla* and *Salvinia*, have frondose leaves, more or less pinnate in their arrangement. The first type presents little that is characteristic, but there are in the Erian sandstones and shales great quantities of filamentous and linear objects which it has been impossible to refer to any genus, and which might have belonged to plants of the type of *Pilularia*. It is quite possible also that such plants as *Psilophyton glabrum* and *Cordaites angustifolia*, of which the fructification is quite unknown, may have been allied to Rhizocarps. With regard to the verticillate type we are at once reminded of *Sphenophyllum*, which many palæo-botanists have referred to the *Marsiliaceæ*, though like other palæozoic Acrogens it presents complexities not seen in its modern representatives. *S. primævum* of Lesquereux is found in the Hudson River group, and my *S. antiquum* in the middle Erian. Besides these there are in the Silurian and Erian beds, plants with verticillate leaves which have been placed with the *Annulariæ*, but which may have differed from them in fructification. *Annularia laxa* of the Erian and *Protannularia Harknessii* of the Silurian may be given as examples. As to pinnate leaves, I have already referred to the remarkable plants of the genus *Ptilophyton*, found both in the Erian and Carboniferous, and which seem to have been aquatic in their habit like Rhizocarps. It is deserving of notice also that the two best known species of *Psilophyton* (*P. princeps* and *P. robustius*), while allied to Lycopods by the structure of the stem and such rudimentary foliage as they possess, are also allied, by the form of their fructification, to the Rhizocarps, and not to ferns as some palæo-botanists have incorrectly supposed.[1]

I do not suppose that the facts above stated furnish any positive proof that the abundant *Sporangites* of the Erian period were the fructification of Rhizocarps, but they establish a certain probability of this, and invite to further researches. If it should prove that these humble plants, now so insignificant, culminated in the Palæozoic age, and occupied the extensive submerged flats of that period with an abundant vegetation, producing a great quantity of the bituminous matter found in the resulting beds, this early culmination of the Rhizocarps would be strictly in accordance with other facts in the development of the vegetable kingdom. We may even be permitted to speculate on the existence in the early Palæozoic and Eozoic ages of a rich Rhizocarpean vegetation, anticipating the great development of the Acrogens in the later Palæozoic.

I have not referred above to the well-known fact that in certain beds of coal and shale of the Carboniferous period there are multitudes of globular spore-cases or macrospores not dissimilar from those above described. These may have been derived from plants of higher organization than the Rhizocarps, yet it is quite possible that this group of plants may have contributed to them. It is, however, only in the Erian that these *Sporangites* are so widely and abundantly distributed in aquatic beds, and that we have direct evidence as to their origin.

[1] See Report on Erian Plants of Canada, 1882.

ON A LARGE CRUSTACEAN FROM THE CATSKILL GROUP OF PENNSYLVANIA. By E. W. CLAYPOLE of Buchtel College, Akron, Ohio.

IN this paper an account was given of a very large crustacean fossil recently found in the Catskill sandstone of Pennsylvania. Only the head remains and part of that is incomplete. One side however is perfect and the other can therefore be readily restored. In shape it is semi-elliptical, resembling one end of an ellipse. Roughly speaking, it bears a striking likeness to a vastly magnified head of Cephalaspis. But the total absence of all traces of bone excludes the possibility of its being ichthyic. The remains of the creature consist of a thin black scale covering the whole surface except where it has been removed by accident or compression ; a longitudinal ridge extends along the middle, rising apparently at both ends into spines or tubercles and another ridge crosses this near its hind end. This ridge is continued in a double curve resembling the arms of an anchor fore and aft.

So far as I have yet been able to make a study of this fossil, it appears to be more readily allied to the king crab (*Limulus*) than to any other existing order. But its immense size, 8½ inches in both directions, renders it very conspicuous and the elongated outline of its head is in strong contradistinction to the semicircular head-thorax of *Limulus*. There can be little doubt that it was closely allied to the great fossil crustaceans of the order Eurypterids. But the squarish outline of the head of these is totally wanting and instead of the flat smooth glabella the surface of the head is ridged as above described.

I have been unable to discover any trace of eyes and am therefore unable to obtain the valuable evidence they would give in placing the fossil. But the addition of so strange a form to the meagre fauna of the Catskill, from which only three or four fish have thus far been described, raises the hope that one day we may become better acquainted with the life of the Catskill age of which at present almost nothing is known.

RENSSELAERIA FROM THE HAMILTON GROUP OF PENNSYLVANIA. By E. W. CLAYPOLE of Buchtel College, Akron, O.

[ABSTRACT.]

THE genus Rensselaeria has been hitherto confined almost exclusively to two horizons or groups: the Lower Helderberg and the Oriskany sandstone. Of the twelve species thus far described, four have been obtained from the former and seven from the latter of these groups.

One only, *R. Johanni*, has been described by Prof. Hall from the Corniferous rocks of Waterloo, Iowa. The specimens have all the outward appearance of Rensselaeria, but Prof. Hall expresses some doubt concerning the accuracy of his determination.[1]

The occurrence therefore of well marked large specimens of Rensselaeria high up in the Hamilton rocks is a fact deserving of note, being at least unexpected.

In regard to the species I must say that I am at present quite unable to separate it from *R. Marylandica* of the Oriskany sandstone.

The material in which it occurs is a coarse sandstone known in the geology of Perry County as the Hamilton Sandstone, a local deposit in the group occurring about 800 feet above the horizon of the Marcellus black shale and about 1000 feet above the Oriskany Sandstone. Its only accompanying fossil is a spirifer closely resembling, and I believe identical with, *Spirifera formosa*, Hall.

PRELIMINARY NOTE ON THE MICROSCOPIC SHELL-STRUCTURE OF THE PALEOZOIC BRACHIOPODA. By JAMES HALL of Albany, N. Y.

[ABSTRACT.]

IN the earlier studies of the fossil Brachiopoda, the numerous species were referred to few generic terms, determined mainly from their perforate or non-perforate apex and external form, and later from the study of the interior as these parts became known. The author said, that from time to time, as these characters had become known to him from the study of large collections, he had found it necessary to propose the separation of more than twenty new generic forms beyond those previously recognized in this class of fossils. Other authors had also proposed new generic terms, until the list had become many times greater than it was twenty-five years ago.

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[1] Professor Hall informs me that he has now removed the species in question from this genus.

While the interior structure of the hinge, and the muscular and vascular markings, were now pretty well known for most of the generic forms in use, comparatively little attention had been given to the minute structure of the shell.

The study of this structure had been commenced by him many years ago, but he had been thwarted in his efforts to procure the required cutting and polishing of specimens of the shells necessary for microscopic study. Through the aid of Mr. C. E. Beecher, Assistant in the State Museum of Natural History, he had now been able to obtain such thin slices of the shell as were required for this purpose, and had already several hundred slides prepared for the microscope.

A few of these only were shown, exhibiting the shell-structure of as many genera. A considerable number of photographs had been made, illustrating, in a very satisfactory manner the minute structure of each one, enlarged to twenty diameters. The photographs exhibited were illustrations of several species of ORTHIS, LEPTÆNA, STROPHOMENA, STROPHODONTA, CHONETES, etc.

The study of this shell-structure has shown, very satisfactorily, what was partially known before, that the genus ORTHIS, as now defined and constituted, includes very heterogenous material. External form, hinge characters, and interior muscular impressions, have been the chief guide; and yet forms have been included under this genus, showing widely different muscular markings. On further microscopic study, it has been found that these differences in form of muscular impressions are accompanied by important differences in the shell-structure.

These differences may be noted in the illustrations presented, where the shell of *Orthis biforata*, *O. borealis*, *O. tricenaria*, *O. occidentalis*, *O. flabella*, are non-punctate and coarsely fibrous, *Orthis* (?) *strophomenoides* is, like STREPTORHYNCHUS, fibrous. *Orthis subquadrata* has, like *O. occidentalis*, a few large punctæ.

In the second group, *Orthis testudinaria*, *O. Vanuxemi*, *O. perveta*, *O. penelope*, *O. elegantula*, *O. clytie*, and *O. hybrida*, have one or more rows of punctæ to each ray, the rows well defined, and the intermediate shell finely fibrous.

The third group, consisting of *Orthis multicostata* of the lower Helderberg, *O. Iowensis* of the Hamilton group, *O. Tulliensis* of the Tully limestone, *O. impressa* of the Chemung group, are highly punctate with a fine fibrous texture of the shell substance.

The punctæ usually come out along the summits of the radiating striæ or plications of the shell. In some species the minute tubes perforating the shell, and producing these punctæ, bifurcate and diverge before coming to the external surface of the shell.

This difference in shell-structure, in forms known as ORTHIS, will require a separation of the species into groups based upon this structure, and character of muscular impressions. Already we see that the shells of compact fibrous texture have a form of muscular impression quite unlike those with the punctate structure; and we shall probably find that all the interior modifications of the muscular system are accompanied by differences in the microscopic structure.

This method of determining the shell-structure, in cases where the specimen may be imperfect, and thereby enabling the determination of obscure or fragmentary material, and its geological relations, will be of much importance to the geologist.

The structure of the shell in STROPHOMENA is closely fibrous, with distant large punctæ. In STROPHODONTA, the punctæ are more numerous. In CHONETES, the punctæ are large and arranged in a single row upon the radii, having a pustulose aspect similar to those of STROPHODONTA.

In many other forms, the punctate texture of the shell is characteristic, and of importance in the determination of the generic forms.

The physiological significance of this peculiar shell-structure will be considered upon some future occasion, illustrated by more numerous examples.

ANIMAL REMAINS FROM THE LOESS AND GLACIAL CLAYS. By WILLIAM McADAMS of Alton, Ill.

[ABSTRACT.]

THE drift clays proper at Alton, Ill., had a maximum thickness of about one hundred feet, and the bluff clays were nearly of the same thickness. These clays were remarkably rich in animal remains, such as teeth and bones, attached to calcareous nodules or claystones. Remains of thirteen different species, now perhaps all extinct, had been found. The rodents were well represented by bones of seven species, including three or more beavers and some gophers. Nearly seventy teeth were found in the quaternary deposits, a majority of them in a single quarry.

A NEW VERTEBRATE FROM THE ST. LOUIS LIMESTONE. By WILLIAM McADAMS of Alton, Ill.

[ABSTRACT.]

ONE of the groups of subcarboniferous limestones is quarried extensively near Alton and St. Louis. It lies beneath the coal, and in some places the coal rests directly upon it. A number of vertebrate remains have been found in one of the quarries near Alton. Specimens were shown to the Section. In the judgment of Professor Newberry, the fossils shown were the bones of some large fish. One appeared to be the mandible or dental bone of the lower jaw. Without pronouncing a final opinion, he would say that it bore a general resemblance to a group of fossil fishes in which the teeth were inserted in sockets; but the animal itself was large and hitherto unknown.

ON A WHITE GARNET FROM NEAR HULL, CANADA. By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

TWENTY-ONE miles north of Hull on the Gatineau river, at Wakefield, Canada, in a seam of crystalline magnesian limestone are found some remarkable white garnets. This occurrence has been known to a number of collectors for a few years, but as yet no information regarding them has been published. I found, on visiting the locality, that they form a vein from several inches to over one foot in width in magnesian limestone, the vein running east and west, a distance of over seventy-five feet. The garnets are entirely embedded in the limestone which can best be removed by the action of acid not affecting the crystals.

This vein was followed to the depth of more than six feet. The crystals vary from the size of a pin-head to three inches across.

The color is from colorless and transparent (small mineralogical gems having been cut from them) to yellow running into brown.

This locality is possibly the only one that has furnished white garnet as a gem. The latter color is very often the result of the oxidation of the pyrrhotite. Associated with the garnet are crystals of pyrrhotite and fine crystals of a white pyroxene; the crystals adhering together, are held together sufficiently by the pyroxene to form fine groups of this mineral when the limestone has been removed by the acid. Perfect isolated crystals are very uncommon. The specific gravity of the mineral is 3.6002 and 3.5948 (C. Bullman); 3.52, 3.53 (T. S. Hunt, Orford, Canada).

The Orford composition is almost identical with an analysis of a white garnet by T. Sterry Hunt, from Geological Survey, Orford, Canada, 1863, p. 496.

	C. BULLMAN.	T. STERRY HUNT
Silica,	38.80	38.60
Alumina,	22.66	22.71
Sesquioxide Iron,	1.75	
Oxide Manganese,	0.30	
Lime,	35.00	34.83
Magnesia,	0.68	0.49
Oxide Iron and Manganese,		1.60
Potash,		Trace.
Soda,		0.47
Ignition,		1.10
	99.19	99.80

Spectroscope gave no potassium lime. Soda not determined.

The forms observed were 1, and 1 and 2–2 combined.

ANDALUSITE FROM A NEW AMERICAN LOCALITY. By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

AT Gorham, Me., on the shores of Sebago lake, about one year ago, some excavating was done for a road, and from the pile of rock thrown out in blasting a mass was selected by Mr. Hayden of Raymond, Me., who resides in the vicinity, who thought from the outward appearance and the occurrence in it of pyrrhotite in crystals that it might contain something of interest. This sometime later, he found to contain the specimens now presented here.

These crystals, for perfection, in color and size, are equal to those found at any known locality where this mineral does not occur as a gem. The color is mostly brownish flesh-color and at times the pink color fades to a faint greyish pink. The crystals are opaque but translucent in pieces of from 3mm. to 5mm. in thickness; specific gravity 3.12mm.-3.14mm.; size, a broken crystal measured 90mm. (long), 53mm. (broad) 48mm. (thick).

Several measure over 80mm. in length and one fragment of a large crystal measured 55mm. on one face and may have originally measured over 70mm. on the macro-prism. Some crystals are very small, say 1mm. in diameter.

The hardness on the side of the prism is from 6 - 6.5, on the O plane 7 - 7.5. The crystals consist of a simple orthorhombic prism with the O plane largely developed, the other faces have not been fully determined as yet, but are possibly I ĭ, I ī.

The crystals occur in a quartzite vein in a mica schist of a brown color scattered through which are beautiful small crystals of pyrrhotite. The crystals which I here exhibit and a few fragments are all that have been found as yet in the locality. A hasty visit paid to the locality failed to bring to light any more specimens, the rock having been thrown back into the excavation and all traces of mineral thus obliterated. More attention paid to this locality would surely repay the mineralogist with a good reward, as minerals of this class are rarely found in a few large isolated crystals, when the indications are so favorable as in this case.

TOPAZ AND ASSOCIATED MINERALS FROM STONEHAM, OXFORD COUNTY, MAINE.[1] By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

ASSOCIATED with topaz that exceed in size and perfection those as yet found at any American locality, I found a number that are worthy of mention. The topazes were in perfect crystals. The following forms have been observed: O, $i\text{-}\bar{i}$; prisms I, $i\text{-}\breve{\tfrac{3}{2}}$, $i\text{-}\breve{2}$, $i\text{-}\breve{3}$, $i\text{-}\breve{4}$; macrodomes $\tfrac{2}{3}\text{-}\bar{i}$, $2\text{-}\bar{i}$; brachydomes $2\text{-}\breve{i}$, $4\text{-}\breve{i}$; pyramids $\tfrac{1}{2}$, $\tfrac{1}{3}$, $\tfrac{2}{3}$, 1, 2; $\tfrac{4}{3}\text{-}\breve{2}$, $2\text{-}\breve{2}$.

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[1] In a lot of minerals in the possession of N. H. Perry of South Paris, Maine, August 1882, I observed a crystal that I at once recognized as topaz. Since then considerable work has been done by us, and the minerals here described brought to light. To Mr. Perry much credit is due for his observation and careful manner of working.

The finest crystals were from 10mm. to 65mm. long; the largest crystal measured *a*, 60mm., *b*, 65mm., *c*, 56mm., and was entirely perfect and transparent in parts. The color of the crystals varied from colorless to light green and light blue. Small perfect gems have been cut from some of the fragments.

The large rough crystals measured from 200mm. to 300mm. across, and weighed from 10 to 20 kilos; faces rough and the crystals broken in the bed; color of these light blue, light green, and white or yellowish.

These crystals are full of the characteristic fluid cavities. The hardness same as the Brazilian.

Columbite was scattered through the cleavelandite in crystals from 1mm. to 10mm. in length. A number of acicular crystals bunched together resembled a fibrous structure.

One pocket contained about 20 kilos of this mineral, and one uncouth part of a crystal weighed 8.0 kilos; evidently the whole mass was either a crystal or a group of crystals.

Triplite is scattered all through the rock in pieces from one gramme to one kilo in weight, staining all the other minerals, especially the quartz and cleavelandite.

One mass that may have been a large rough crystal furnished over fifty kilos of this mineral. The color is a light chocolate brown, streaked with almost transparent veins of a lighter colored variety of the same mineral, and is remarkably pure in character, translucent in small pieces, possibly the Var Zweiselite.

Triphyllite, one very imperfect crystal, was found about 15mm. long.

Montmorillonite in pink masses, shading from light to dark pink in color, filling cavities between crystals and plates of cleavelandite, these impressions having an apparent crystalline form; also in small botryoidal masses, resembling rhodocrocite identical with the Branchville mineral described by Brush and Dana.

Uranite in minute scales on the cleavelandite was observed.

Dendrites cover the cleavage faces of the felspars.

Beryl occurs in huge crystals through the entire hill and almost in contact with the topaz. One vein was traced for over forty feet that was full of crystals several feet long and about one foot across. No fine crystals were found here.

Zircon crystals are scattered through the cleavelandite, from 1mm. to 10mm. in length. The faces are all dull and sometimes the crystals are altered to malacon; faces observed were: Pyramid 1. Prism I, *ii*.

Garnet in poor crystals occurs sparingly; it is evidently a manganesian variety and resembles the triplite in color.

Cleavelandite in fine, large plates and cleavages, by the ton, is of a snowy white color or turns to this color, from a dark brown, after a short exposure to the action of the sun. Pockets in it are, as elsewhere, lined with beautiful minerals. In and at the juncture with the mica the fine topazes were found. The color is due to the decomposition of the triplite.

Quartz occurs in abundance, massive and milky in color, breaking with an apparent cleavage, at times curiously spotted with triplite.

Small, irregular crystals, and one smoky crystal 80mm. by 75mm., were found. The massive variety is filled with fluid cavities, and large pieces break easily as if with apparent explosion.

Apatite, small, very perfect crystals, white in the centre, and blue, purple or green at each termination. Faces observed: Pyramid 1. O largely developed. Prism I and *i*2.

A massive, compact, vitreous, dark green variety in nodules weighing one kilo each.

Fluorite fills cavities in the cleavelandite, dark purple in color; and some small, beautiful octohedral crystals, of a light blue color, resembled topaz in appearance.

Margarodite and muscovite in large quantities, forming veins; the former light yellowish green almost radiated in structure, the latter cleaving in fibrous masses and then in crystals transparent through the prism, although two to six inches in diameter.

Curved mica Damourite? in large curved shells resembling saucers, some of them four inches across.

Biotite crystals crystallize in the muscovite and the color of very thin sections is a dark brown by sunlight.

These minerals occur in a coarse granite hill (250 feet long and about 100 feet wide at the top), on Harndon Hill, Stoneham, Me., near the Stow line, all the constituent minerals crystallizing on a large scale. Work may be resumed at an early date and may bring some very interesting things to light.

COLORED TOURMALINES AND LEPIDOLITE CRYSTALS FROM A NEW AMERICAN LOCALITY. By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

IN addition to Paris, Hebron and Norway, Maine, as localities for colored tourmalines, I will now briefly describe a new locality, *i. e.*, Auburn, Oxford, Maine.

Here, associated with smoky quartz, orthoclase, amblygonite, columbite, lepidolite, muscovite, cleavelandite, beautiful blue, pink, green and purple apatite, cassiterite and zircon, were found a number of pockets of fine colored tourmalines. The colors of the crystals obtained were red, blue, green and colorless, and at times all three colors in different parts of the same crystal as at Paris, Maine. The crystals, however, differ in general appearance and also in form, and as a rule are lighter in color. The largest crystal of tourmaline was 105mm. long (over 4 inches), and 10mm. wide, transparent, but flawed, and had all the colors combined, red, green, blue and colorless, in different parts. Crystals from 10 to 60mm. were found, and the larger number were terminated by a pink cap. A number of gems were obtained, principally colorless, green pink and blue.

Forms observed: Pyramid $\overset{O}{\underset{-\frac{1}{2}}{R}}$ Prism 1 *ii*

Smoky quartz in crystals 150mm. to 200mm. in length, coated with a curious felspathic mineral, and at times penetrated by the colored tourmalines for several inches.

Lepidolite is found here in hexagonal, isolated crystals, crystallizing out in distinct crystals on larger ones of the same mineral or on crystals of quartz and feldspar, although not sharply enough defined for measurement, yet they are the finest examples of this mineral in crystal form that I have as yet observed from any locality.

In size they are from 5 to 8mm. wide, and from 5 to 15mm. in length. Large masses have parts of crystals showing that they are from 30mm. to 60mm. in length.

The interior of the crystals is a fine pink color, but the outside is a light pink with a silvery lustre. The other minerals are not worthy of any special mention, except the columbite, of which a very fine perfect crystal was found, and that the muscovite alters to lepidolite. This locality was first worked and the minerals found in place by N. H. Perry of South Paris, Maine, to whose kindness I am indebted for information and use of specimens.

A NOTE ON THE FINDING OF TWO FINE AMERICAN BERYLS. By GEORGE F. KUNZ of New York, N. Y.

[ABSTRACT.]

IN addition to the topaz and other minerals from Stoneham, Oxford Co., Maine, the finding of two beryls, exceptional for American occurrence, may be mentioned. These were found several miles apart, and several miles from the topaz localities. They were discovered by chance within the last two years, by farmers, in pastures in this township. The first is only one-half of the original crystal and has been roughly used by some one who, possibly, discarded it as worthless after breaking, or else broke it in taking from the rock and then rejected it, not knowing its value.

This crystal is 120mm. long and 54mm. wide, and was evidently 130mm. long and 75mm. wide. The color is a rich sea-green as viewed in the direction of the longer axis of the prism and sea-blue of a very deep tint through the side of the crystal. The color and the material in this crystal are the finest that have been found at any American locality and, if not broken, the crystal and material would be equal to the finest foreign specimens known. It will, if cut, furnish the finest large gem of this mineral found in the United States, one gem weighing at least thirty karats, and a number of small ones weighing from one to six karats.

The other crystal is doubly terminated, being 41mm. long and 15mm. in diameter. Over one-half is transparent with a faint green color, the remainder being of a milky green and only translucent. At the juncture of the two colors the crystal has the appearance of a solution in which a flocculent precipitate has almost completely settled leaving the upper portion nearly clear.

The observed planes are, O, largely developed, 1 and 2–2; prism, I and i–2. The finding of the two crystals in such a manner can but lead one to think that rich material must be stored in the vicinity and would warrant farther search.

SECTION F.

BIOLOGY.

ADDRESS

BY

PROFESSOR W. J. BEAL,

VICE PRESIDENT SECTION F.

AGRICULTURE: ITS NEEDS AND OPPORTUNITIES.

INSTEAD of presenting a summary of the progress made in biology during the past year, I have chosen, rather, to speak of the "Needs and Opportunities of Agriculture,"—a subject that has heretofore scarcely been mentioned at the meetings of this association. Within the past few years the progress of agriculture, which I use in its broadest sense, has been greater than ever before. This may be attributed to a variety of causes; such as the general thrift and intelligence of our people, and the advancement of science.

Many agricultural schools have been established, experiment-stations organized, the rural press has been much improved in quality and quantity, clubs and societies are flourishing, and thousands of granges have helped to stimulate thought and investigation. Though there is much chance for improvement, the U. S. Department of Agriculture in several of its sections has done excellent work. It is true, and it is strange that it should be true, that, until within a comparatively recent period, but very little of the best thought, even of civilized nations, has been devoted to subjects intended to advance agriculture.

Columella, eighteen hundred years ago, keenly felt the want of more thought in agriculture when he said, "Husbandry alone, which, without doubt, is next to, and, as it were, near akin to, wisdom, is in want of both masters and scholars. Of agriculture, I have never known any who professed themselves either teachers or students." Many of our states have freely appropriated money to conduct surveys in geology, mining, with a little attention given to zoölogy and botany, not neglecting to provide liberally for coast-surveys.

The nation, considering its age, has also been very generous with money in support of surveys of various kinds, including also, anthropology, construction of lighthouses, improving rivers and harbors, investigating the supplies of fish, and even astronomy has been generously supported. It is true that some of this work performed by the government has been very poorly done, and has been enormously expensive; but the methods of work are improving.

This munificence of the United States in support of science is encouraging, and, as far as it goes, speaks well for the country and our law-makers. Doubtless, in many cases, the close connection with politics is one great hindrance to conducting successfully investigations in science for the government. The chances of losing positions are often too great to make them desirable, especially to persons who dislike political contests. Frequent changes are fatal to good, long-continued work.

Notwithstanding the large sums of money expended by our national and state governments in support of science, but a small sum, considering its importance, has been appropriated in the interest of agriculture. Even private gifts have gone to endow literary colleges, schools of physical or natural science, astronomical observatories, public libraries, and not to endow something which is directly intended to encourage agriculture. The men like Lawrence, Sheffield, Smithson, Peabody, Washburn, Swift, Stevens, are numerous, but not numerous enough. All honor to the noble names of those who have so generously contributed to the advancement of science!

To illustrate the hesitancy of men to bequeath money for the promotion of agriculture, I take the following from an address given by President T. C. Abbot:—

"I met a very pleasant and intelligent gentleman, who, from his large wealth, was about to give some sixty or seventy thousand dollars for the advancement of higher education. He had been for some years, and was still, the president of a state agricultural society. He was a farmer. Did he then endow some chair of agriculture, or agricultural chemistry, of veterinary science or horticulture? Did he fit out an experiment-station to analyze fertilizers, to study the value of cattle-foods? Did he establish an agricultural library? None of these. He found the science that was the most advanced of any, the one that government supports at a great expense from the public treasury. This farmer gave his thousands to endow another workshop of astronomy."

Yet, even in respect to private endowment, there are approaching signs of better days for agriculture. A few far-seeing men have observed the needs of this interest, and have set a noble example by giving of their wealth bounteously. Cornell, Bussey, Purdue, Valentine, Storrs, in this country, are names which will long be honored for their liberal gifts in the interests of agriculture. They showed great sagacity, and not a little originality, by placing endowments in a new field, where gifts are few, and the opportunities for good are boundless. It is hoped that these illustrious examples will stimulate others to make similar bequests.

Where agriculture thrives, there we always find a prosperous people. She needs more trained minds to work in her interests. With better thought would come great and needed improvements in the agricultural department of the nation. It lacks means, strength, and stability.

The matter of plans, and the naming of a competent director of the geological survey, were referred to the National Academy of Sciences, whose suggestions the government authorities sought and adopted.

The same body, or the Standing Committee of this Association, or the members of the Society for the Promotion of Agricultural Science, would be amply competent to name a good man for commissioner of agriculture. Such a plan would throw the position more out of politics, and it would be more likely to run smoothly on, like the work of the Smithsonian Institution.

Greater permanency would tend to make the department more efficient, and help it to coöperate with the agricultural departments of the several states and the agricultural colleges and experiment-stations.

The leading object of these remarks is to call the attention of those who are working for the advancement of pure science to the great needs of agriculture, the grand opportunities for making discoveries, and the lasting gratitude which such workers are likely to receive from the people. Of course, we grant that all science is valuable, that much of pure science has a practical bearing, that no one can foretell what practical results may be reached by investigations in pure science; still there is a tendency among scientific men to ignore economic science.

I will illustrate my meaning. The U. S. Signal Service is generally supposed to have been established in the interest of science, with the avowed intention, also, of benefiting navigation. The benefits in these respects are certainly worth all they cost, but these are not all the benefits which the service should recognize.

I note the following as given by Dr. R. C. Kedzie some months ago, to illustrate the tardiness of science and the government to grant assistance promptly to the interests of agriculture.

"No industry, except navigation, is so completely at the mercy of the weather as agriculture, in its widest sense. In the magnitude of the interests thus threatened, agriculture outweighs all others in importance. Indeed, without the sustaining influence of agriculture, commerce itself would vanish like the dew of morning. Timely warnings of impending meteorological dangers might be given by the signal-service, which would be of incalculable worth to agriculture."

He illustrates the subject by referring to the protracted rainy weather during the wheat-harvest of 1882, in Michigan, where the loss was very great. "The approach of a protracted storm was known for days before the damage was done. If specific warning had been given our farmers at that time, most of the wheat might have been safely housed, and the farmers of Michigan saved from a loss of more than $1,000,000. The damage inflicted in this way is not isolated and exceptional."

At length the growers of cotton and tobacco in the south, and of cranberries in New Jersey, have been recognized by the government, and warnings of approaching frosts have been promptly given. "The general government, through the signal-service, should hold the shield of its protection over land as over sea, over corn fields as over tobacco-plant, over hay-field as over cranberry-marsh, over wheat-field as over cotton-plantation, over orchards and vineyards, and the cattle upon a thousand hills and prairie leas. Why not extend this work into wider fields by doing for the *producer* what it has so well done for the *carrier?*"

The opinion seems to be too prevalent that few experiments in agriculture are worth attempting, unless it be those conducted by a chemist. This is by no means the case, though it is true that none but a chemist is capable of making those of a certain nature.

A physicist will still find in the soil much to interest him, and there is, no doubt, a chance to make discoveries valuable to agriculture.

With regard to the great importance of investigations and united action concerning the control of various plagues of our domestic animals, we should suppose no one would give a dissenting voice. Some valuable investigations have been made concerning the cause and nature of these diseases, among the most interesting of which, it seems to me, are the experiments made by Dr. Salmon in using an attenuated virus for inoculating animals, and inoculating again and again with a stronger virus those not affected by the attenuated virus. If the subject of animal plagues and the means of controlling them were fully discussed at meetings of this association, it would tend to allay prejudice, enlighten the minds of our citizens, and stimulate our law-makers to action. That there is need of a more general knowledge of this subject, I quote from a recent article by Professor Law in the proceedings of the Society for the Promotion of Agricultural Science. "The present agitation on behalf of legislation for the extinction of this lung plague in America began actively in 1878, and, notwithstanding that the subject has been continually before federal and state legislators for four years, but little real progress has been made. Among the drawbacks that may be specially named is the ignorance of legislators, of executives, and even of electors, on this subject."

In learning how to feed domestic animals economically, there is a great opportunity for investigation. There is much of interest and value to be learned in reference to the causes of fluctuation in weight of animals which are carefully fed and watered in a uniform manner.

Concerning the great need of continued and increasing efforts to investigate our injurious and beneficial insects, I need say but little; for the subject has been kept before the people, and the people are always interested to know something about an insect as soon as it injures their crops, or causes them trouble in any way. There is especially much need of more experiments to find better remedies for injurious insects. Attention to this portion of the subject cannot fail to meet with some degree of success. Success here is sure to win the favor of every one engaged in agriculture. Success in finding good, cheap, and safe remedies for injurious insects will tend to make science popular, and make endowments for research much easier and more frequent than ever before.

I need hardly add, that he who finds or breeds a race of honey-bees which is hardier, more industrious, longer-lived, quieter, possessed of longer tongues, and, last but not least, possessed of blunter stings, with less inclination to use them,—he who can succeed in any or all of these objects is entitled to rank with the man who shall cause two blades of grass to grow where only one grew before.

The U. S. Commission on Fish and Fisheries is an example of good scientific work, with prospects of early returns in the form of an increase in knowledge and a large increase in the supply of fish. A somewhat similar work, conducted by Prof. S. A. Forbes of Illinois, is in progress, where the object of the survey is to inquire into the food of birds and the food of fishes.

Some valuable scientific work of an economic nature has been done in connection with the tenth census, conspicuous among which is that performed by Prof. C. S. Sargent, in the study of forestry.

Botanical explorers in every land have repeatedly and liberally contributed plants of economic importance to the horticulturist,—a few new fruits, but more especially flowers and foliage-plants. An occasional contribution has been made to agriculture in the form of plants which promised to be of value for seeds or forage, or for some other purpose.

I have often been surprised that more attempts had not been made to secure the introduction of some new foreign grasses, and test them to ascertain their value for meadows and pastures. To be sure, grasses from western Europe have been tried; but we need others.

More than twelve years ago this idea appeared in my address on grasses, as given before the Northwestern Dairymen's Association, where the advice was given to yet other grasses from Japan, China, central Asia, and the dryer portions of South America. The cereals and pasture-grasses, the world over, are of more value to man and his domestic animals than all other plants taken together; yet the list of pasture-grasses now generally sown in any state can be counted on the fingers of one hand. In Great Britain, where much attention has been given to the subject, twenty-five or thirty species are much cultivated. It is hard to give all the reasons why so few grasses are employed in this country, but the fact remains that few are cultivated. The grass family is a large one, containing from thirty-one hundred to four thousand or more species. They are widely distributed in nearly all parts of the habitable globe, in every soil, in society with others, and alone. This does not convey an adequate idea of their value in unwooded regions, because the number of individuals of several of them is exceedingly large.

I have recently found the following in the American Agriculturist for 1858, a statement probably made by Dr. Thurber. "A dozen sorts, probably, cover nineteen-twentieths of all the cultivated meadow-land from Maine to Texas. It can hardly be supposed that so limited a number meets, in the best manner possible, all the wants of so great a variety of soil and climate. This is one of the pressing wants of our agriculture. Experimental farms are needed where the value of new grasses and kindred questions can be determined. A single new grass, that would add but an extra yield of a hundred pounds to the acre, would add millions of dollars annually to the productive wealth of the nation."

Still farther back, in 1853, the late I. A. Lapham of Wisconsin expressed similar views; and still longer ago, it 1843, forty years ago, in a prize-essay, J. J. Thomas said, "The great deficiency in number and the variety of our cultivated grasses has been long felt by intelligent cultivators." In this subject but very slow progress has been made in forty years.

In the extensive unwooded regions west of the Mississippi, the native grasses afford much pasture; but many of them start very late in spring, and stop growing early in autumn. They do not completely occupy the ground: they are easily stamped out by the hoofs of cattle and sheep. Some of the tame grasses will thrive better, and afford much more pasture.

In Science, vol. i, p. 186, of this year, Prof. N. S. Shaler refers to this subject. He says: "It seems possible to improve this pasture by the introduction of other forage-plants indigenous to regions having something like the same climate. The regions likely to furnish plants calculated to flourish in a region of low rainfall include a large part of the earth's surface. Those that would succeed in Dakota are not likely to do well in Texas or Arizona. For the northern region, the uplands of northern Asia or Patagonia are the most promising fields of search; while for the middle and southern fields, the valley of the La Plata, southern Africa, Australia, and the Algerian district, may be looked to for suitable species." He recommends three experiment stations,—one in Nebraska, one in Texas, and one in Arizona.

In this connection, when we remember that exotic plants often thrive better than natives, we see what a vast field lies ready for experimenting with the grasses.

Grasses look much alike to all who have not closely studied them; so that farmers, in fact, none except botanists, are likely to attempt experiments. This is a strong reason why the state and national governments should assist agriculture in an undertaking which seems so fruitful of good results within a short time, at so trifling an expense. Expeditions are sent at great expense to explore polar seas, with a view to slightly extending our knowledge of a barren portion of the earth's surface. Large sums are employed to fit up in magnificent style, and to send to the remotest parts of the earth, expeditions to spend a few minutes in observing an eclipse or a transit of Venus. Would the sending of competent persons around the earth in search of better grasses be an undertaking less praiseworthy?

The men who control the Northern Pacific Railway were enterprising enough to see that a complete economic survey of the adjacent territory would help the sale of their land. Among other things, the grasses will be carefully examined.

For the past ten years the writer has been testing, in a small way, some hundred and fifty species of grasses. These, with few exceptions, are natives of the eastern United States and western Europe. I am fully convinced that further experiments, carefully made on a larger scale in several portions of our country, would be quite sure to result in great gain to agriculture.

Grasses suitable for the western prairies, to take the place of those which will be rapidly stamped out by close feeding, are sure to be found even without the aid of the government; but greater time will be required.

Prof. E. M. Shelton of Kansas Agricultural College has probably done more than any one else in the west to test grasses and clovers, and diffuse imformation in regard to the results, which are most gratifying. At nearly all the gatherings of farmers in the west, this question of new grasses is a prominent topic of discussion.

Wherever irrigation has been well tried, especially on land which is light and well drained, the results have been quite surprising, converting a dry, hungry meadow into a little oasis. Such a meadow is the triumph of agricultural art.

One of the most remarkable results of irrigation, as viewed by a scientific man, is this: the list of grasses will not remain the same, or maintain the same proportion. The bad grasses will nearly all die out, or improve in quality; while the best ones will rapidly increase. And again: experiments in England have shown that irrigation causes many herbaceous plants, distinct from grasses, such as plantain and buttercups, to give place to good grasses. Docks are not diminished by irrigation. The best grasses are a sign of good land in fine condition. Such grasses are hearty feeders, and are most sensitive to good treatment. In a well-managed meadow, irrigation in four years increased the value threefold.

Solon Robinson long ago expressed the view, that, if the streams of the Connecticut were properly utilized in irrigating the soil, they would be more productive in value than by turning all the water-wheels of the state. More experiments in irrigation are much needed in this country.

Baron J. B. Lawes, a most renowned experimenter in agriculture, possessed an old pasture which had been in permanent grass over a century. No fresh seed of any kind was sown during this period. For some seven years or more, he experimented by applying to this old pasture, on different plats, twelve different kinds of manures. The results were very interesting and gratifying. "The manures, which much increased the produce of hay, at the same time very much increased its proportion of graminaceous herbage. The total miscellaneous herbage (chiefly weeds) was the most numerous in kind, and nearly in the greatest proportion, on the *unmanured* land,—viz., sixteen per cent,—while on the manured plat it decreased to two per cent. Every description of manure diminished the number of species and the frequency of occurrence of the miscellaneous or weedy herbage. A few weeds were increased by the manures, such as Rumex and Achillæa."

"The plants of a meadow," in the words of the Agricultural Gazette, "live in harmony on the unmanured open park, having nothing to fight for in a state of nature; but toss them a bone, ground fine, or any other choice bit, and their harmonious companionship terminates at once. Every act of improved cultivation occasions instant war. A grass likes the best that can be got.

It will swallow soda, but not when it can get potash. As a general principle, all manures tend to drive out the weeds by increasing the better herbage." A repetition of like experiments in this country could not fail to give valuable results.

In Europe some success has been reached in selecting and cultivating different varieties of *Lolium perenne*, *Dactylis glomerata*, and *Trifolium pratense*.

The field is a promising one for any careful and enthusiastic student. For three years past, I have been studying hundreds of plants of red clover at all seasons and stages of growth. I have plants growing, the seeds of which came from marked plants which varied much from each other. Plants in the fields of red clover vary amazingly in many respects, which influences their value for forage-crops. I believe our fields of red clover to-day contain nearly or quite as great a variety of plants as would a field of Indian corn, if we were to mix in a little seed of all the varieties cultivated in any one state.

Some of our grasses in cultivation are quite variable, notably the fescues, orchard-grass, and perennial rye-grass. It was some time ago observed that alfalfa of California, and lucerne of Europe, were quite different in their capacity to endure dry weather, though they belong to the same species. Different treatment in widely separated countries for many years has wrought a great change.

The subject of changing seed, planting old seed, mixing seed, raising it one year or more in a remote country and then returning to the starting-point, deserves the attention of careful experimenters.

The late Charles Darwin experimented on the effects of cross and self-fertilization of plants, and found that in most cases plants from crossed stock were earlier, hardier, germinated better, and yielded more seeds, than those from seed of self-fertilized plants, while crossing with foreign stock of the same variety is a far greater improvement. The idea is to cross the flowers of a plant with pollen from other plants of the same variety, the seeds of which were raised pure for five or more years in a remote locality, fifty miles or more away.

Mr. Darwin said, "It is a common practice with horticulturists to obtain seeds from another place, having a very different soil, so as to avoid raising plants for a long succession of generations under the same conditions; but, with all the species which freely intercross by the aid of insects or the wind, it would be an incomparably better plan to obtain seeds of the required variety, which had been raised for some generations under as different conditions as possible, and sow them in alternate rows with seeds matured in the old garden. The two stocks would then intercross, with a thorough blending of their whole organizations, and with no loss of purity to the variety; and this would yield far more favorable results than a mere exchange of seeds."

In a word, with plants which may be easily crossed, get some foreign seed of the same sort and mix with your own seeds to raise seeds for ensuing crops.

In 1877 I began some experiments of this kind with Indian corn and with beans, and have since made others. The advantages shown by crossing of corn over that not crossed was as 151 exceeds 100, and in the case of black wax-beans it was as 236 exceeds 100. Since then similar experiments have several times resulted in showing a large increase in favor of crossing with foreign stock.

In a review of Darwin's book, the Gardener's Chronicle of England said in 1877, "It is certain that these practical results will be a long time filtering into the minds of those who will eventually profit most by them." The results of my experiments have been widely printed in the agricultural papers of the day, and have been given at numerous farmers' institutes and granges, beginning in the winter of 1877, nearly six years ago; and yet I cannot learn that any other person in this country has attempted similar experiments. I will make one exception, in the case of Prof. W. A. Henry of Wisconsin University, who tried the experiment in connection with myself. The results, so far, fully accord with the prophetic statement above quoted from the Gardener's Chronicle.

In originating new varieties and races, see what has already been done, largely in our own country, in a haphazard way, with strawberries, raspberries, blackberries, gooseberries, and grapes, to say nothing of improvements in ornamental plants.

I need hardly add, that some of the best results, considering the means and time employed, have been obtained by persons who have crossed and hybridized according to some well-devised plan.

Our varieties of fruits in cultivation have become so numerous, that to describe them by the fruit and foliage alone often baffles the skill of the most expert pomologist. In the proceedings of the American Pomological Society for 1877, 1879, and 1881, I have shown that much help can be obtained by noticing the peculiarities of the flowers of apples and pears. The same is no doubt true, to some extent, with grapes, peaches, gooseberries, and other fruits.

Here is a promising field, full of interest to the botanist,—a field where he may accomplish much to aid the horticulturist, and something to advance science. A new variety of any cultivated fruit can no longer be considered as well described, unless some account be made of the flowers.

It has often been shown that many kinds of insects are beneficial to plants by aiding the fertilization of the flowers. The subject has still about it much that is new. Even Mr. Darwin said he did not suppose that he fully understood all the contrivances for fertilization in any one flower.

If it may be true, as my experience during the past six years help to indicate, that bumble-bees aid in fertilizing red clover, then farmers should try and encourage these interesting insects, even though they be disagreeable companions. Bumble-bees prefer to raise their colonies in old nests of meadow mice. I mentioned in my last report, that it had been suggested that we should not keep many cats, nor allow hawks, foxes, or dogs to catch these mice; for they make nests which are quite necessary for the bumble-bees, which help to fertilize our red clover, and thereby largely increase the yield of seed.

Perhaps it may not be altogether visionary to predict that men will yet engage in raising bumble-bee queens, and sell them to farmers at a fair profit, for starting colonies to improve the yield of clover-seed. We may yet have conventions and societies where the leading object shall be to discuss the merits of different sorts of bumble-bees.

A few years ago experiment-stations in Europe began testing seeds which were offered for sale in the markets. Adulterations were discovered most ingenious in character, harmful in effect, and remarkable in amount.

The more the subject was investigated, the worse it seemed to be. Something of the same sort has been undertaken in this country, showing that even in Michigan some worthless seeds are put on the market. In 1877 and later I tested large numbers of vegetable-seeds purchased of fifteen of our large dealers and growers. Not one of these is free from selling seeds that are worthless. The remedy is not easy. On account of its effect on their advertising, publishers are unwilling to print for their readers the results of these experiments. Only a few people can acquire the information after experiments are made.

In making tests of seeds, we still lack information in regard to the surest and best mode of testing each sort. Here is a good work for some accurate and ingenious scientist to invent new apparatus, learn the proper amount of heat, air, and moisture, for producing the best results, find out whether seeds will thrive best with a constant temperature, or a variable temperature; and learn the best modes of preserving seeds alive from one year to another.

I need hardly mention to intelligent students, that there is an extensive field, a very attractive one, in the study of fungi. The agriculturist, who deals with plants, not only wants to know the kinds, but the requirements which are favorable or unfavorable to their development. In the study of effectual remedies against fungi, something has been done; but there is still much demand for more knowledge. Successful experiments in regard to fungi are not likely to be made except by botanists.

I have only glanced at a few points where the biologist can find interesting work which will give threefold returns by advancing science, helping to elevate agriculture, and benefiting our country. There are many experiment-stations in Europe, and some in this country. We hope their number may soon increase, and that liberal and permanent endowments will not be lacking. This Association, and all other societies working in the interest of science, can render a great service by doing what they can to encourage experiments in all departments of agriculture. Men can be encouraged to prepare papers, and committees can make reports pertaining to the subject. There is a need of thorough state surveys, solely with a view to the interests of agriculture and kindred subjects. More knowledge of our soils, water, building-materials, plants, timber, injurious fungi, insects, and birds, would return to a state fivefold the cost of acquiring such information. In brief, then, as one of the humble workers in the interests of agriculture, I most cordially invite you to turn your attention to some of the problems which vex the husbandman.

PAPERS READ.

AGRICULTURAL BOTANY. By E. LEWIS STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

IF kitchen garden plants be closely studied in many varieties, it will be found that selection has differentiated the various natural species in accordance with desired uses.

It will be noticed that while there is a striking uniformity within varieties in those portions of the plant which have not been selected for improvement, there is a great variation between those portions which have secured attention on account of their uses. Thus, in forty-five varieties of onions growing side by side, the foliage is all similar, yet the bulbs vary in size, color, shape and habit of formation. The twenty-two varieties of carrots present like foliage, yet unlike roots. In sixty kinds of lettuce a likeness of bloom and great unlikeness between plants while of edible size. Among sixty-six kinds of tomato a sameness of bloom, certain variations in foliage and growth habit, and a very marked diversity in form of fruit borne.

It follows from a careful observation of over eleven hundred named varieties of kitchen garden plants, that two series of variations can be distinguished: the least marked, the normal variations that occur between the parts not subjected to conscious selection as being of little account for use; the very marked, or artificial variations that are produced by conscious selection exerted upon the parts which are valued for their use, and which selection has been exercised most stringently upon form.

It will be further noticed that according as selection for the same purpose has been exercised upon the various parts a parallelism of development has taken place between plants of different species, genera and orders. This parallelism of development points to a unity of arrangement in the forces by which the correspondences are produced.

The effect of selection concentrated upon visible forms has been to produce and fix changes from the natural plant to such an extent as in cases to mask the original species so that historical data must supplement morphological data in order to connect the genetic record. Thus, for illustration, there at first seems no specific connection between certain cultivated kales, cabbages and cauliflowers, while a specific connection of these with the ruta-baga seems uncertain. It is only as these forms are produced through the sowing of the seeds of *Brassica oleracea* that their close relation becomes manifested.

In the case of some long cultivated plants the original prototype is unknown, and can be sought but by conjecture. Such is the case with many of our cereals, with lettuce, etc.

In some plants selection has been exerted in different directions, as, for illustration, with the beet, whereby the root beet and the leaf beet have been produced; the celery and the celeriac; the parsley and the Hamburg parsley; the onion and the top-onion, etc.

It is clearly evident that conscious selection is a powerful agency for the changing of form, and by long exercise can overcome the norm or type affixed by nature to a species. The direction of this change and its consequences are determined by man in the direction toward usefulness to him as distinct from nature's intent toward the maintenance of the species.

We hence have a different set of motives governing the domesticated plant than those which govern the feral plant.

The individuals of a natural species cluster about a common type, held in place by a natural environment reacting with the force transmitted by heredity. The individuals of an artificial species (domesticated variety) cluster about a common type formed and continued through man's agency whereby an assisted environment reacts upon heredity modified by selection in influencing plant form.

In the feral plant genetic resemblance, as observed or inferred, furnishes a method of classification in accord with the law of evolution as expressing harmonies of action of special causes. Through the reaction of natural heredity with natural environment, classes are already formed for us and but await our discovery.

In the domesticated plant the power of intelligence to eliminate, modify and direct the action of natural laws under a given purpose, introduces a new factor to influence plant growth, and forms designed for uses mask genetic resemblances in those portions of the plant where change means value to man. In accordance with the law of evolution, but under a new application, form becomes paramount as a motive over genesis: usefulness, freed from the motive of self-maintenance, becomes paramount over the motive of continuance of the species. Through the reaction of an assisted heredity with an artificial environment including conscious selection, in artificial plants, classes are also already formed for us, and but await our discovery.

If, in nature, classes are formed through considering genetic data as paramount, then the natural system is in force for classification. If, under art, classes are formed through human selection based upon form as paramount, then an artificial system of classification is as truly scientific, and expresses in like manner as true a set of relations, but relations of a different character, as does the natural system. In a scientific artificial arrangement we seize upon a quality or qualities which yield us information as to the motive or development for uses, and which furnish signs of natural laws as modified and influenced by man, and form of selected parts furnishes the key; in a scientific natural arrangement we seize upon a quality or qualities which, through equal correlations yield signs of natural laws, and the floral organs, as indicative of the motive of the wild plant's existence, furnish the key.

If these views are correctly stated, then it is seen that an Agricultural Botany as an annex to the Natural Botany is imperatively required for the purpose of furthering classification and indentification of domesticated plants, and such an annex must vary in its methods as widely from the methods of the natural botany as cultivated plants vary from feral plants, the key to the motive being in one case the form of parts in use, in the other the floral organs.

INFLUENCE OF POSITION ON SEED. By E. LEWIS STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

IN trials carried forward at the New York Agricultural Experiment Station last winter it was found that for an average, 91 per cent of but kernels, 88 per cent of central kernels and 98 per cent of tip kernels of flint corn germinated; while for dent corns 70 per cent of but kernels, 56 per cent of the centres and 60 per cent of the tip kernels germinated. Our trials were, however, all too few in number to justify generalization without further evidence in the field. The following table represents the result of planting 3420 each of but, central and tip kernels, the three kinds taken from the same ears, and representing the five but, five central and five tip kernels of each row of the Waushakum Flint corn.

There were five plats, each containing three rows of each kind of seed: 38 hills with 6 kernels in each, or 228 kernels to a row, or 684 kernels of a kind in a plat. Planted May 17, vegetated May 28, final count June 8.

VEGETATIONS.

Plat.	Buts.	Centres.	Tips.
I. A. 1	542	598	607
I. A. 2.	551	581	599
I. A. 3.	571	580	588
I. A. 4.	534	574	606
I. A. 5.	517	556	565
Total.	2715	2889	2965
Per cent.	79	84	86

Comparing these figures with the average obtained in other plats with ordinary seed of the same corn, as carefully selected.

Plat.
E. 6. 2376 kernels planted; 2019 vegetated, or 85 per cent.
E. 7. 1188 " " ; 1006 " " 84 "
D. 1. 1126 " " ; 790 " " 70 "
D. 2. 1126 " " ; 776 " " 69 "

In a field trial on a smaller scale in 1882, 70 per cent of the but, 96 per cent of the central and 97½ per cent of the tip kernels vegetated. For the crop, calculating the merchantable ears to the 100 plants, we had,

Merchantable ears yielded per 100 plants,

	From but seed.	From central seed.	From tip seed.
Unmanured plat.	110	73	120
Manured plat.	112	107	117

Unmerchantable ears yielded per 100 plants.

	From but seed.	From central seed.	From tip seed.
Unmanured plat.	57	26	20
Manured plat.	27	14	12

Av. length of merchantable ears. Inches.

	From but seed.	From central seed.	From tip seed.
Unmanured plat.	7.57	5.55	8.13
Manured plat.	6.64	7.10	7.55

Preliminary trials towards establishing the limits within which our figures will apply, justify the conclusion that these figures offer, viz:—that for flint corn the tip kernels have the stronger vegetative power. It is part of the station work to establish the range within which variation may be expected to occur in agricultural work, or, in other words, to define the limits of probable error in various series of equivalent events, but our results are as yet not sufficiently advanced to offer to the public.

PARALLELISM OF STRUCTURE OF MAIZE AND SORGHUM KERNELS.

By E. LEWIS STURTEVANT of Geneva, N. Y.

[ABSTRACT.]

IF kernels of flint, pop, sweet and Tuscarora maize be split parallel to the germ, each race will be seen to present a definite arrangement of structure. Thus, the flint corn exhibits a germ surrounded by starchy matter, and this in turn by a corneous envelope; in the pop corn proper the germ is enclosed in the corneous matter, the starchy matter being absent, except as the pop variety entrenches upon the flints. The sweet corn has a similar structure to the pop, but the corneous matter is translucent and wrinkled; in the dents the corneous matter occupies the sides, the starchy matter extending through to the summit, and including the germ; in the Tuscarora the corneous matter is entirely absent, and only the germ and the starchy matter are to be made out.

If kernels of sorghum be split in like manner, a similar arrangement is to be found. Thus, the seed of the varieties distributed under the name of "Dhourra," "African Wheat," and "Millo Maize" correspond to the flint corn type, the germ surrounded by starchy matter, and this in turn included in a corneous envelope. In "Rice Wheat" we find the pop corn type, germ and corneous matter, no starchy matter being visible. In "Neeazana" we have a structure modelled upon the dent corns, and in "Chinese Sugar Cane," a structure modelled after the Tuscarora, or starchy matter and germ without visible corneous matter.

On account of the confusion of vernacular names it seems unnecessary to introduce further examples, although an examination of some thirty or forty so-called varieties shows that this observation is founded upon general characters.

In the case of the maize kernel we might assume that these differences have been produced by consciuos selection, the kernel being the portion used, and the various races being fitted for uses founded upon the structure. In the case of the sorghum, however, the supposition would scarcely answer, as the kernel is of small size, and apparently has not been subjected to conscious selection. In view of this parallelism of structure, together with the probability that a like selection has not been exercised upon both species, it seems safe to infer that these structural differences within each genus have more meaning than simple variety differences, and that the same causes have been at work, independent of man's conscious selection, to produce the races which in Zea and Sorghum have such well-marked and allied characteristics.

Relation of Root and Leaf areas in Corn. By D. P. Penhallow of Montreal, Canada.

[ABSTRACT.]

The exact determination of the relation between the aërial and subterranean portions of a plant is a question which has an important practical bearing, but the obvious difficulties in the way of its solution have apparently deterred observers from any special effort in this direction, and in my own case have resulted in rendering many attempts fruitless. Measurements made during the summer of 1882, however, have given us figures which may be considered reasonably accurate; they are therefore given here, not as fully conclusive, but as indicative and for the purpose of enlisting the efforts of others in a similar direction, for a law which can be generally applicable in this case will only be secured after repeated trial and estimation. The plants selected for the purpose, were those of the white flint corn, four weeks from the ground. They were obtained without breakage of the roots by digging a

trench parallel with the row and washing out by means of a hose. The plants were then taken to the office and the roots carefully separated from each other under a strong pump stream. The measurements of roots and leaves of the same plants gave the following results.

STEM.		
Average circumference,	3.05	C. M.
Length,	18.00	C. M.
Total area,	54.90	S. C. M.
LEAVES.		
Whole number,	8	
Total area of one surface,	381.07	S. C. M.
Total area of both surfaces,	762.14	S. C. M.
Total area of transpiration,	817.04	S. C. M.
ROOTS.		
Primary root, total length,	22	C. M.
Laterals of primary,		
With branches,		
Whole number,	78	
Total length,	510.30	C. M.
Average length,	6.54	C. M.
Branches.		
Whole number,	1339	
Total length,	2075.45	C. M.
Average length,	1.55	C. M.
Without branches.		
Whole number,	207	
Total length,	353.90	C. M.
Average length,	1.71	C. M.
Secondary roots.		
With laterals.		
Total length,	247.00	C. M.
Laterals.		
Without branches.		
Whole number,	1086	
Total length,	3138.84	C. M.
Average length,	2.89	C. M.
With branches.		
Whole number,	18	
Total length,	95.63	C. M.
Average length,	3.6	C. M.
Branches.		
Whole number,	131	
Total length,	85.63	C. M.
Average length,	0.73	C. M.
Without laterals.		
Whole length,	74.80	C. M.
TOTAL LENGTH OF ROOT,	66.829 M. or 217.762	feet.

DIAMETERS.

A	Primary roots.		
	Secondary roots.	0.123	C. M.
B	Laterals of primary root,	0.048	C. M.
C	Branches of primary laterals.		
	Branches of secondary laterals.		
	Laterals of secondaries,	0.036	C. M.

CIRCUMFERENCES.	
A	0.3864 C. M.
B	0.1507 C. M.
C	0.1130 C. M.
AREAS.	
A	132.844 C. M.
B	130.234 S. C. M.
C	604.047 S. C. M.
TOTAL ROOT AREA.	867.125 S. C. M.

In making these determinations, each root and fragment was measured, so that the entire length passed under inspection. The measurements were all made under a dissecting glass. The leaves were measured by dividing them up into a number of trapezoids and measuring each separately. By these precautions it is believed the unavoidable error was reduced to a minimum. It is proper to mention here that this work was rendered possible only through the hearty coöperation of two devoted students, Miss Chapman and Miss Hatch. The error remaining in the figures as presented, arises from inaccuracy in leaf measurement, and though inconspicuous, would have a tendency to make the results a trifle too small. The error in estimation of the roots would be more conspicuous, but would make the results too large. In view of these facts, it seems highly probable that the root and leaf areas normally approach a strict equality. If such be the case, then we might expect to find the root hairs, or organs of absorption, equalling in functional power, the stomata of the leaf or transpiratory organs. Numerous attempts to determine the number of root hairs upon a given area have always met with great difficulties. As new hairs are constantly being formed and old ones as rapidly passing away, or, what is worse for our purpose, becoming dead but still persistent, it is quite impossible to determine just where to draw the line of functional activity and select only those which are strictly equivalent to the action of the stomata in the leaf.

The results obtained, however, from a large number of averages, showed a total of 10561.00 per S. C. M.

As a result of estimation by three separate observers, the stomata were found to be for the

Upper surface of the leaf,	5411 per S. C. M.
Lower surface,	6980 per S. C. M.

These figures were obtained from a very large number of averages, and may be considered reasonably correct, since the extreme percentage error for the three observers was not more than two and one-half.

Comparing the whole number of stomata for both surfaces of the leaf, 6195.5, with the whole number of root hairs, 10561.00 per square centimetre, a very considerable discrepancy will be found in favor of the latter. It should be borne in mind, however, that while it is comparatively easy to determine the proportional part of a stoma which occupies a given square of the micrometre, it is not so easy to determine exactly the full number of root hairs in the same area, and thus serious error is sure to arise. Again, the determinations were made upon a basis of 1-10000 of a square centimetre, hence all errors are multiplied just 10000 times. Therefore, if we go back to the original area of estimation, we shall have for the stomata 0.61955, and for the root hairs 1.0561 per 1-10000 of a square centimetre. While this number may represent an approximation to the true ratio, we cannot assert with any assurance that the relation is at all well defined so far as mere numbers are concerned. The size of a stoma is subject to little or no variation after it reaches the period of functional activity, and its power to permit the transpiration of a given quantity of water is controlled only by the continuation of its vitality and not by its stage of growth. In the root hairs we have organs which are performing their functions during the entire period of an active growth. New hairs are constantly arising where old ones are becoming useless, and thus with such diverse lengths, different areas may represent entirely different quantities of functional energy. Moreover, the number of root hairs to a given area is largely controlled and modified by the special conditions of growth, while the same is not true, in the same degree at least, of the transpiratory organs. Thus in time of drought, the roots develop an excess of root hairs to supply the demands of the leaves which remain more nearly constant. At such times, however, we do not find an increased development of leaf and stomata, for the plant already has too many. On the other hand, there is a strong check upon their further development and thus from a twofold direction, there comes in a strong element in favor of a disproportion between the special organs of transpiration and those of absorption. Again, from the fact that the water taken up by the roots is partly used up in the chemical processes of the plant, while the remainder is transpired, it follows that the root hairs must either exceed the stomata in numbers or in size and functional energy.

All this taken into consideration would lead us to believe that, while under wholly normal conditions of growth, there may be a more or less definite numerical relation between root hairs and stomata of the leaves, under ordinary conditions of growth this relation is more strictly dependent upon functional considerations alone, and that it cannot be exactly expressed by numbers. Whether we recognize the root hairs as strictly the functional equivalents of the stomata or not, it is true that we must admit the functional equality of the roots and leaves as a whole. An extension of this thought would lead us to see that these organs are equivalents in the first place as mere areas of extension; in the second place their equality functionally depends upon (*a*) their degree of specialization and (*b*) the necessity for specialization of certain parts. As a matter of fact, if the roots absorb at all as such, this power is confined to a short distance near the tip where growth is active but it ceases as soon as the epidermis is replaced by a corky layer, and this occurs very soon.

If the leaves, as such, perform the functions of transpiration, their power in this direction is very largely controlled by the special conditions of growth as modifying their structure, since we know that, while under ordinary circumstances, the epidermal structure is so developed as to permit a certain amount of transpiration, in many cases, as in cacti, there is a specialization of this structure for the express purpose of retarding loss of moisture. We must consider, therefore, that, primarily organs of extension, the functional value of both roots and leaves must depend chiefly upon the degree in which certain parts are specialized, and this we must look for in the hairs and stomata. As we have seen, moreover, that roots in general are incapable of absorbing moisture beyond a very limited extent, while leaves and all green parts of the plant not too highly cuticularized may partake of the transpiratory function, we may reasonably assume that part of the inequality shown in our figures may be accounted for, and is compensated for in this way.

In the absence of confirmatory observations, we must assume for the present, that these indications are correct; that nature maintains a constant and well defined relation between the portions of the plant exposed to the air and that covered by the soil.

Careful reflection cannot fail to show the disturbance of normal functions which must invariably follow any act upon the part of man, by which this relation is disturbed. Applications of the principle must readily suggest themselves to the intelligent reader in such operations as transplanting and similar processes where a portion of the root is likely to be removed.

ORIGIN OF THE FLORA OF THE CENTRAL NEW YORK LAKE REGION.

By WILLIAM R. DUDLEY of Ithaca, N. Y.

AN ABNORMAL ORCHID. By WILLIAM R. DUDLEY of Ithaca, N. Y.

[ABSTRACT.]

Two years since the writer found some thirty specimens of *Habenaria hyperborea* growing in dry ground under beech trees, all the flowers of which were destitute of spurs; and in all cases the labellum was so changed in form as to resemble the labellum of *Habenaria dilatata* the nearest congener of *H. hyperborea*. It will be seen from the drawings accompanying this paper as read, and from the specimens themselves, that it is not a case of ordinary "peloria."

The condition of peloria is excellently illustrated by drawings of an abnormal *H. tridentata* which accompany this paper, as read.

This case of *H. hyperborea* comes under no one recognized class of teratological variations, but is evidently caused by an arrest of development; and if we may suppose the general law to hold that variations produced by this cause usually show a more primitive form of the species, then this variation of the lip or labellum toward *H. dilatata* indicates the latter species to be the older species or a more primitive type, while *H. hyperborea* may have originated from it. The absence of the spur would seem to point to the conclusion that it was one of the latest modifications of the Habenaria flowers, the one most easily lost in any unusual strain upon the plant's resources.

NOTE ON THE PRESENT CONDITION OF THE BOX HUCKLEBERRY, VACCINIUM BRACHYCERUM MCHX., IN PERRY COUNTY, PA. By E. W. CLAYPOLE of Akron, Ohio.

[ABSTRACT.]

MICHAUX described a species of Huckleberry from Virginia under the name of *Vaccinium brachycerum.* This plant cannot now be found in either of the two habitats mentioned by that author. It is possibly extinct. Its chief habitat now is in Perry County, Pa.

The plant occurs on a hill side composed of the Chemung shale near New Bloomfield. It is very abundant on this spot but its range is very small and remarkably restricted. It does not cover more than about ten acres of ground in a piece of woodland, but the causes which determine its limitation are not easily seen. In one direction it grows down to a small run forming a perfect mat to the water's edge and on the other side only two yards away not a plant can be seen. In the wood also it is possible to walk with one foot buried in a mass of this plant and the other where not a specimen is growing. The line of its growth is marked almost as plainly as if by an invisible fence which it does not pass.

This pretty little huckleberry is hovering as it were on the very verge of extinction. The clearing of the few acres on which it grows and ploughing for one or two seasons would totally eradicate it and I know of no other place in the county where it occurs. I have heard that it is to be found on a neighboring hill but have not found it.

The fruit which is now ripe is edible but possesses less flavor than the other huckleberries. In appearance it most resembles the low blueberry being covered with a bloom. It is worthy of notice that it does not fruit freely. This year when the crop of other huckleberries is unusually large the berries of this species are scattered singly and sparingly over the plants. Possibly we have here one cause of its diminution. It flowered very abundantly.

The plant grows from six to ten inches high, is evergreen, but turns red in the winter.

The only other locality for it known to me is on the bank of Indian River about tidewater, in Newcastle Co., Delaware, where it has been found by Mr. Commons, who has kindly sent me specimens exactly resembling those of Perry Co.

A SUPPOSED POISONOUS SEAWEED IN THE LAKES OF MINNESOTA.

By J. C. ARTHUR of Charles City, Iowa.

[ABSTRACT.]

THIS paper gave the incidents which led to the apprehension of a certain alga of Minnesota lakes as the cause of considerable mortality among domestic animals. In July of 1882 several cows and young cattle, and a small number of horses and hogs died at Waterville, a small place about sixty-five miles south of Minneapolis, in such a manner that the probable cause was traced to the water they drank, which in every instance was that of the lakes in the vicinity. These lakes are of various sizes, Lake Tetonka, the largest, covering a little over two square miles, and are in no way peculiar except in being connected and drained by a small sluggish river. The water of the lakes and river at the time of the trouble was green with a small floating alga, which pervaded the water and formed a scum on the surface that the wind collected into masses along the shores. The alga proved to be the same as one described by Dr. Cohn of Germany under the name of *Rivularia fluitans*. The synonymy of the species has not yet been traced, and it may have been described under an earlier name. The Waterville plant, also detected during the present session in Lake Minnetonka, consists of little balls of the uniform size of mustard seed, of a dark olive-green color, turning reddish-brown when dead, which, contrary to the usual habit of members of the genus *Rivularia*, float free. The minute anatomy has been given elsewhere, and it is rather the design of this paper to call attention to the plant's supposed poisonous qualities. In both 1882 and the present year, by the time the spot could be reached after notification that the plants had attained their maximum abundance, the danger was passed, through the almost total disappearance of the alga, and in consequence no experiments could be undertaken. The history of the circumstances, elsewhere given in detail, shows that while the evidence of poisonous qualities in this plant is purely presumptive, it is seemingly well founded. The observations thus far have been under the direction of the Department of Agriculture of the University of Minnesota, and are to be continued.

Relations of certain forms of Algæ to disagreeable Tastes and Odors. By W. G. Farlow of Cambridge, Mass.

[ABSTRACT.]

Although large masses of any decaying vegetation may render water unfit for drinking, the only group of plants to be feared, as far as their effect on the taste and odor is concerned, is the members of the nostoc family, which form floating scums of a bluish-green color. When exposed to a bright sun, especially in shallow water, they are transformed into fœtid, repulsive-looking masses of slime, which give to the water the so-called pig-pen odor. The water-supplies of several eastern cities have been thus contaminated, and principally by species of Cœlosphærium, Clathrocystis, and Anabæna. In Minnesota is the representative of a fourth genus, Rivularia, which was first found last year at Waterville by Professor Arthur, and which has been found to be very abundant this year in Lake Minnetonka; and in all probability it occurs in most of the other lakes of this region. The singular fact is, that while unknown elsewhere in this country, the species was found several years ago by Cohn in Silesia, who named it *Rivularia fluitans;* and it was detected also by Gobi near the Gulf of Riga. It appears also to be very closely related to, if not identical with, an alga abundant in certain parts of England, referred by Harvey and more recently by Philips to *Echinella articulata* Ag. This is another illustration of the very wide distribution of the species of the nostoc family, of which we have other recent illustrations in the *Nostochopsis lobatus* of Wood, first described from the northern states, but which has since been found to be identical with *Mazea rivularioides* subsequently discovered in Brazil; and again in *Hormactis Quoyi* which is found only at Falmouth, Mass., and the Marianne islands in the Pacific.

There is a strong probability that, in the future, Minneapolis may be troubled by the decay of the different nostocs floating in the lakes near the city, where they are very abundant. As far as avoiding trouble from these plants is concerned, undoubtedly river-water is to be preferred to lake-water; but before many years the Mississippi near Minneapolis will be contaminated by sewage, and the water will probably then be obtained from the lakes. If the shallower lakes near the city are used, there can be little doubt that in summer Minneapolis will have the same trouble as that experienced in Boston. Even at greater expense, the water should be brought from large and deep lakes, especially those across which the winds sweep so as to keep the surface-water roughened.

THE SPREAD OF EPIDEMIC DISEASES IN PLANTS. By W. G. FARLOW of Cambridge, Mass.

[ABSTRACT.]

IN the case of animals it can be said, that, excepting the diseases attributed to bacteria, they are subject to but few diseases caused by fungi. In the case of plants, however, the greater part of the diseases to which they are subject are caused by parasitic fungi; excepting, of course, the injuries caused by insects, which need hardly be considered in speaking of epidemic diseases. Most of the violent epidemic diseases of plants are caused by fungi of the orders Uredineæ, rusts, and Peronosporeæ, rots. Fortunately, the species of these orders attack only a single species of host, or at most several species closely related botanically; so that, for instance, a rot which would attack the potato would not probably attack the grape, although it might be expected to attack the tomato, which is botanically closely allied to the potato. As might be expected, the most violent epidemics occur during, or just after, unusually wet periods. An epidemic disease spreads either by the dispersion of its spores through the air, or by the transportation of the host-plant on which it is growing; the latter being probably the means by which diseases are carried across large bodies of water, as the Atlantic.

With the introduction of food-plants from Europe to this country come, of course, many of their parasitic diseases. It should be noted, however, that the most violent plant-epidemics of recent times have advanced not from east to west, but from west to east. The best known case is that of the potato rot in 1845, and since then the very accurately recorded case of the grape-mildew, *Peronospora viticola*, has arisen. In the first case, the disease is supposed to have reached Europe from the west coast of South America, by way of the United States. In the latter case, the grape-mould, which is a native of North America, can, as I showed by experiments in 1876, be transferred to the European vine; and it was prophesied that the disease would extend to Europe and do more harm than with us. The prophecy was very soon fulfilled, as you all know. In the two diseases just mentioned, it is a characteristic of the spores that, in germinating, instead of giving off a filament, they discharge a number of motile zoöspores, each of which is capable of propagating the disease. We have several other species of Peronospora, which produce zoöspores, some of which have apparently crossed from America to Europe; and there are others which, although common in this country, have not yet appeared in Europe, although, following the grape-mould, they may be expected to appear there hereafter. Among these may be mentioned *Peronospora Halstedii*, which grows on composites, and may later be found in Europe on the Jerusalem artichoke. Professor Trelease has recently found a Peronospora on Sicyos in Wisconsin, which resembles the grape-mould in general appearance. The germination of the spores has not yet been observed, but judging by analogy one would expect them to produce zoöspores. It would not be surprising if the Peronospora on Sicyos should also be found hereafter causing a disease of squashes or melons; and its progress eastward might be expected as in the cases previously cited.

Reference was made to a modification of the spores sometimes observed in Peronospora. Mr. Earle of Cobden, Ill., collected species on Geranium and Viola, where, instead of the usual branching spore-stalks, the spores were borne on the mycelium close to the breathing-pores; the spores themselves being very much larger than in the common form. A similar monstrosity has been noted by Cornu in the grape-mould. The specimens were collected by Mr. Earle in April, and the speaker suggested that this form of spores might perhaps be an adaptation to the cold and wet weather of spring. The conditions which produce the monstrous forms are worth considering by collectors.

Of the diseases caused by Uredineæ which have advanced from west to east, the hollyhock-disease, *Puccinia malvacearum*, is the best-known instance. Its original home was probably Chili; but it spread through Europe about ten years ago, not, however, by way of this country, as was probably the case with the potato-rot. The diseases produced by fungi of other orders, as Ascomycetes, do not spread with the same rapidity as the rusts and rots. This is shown by the black knot, which is so destructive in this country to plums and some kinds of cherries. It is a native of this country, and is found on most of our wild species of Prunus, especially the choke-cherry, a shrub which has been introduced into many places in Europe. As yet, however, the black knot has not made its appearance in Europe.

The speaker then said he had just found the grape-mildew growing on the Virginia creeper (*Ampelopsis quinquefolia*) near Minneapolis. As this plant is closely related to the vine, the occurrence of the mildew might have been expected. In attempting to prevent the spread of the disease to countries where it is unknown, the discovery is of importance. It is evident, that, to prevent the spread of the disease, the importation of Ampelopsis as well as of grape-vines must be prohibited.

LEAVES OF THE GRAMINEÆ WITH CLOSED SHEATHS. By W. J. BEAL of Lansing, Mich.

[ABSTRACT.]

AUTHORS speak of the leaves of Cyperaceæ as having closed sheaths, while those of the Gramineæ have open sheaths. It is true, at even a very early stage, that many of the leaves of the Gramineæ have open sheaths, but all of our common grasses, so far as examined, have one or more of the lower sheaths closed. In many cases these lower sheaths are split or torn open as the stem and succeeding leaves enlarge. The same is true of the lower leaves of the branches. Some twenty species of grasses, mostly natives of North America, have been examined.

In case of *Arrhenatherum avenaceum*, three or more of the lower leaves have closed sheaths while there are are five or more in *Dactylis glomerata*. Species have been examined belonging to Alopecurus, Phleum, Poa, Festuca, Horedum, Colamagrostis, Lolium, Setaria, Panicum, Secale, Avena, Eragrostis, Sporobolus.

DEVELOPMENT OF A DANDELION FLOWER. By JOHN M. COULTER of Crawfordsville, Indiana.

[ABSTRACT.]

THE paper gives, as a result of personal investigation, testimony upon which are based the following conclusions in respect to this species.

1. The inferior ovary is produced by an arrest in the development of the floral axis, the rising in a peripheral ring of the floral organs, and the gradual arching over of the cavity thus produced by two carpellary leaves.
2. The syngenesious anthers are united by contact and pressure, but in no sense structurally.
3. The ovule is not produced directly from the axis, but is an outgrowth from the surface (probably the midrib) of a carpellary leaf.

A FACT BEARING UPON THE EVOLUTION OF THE GENUS CYPRIPEDIUM. By E. S. BASTIN of Chicago, Ill.

[ABSTRACT.]

IN June 1881, while botanizing at the southern end of Lake Michigan, I found a specimen of *Cypripedium spectabile* that was peculiarly interesting. I described and figured it at the time in "The Druggist," but it seems to me to be of so much significance in connection with the evolution of the Orchidaceæ in general and of Cypripedium in particular that I have thought best to bring it to the attention of this Association. The monstrosity to which I allude was an almost regular flower growing on the same stem with one of the ordinary form. In the usual form, the calyx consists of two whitish, broadly oval, obtuse, or scarcely pointed sepals, the lower one evidently composed of two united pieces. The corolla consists of two lateral white petals about as long as the sepals, but narrower. The lip occupies the position of the third petal and is a large and sac-like organ, often two inches long, pinkish, except on the posterior under portion, and decidedly pink on the anterior upper portion. The sides of the oblong aperture of the lip are provided on either side with an ear-like infolding. The gynandrous stamens are two in number and lateral, opposite the petals. The anthers face backward toward the column, which is declined. Occupying the upper part of the column, in nearly the position of the third stamen, is a large dilated-triangular, somewhat petal-like body which curves downward toward the interior of the lip and covers the stigma and stamens. The style is bent downward at right angles to the column and is about two-thirds as long. The stigma is dilated-triangular, obscurely and unequally three-lobed, facing toward the interior of the lip, and is not seen until the latter organ is bent downward, owing to the fact that it is covered by the broad rudimentary stamen. The ovary is inferior, six-ribbed, somewhat curved and twisted and one-celled with parietal placentæ.

The monstrous form which I found differed from this in possessing three broadly lanceolate sepals all alike and not at all united. It had no lip, but three regularly formed pure white petals all of the same size and shape, as long as, but somewhat narrower than, the sepals and alternating with them. Representing the first staminal whorl were two instead of one fleshy dilated triangular bodies occupying the normal position, *i. e.*, alternating with the petals. They resembled in thickness and in general shape the one in the normal flower and were also spotted like it, but were somewhat smaller and not bent but erect or nearly so. The second staminal whorl consisted of three fully developed stamens inserted on the column opposite the petals and closely resembling those of the normal flower in structure, except that coalescence with the style was less complete, and the connective, or projection at the back of the anther, was rather more conspicuous. The stigma was nearly equally and deeply three-lobed, the lobes alternating with the stamens. The column was but slightly bent, the ovary scarcely twisted and the flower was nearly erect. It will be observed that here, in a genus of plants affording some of the most strikingly irregular flowers in the vegetable kingdom, was a flower all but regular, and unsymmetrical only with respect of one thing, viz.: in not possessing even a vestige of the third stamen of the first staminal whorl.

If the specimen is to be regarded (as undoubtedly it should be) as an instance of atavism or reversion to an ancestral type, we have here a striking confirmation of the view botanists have reached of the typical structure of the flower of a monocotyledon, viz.: that it is a trimerous, regular and symmetrical flower possessing five whorls of floral organs. It tends also to establish the conclusion already reached by Robert Brown and other able botanists that the large dilated-triangular organ of the ordinary flower is the rudiment of a stamen belonging to the first staminal whorl, a view, which plausible as it seems on theoretical grounds, has nevertheless had but few facts for its support. No doubt the organ originated by the disappearance of the anther and the broadening and thickening of the filament and its extension, the connective. In fact, in the monstrosity under consideration the fertile stamens were when viewed from the back, very much like miniatures of the two rudiments of the outer whorl.

But the study of this monstrosity seems to throw some light also on the nature and origin of the lip of Cypripedium, and hence, by inference also, of that of other Orchidaceæ. Mr. Darwin, from a study of the arrangement of the spiral vessels in the lip of Cypripedium has reached the conclusion that this organ is a compound one, made up of the lower petal, two stamens of the outer, and one of the inner whorl. This view would seem a very plausible one indeed aside from the evidence furnished by the grouping of the spiral vessels, for it accounts at once for the disappearance of parts, for the enormous development of the lip, and for the two curious earlike infoldings of the upper margin. But if this monstrosity truly represents a stage in the development of Cypripedium, I do not see how this view can be maintained, for one stamen of the outer whorl had entirely disappeared and no trace was left behind, or any evidence from a difference in the size or thickness or venation of the petals that it had coalesced with one of them.

Then also if Mr. Darwin's view were the correct one, that the two stamens of the outer whorl had gone to form the outer part of the lip and the earlike infoldings, they would both have disappeared or coalesced with the third petal at the same time, but this specimen indicates that they disappeared one at a time. We must conclude, then, that the lip, if a compound organ at all, is made up merely of the lower petal united to the stamen of the inner whorl that would normally come opposite to it, and Dr. Crüger is right in denying that the homologies of the parts of flowers may safely be deduced from the course and grouping of the spiral vessels.

THE POSITION OF THE COMPOSITÆ IN THE NATURAL SYSTEM. By JOS. F. JAMES of Cincinnati, Ohio.

[ABSTRACT.[1]]

AN examination of the structure of the flowers of the Compositæ, with a suggestion that the Gamopetalæ should be considered as of a higher type than the Polypetalæ; and that the Compositæ should be placed at the head of the vegetable kingdom.

PERIODICITY OF SABBATIA ANGULARIS. By MARY E. MURTFELDT of Kirkwood, Mo.

ON THE TRITUBERCULATE TYPE OF MOLAR TOOTH IN THE MAMMALIA. By E. D. COPE of Philadelphia, Pa.

[ABSTRACT.]

IT is now apparent that the type of superior molar tooth which predominated during the Puerco epoch was triangular or tritubercular; that is, with one internal and two external, tubercles.[2] Thus of sixty-seven species of placental Mammalia of which the superior molars are known, all but four have three tubercles of the crown, and of the remaining sixty-three all are triangular excepting those of three species of Periptychus, and one of Ectoconus, which have a small supplementary lobe on each side of the median principal inner tubercle.

This fact is important as indicating the mode of development of the various types of superior molar teeth, on which we have not heretofore had clear light. In the first place this type of molar exists to-day only in the insectivorous and carnivorous Marsupialia; in the Creodonta, and the tubercular molars of such Carnivora as possess them (excepting the plantigrades). In the Ungulates its persistence is to be found in the molars of the Coryphodontidæ of the Wasatch, and Dinocerata of the Bridger Eocenes. In later epochs it is occasionally seen only in the last superior molar.

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[1] The paper is printed in full in the American Naturalist for December, 1883.

[2] See American Naturalist, April, 1883, page 407.

It is also evident that the quadritubercular molar is derived from the tritubercular by the addition of a lobe of the inner part of a cingulum of the posterior base of the crown. Transitional states are seen in some of the Periptychidæ (*Anisonchus*) and in the sectorials of the Procyonidæ.

The tritubercular or triangular superior molar is associated with a corresponding form of the anterior part of the inferior molar. This kind of inferior molar I have called the "tubercular sectorial,"[3] and is very variable as to the degree of development of the sectorial cutting edge. The anterior triangle is formed by the connection by ridge or crest, of the median and anterior internal cusps with the anterior external. Its primitive form is seen in Didelphys, Pelycodus, Pantolambda and the Amblypoda generally; in Centetes and Talpa; and in its sectorial form, in Stypolophus and Oxyæna, etc.

The mechanical action of such teeth is as follows. Of course it results from the form of the superior molars that the spaces between them are wedge shaped, the apex external, the base opening to the palate. The base of the triangular section of the anterior part of the inferior molar is interior, and the apex exterior; and when the jaws are closed, this triangular prism exactly fits the space between the superior molars. The lower heel of the inferior molar receives the impact of the crown of the superior molar. Thus the oblique edges of the inferior triangle shear on the edges of two adjacent superior molars. The anterior parts of the inferior molars form, with the superior molars, an alternating dental series, as distinguished from the prevalent opposed dentition of most mammalia. In so far it resembles the reptilian dentition.

This primitive dentition has been modified in two directions: viz., to form the grinding and sectorial dentitions. As already remarked, the superior molars gradually acquire a posterior internal lobe, which produces the quadrituberculate type. This lobe, by opposing the anterior internal lobe of the next posterior inferior molar, precludes the entrance of the anterior triangle of the latter between the two superior molars. Hence we find in the types which possess quadritubercular superior molars that the anterior triangle of the inferior molar is not elevated, if present, as for instance in Rhinocerus. It is, however, more frequently atrophied, and disappears, forming the inferior quadritubercular molar so well known.

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[3] See Report G. M. Wheeler, to chief of Engineers on Expl. Surv., W. 100th Mer. Vol. iv, pt. ii, on the Creodonta.

On the other hand, as I have pointed out[4], the anterior internal cusp of the triangle of the inferior molar may be more developed antero-posteriorly, giving the antero-external edge of the triangle much greater obliquity than the postero-external. In correspondence with this modification, the superior triangular molar loses its equilateral character by the more anterior position of its internal angle, thus elongating the posterior internal side. The latter thus fits the corresponding form of the triangle of the inferior molar, forming with it the shear of the sectorial tooth.

In a former article "On the Homologies of the Molar Teeth, etc.,"[5] I traced the modifications of the superior and many of the inferior molars of the Ungulate mammals to a parent quadrituberculate type. In a subsequent essay[6] I traced the origin of the inferior sectorial to a primitive five-tubercled or "tubercular sectorial" type. Farther than this I did not go, and made no attempt to derive the few cases of triangular superior molars then known, nor the type of the superior sectorial. The revelations of the Puerco fauna show, that the superior molars of both ungulate and unguiculate mammalia have been derived from a tritubercular type; and that the inferior true molars of both have been derived from a "tubercular sectorial" type. Shall we look for the origin of the latter in a tritubercular tooth also, *i.e.*, the tubercular sectorial without heel; and will the crowns of the true molars of the primitive mammals alternate with, instead of oppose, each other?

ON THE STRUCTURE OF THE SKULL IN DICLONIUS MIRABILIS, A LARAMIE DINOSAURIAN. By EDW. D. COPE of Philadelphia, Pa.

[ABSTRACT.]

THIS paper was based on a nearly perfect skeleton, found by Messrs. Wortman and Hill, in Dakota, of the *Diclonius mirabilis* of Leidy, a species closely related to the *Hadrosaurus foulkei*. The total length of the skeleton is thirty-eight feet; that of the skull 1.18 meters. The skull is much "like that of a goose." The dentition is remarkable for its complexity and for the difference between the superior and inferior series; it includes more teeth than the Hadrosaurus, there being 568 in each maxillary bone, and 378 in each splenial bone, making in all 1,892. Dermal or corneous structures have left distinct traces in the soft sandstone about the end of the beak-like muzzle. The affinity of the Dinosaurs to birds is confirmed by the skeleton, but it is empirical rather than essential, and is confined to a few points, as the form and position of the vomer, the large development of the premaxillary bone, and the toothless character of this bone and the dentary. So far as the author is aware, the extension of the premaxillary above the maxillary, so far as to overlap the lachrymal, is unique among Vertebrata.

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[4] On the origin of the sectorial tooth of the Carnivora. American Naturalist, 1875.

[5] Journal Academy Nat. Sciences, Philadelphia, March, 1874.

[6] On the origin of the sectorial tooth of the Carnivora. American Naturalist, 1875.

ON TWO PRIMITIVE TYPES OF UNGULATÆ. By E. D. COPE of Philadelphia, Pa.

[ABSTRACT.]

EXHIBITIONS of figures of restorations of *Phenacodis vortmani* and *Hyracotherium venticolum*.

PHARYNGEAL RESPIRATION IN THE SOFT-SHELLED TURTLE (ASPIDONECTES SPINIFER). By SIMON H. GAGE of Ithaca, N. Y.

[ABSTRACT.]

DURING the last twenty-five years, the mechanism of respiration in the Chelonia has been investigated with considerable thoroughness both in this country and in Europe; and at present the Chelonian form of respiration is considered to be comparable with that of the mammal rather than with that of the frog, as formerly supposed. While, however, the mechanism of respiration has been quite fully investigated, there has been, so far as I am aware, but one who has considered the organs of respiration in the different groups of turtles.

Professor Agassiz, in part II, of the "Contributions to North American Zoology" (p. 284), states that the lung capacity of the soft-shelled turtle is far less in proportion to its body weight than is that of the land turtles. He also states, in considering this fact, that the skin on the ventral side of the body, from its rich network of blood vessels, acts as a respiratory organ. He further states that in the pharynx are many fringelike processes which resemble the inner gills of tadpoles, and probably have the same function, although no mention is made as to the method of their use.

In 1878 while watching a soft-shelled turtle from Cayuga Lake confined in a glass aquarium, it was observed that the throat and the floor of the mouth became alternately swollen and collapsed, while the turtle was completely immersed in the water. The appearance was very much like the respiratory movements of a frog in the air. As no air escaped from the turtle the bulging of the throat and mouth must be caused by filling the mouth and pharynx with water, and expelling it, or the air must be forced into the mouth from the lungs and then forced back into the lungs, as is done sometimes by men when swimming under water.

In order to determine whether or not water was taken into the mouth and expelled, the bottom of the aquarium was covered with fine sea sand, and the observations were made when the animal was resting quietly on the sand.

At the beginning of the movement, the mouth would slightly open, and its floor would swell out, the swelling passing steadily onward to the throat. After a moment of quiet, the swelling would disappear in the inverse order of its appearance.

During the disappearance of the swelling of the throat and mouth, the sand for a considerable distance in front of the animal's head, would be swept aside as by a rapid stream. The movement of the sand, without the escape of air, seemed to prove conclusively that the mouth and throat were alternately filled with water and emptied.

These pharyngeal respirations, as they may be called, were very regular occurring ten to fifteen times a minute. My observations were verified by Professor Wilder and several of our laboratory students.

While therefore the investigations of Agassiz showed that in the pharynx of the soft-shelled turtle were organs apparently suitable for aquatic respiration, the observations here recorded of the rhythmical bathing of these organs with fresh water seem to make the evidence complete, that a true aquatic is combined with an aërial respiration.

It is hoped that during the coming year, investigations may be completed which shall determine the exact amount of oxygen consumed in this pharyngeal respiration, and the structure of this unusual respiratory organ in the soft-shelled turtle.

ON THE USE OF VASELINE TO PREVENT THE LOSS OF ALCOHOL FROM SPECIMEN JARS. By BURT G. WILDER and SIMON H. GAGE of Ithaca, N. Y.

THE petroleum preparation known as vaseline is known to be practically unaffected by ordinary temperatures and by most substances. In the Journal of the Chemical Society, July, 1882, p. 786, it is said to be "sparingly soluble in cold strong alcohol, and completely in hot, but separates out on cooling." After trying various substances,—wax, paraffine, oil and glycerine with but partial success, the use of vaseline was suggested by the two authors independently and nearly at the same time. The experiments tried this spring indicate that, during three months, at ordinary spring and summer temperatures, there is no appreciable loss of 95 per cent. alcohol from glass vials or jars, whether upright or inverted or on the side, provided corks are anointed on the bottom as well as on the side, provided ground glass stoppers are anointed and firmly inserted, and provided the rubber rings of fruit jars and the specimen jars made by Whitall, Tatum and Co., are anointed on both sides and the covers well screwed down.

We have also used the vaseline for preventing the loss of other liquids, excepting chloroform and spirits of turpentine; as a lubricator of drawers, and to prevent the sticking of the covers or stoppers of cement vials; and for the prevention of rust upon steel instruments.

THE PSYLLIDÆ OF THE UNITED STATES. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

THE Psyllidæ, or flea-lice, are rather small homopterous insects, that have remarkable jumping powers. Some of them injure cultivated plants. This is notably true of the *Psylla pyri*, which blights the buds of pear-trees; and *Phylloplecta tripunctata*, which crumbles the tips of the blackberry. The family has received little attention in the United States, and scarcely anything has been known of the life-history and development of the species. The paper enumerates seventeen described species, four of these being synonymes, and one of them (*Psylla pyri*) introduced from Europe. They fall into four subfamilies, and represent four genera already characterized, and three new genera,—Brachylivia, Pachypsylla, and Phylloplecta. The new species characterized are *Calophya vitreipennis*, from Arizona; *C. nigripennis*, on *Rhus copallina; C. flavida*, on *Rhus glabra; Pachypsylla celtidis-curcurbita*, forming galls on leaves of *Celtis texana*, *P. c.-pubescens*, *P. c.-asteriscus; P. c.-umbilicus* and *P. c.-vesiculum* — all forming galls on leaves of *Celtis occidentalis; Blastophysa* (nov. gen.) *c.-gemma*, forming galls on the twigs of the same tree; *Ceropsylla* (nov. gen.) *sideroxyli*, a remarkable form developing in pits on the leaves of *Sideroxylon masticodendron; Trioza sanguinosa*, on *Pinus australis; T. sonchi*, on *Sonchus arvensis;* and *Rhinopsylla schwarzii*, from the cypress-swamps of Florida. The paper records discoveries as to the life-history of the species, and especially those affecting Rhus and Celtis; the latter forming a group peculiar to North America, and the most perfect gall-makers in the family.

The eggs of Psyllidæ are attached to leaves by a pedicel, are somewhat pointed at one end, and often terminate in a filament. The young are broad and flattened, with a fringed margin. They are generally pale, and more or less covered with a flocculent secretion. Those on sumach are dark, and without such flocculence. Those making galls on hackberry have stout spines at the end of the body, by the aid of which they are able to work out of their galls.

SOME RECENT DISCOVERIES IN REFERENCE TO PHYLLOXERA. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

EVERY new fact in the life-history of the insects of this genus has an exceptional interest, because of its bearing on the destructive grapevine Phylloxera. The genus is most largely represented in this country by a number of gall-making species on our different hickories, and the full annual life-cycle of none of them has hitherto been traced. The galls are produced, for the most part, in early spring; the winged females issue therefrom in early summer; thenceforth, for the remainder of the year, the whereabouts of the insect has been a mystery. The author has for several years endeavored to solve this mystery and at last the stem-mother (the founder of the gall), the winged agamic females (issue of the stem-mother), the eggs (of two sizes) from these winged females, the sexed individuals from these eggs, and the single impregnated egg from the true female, have been traced in several species. There is some evidence, though not yet absolutely conclusive, that this impregnated egg hatches exceptionally the same season; also, of a summer root-inhabiting life. In *Phylloxera spinosa*, which forms a large roseate somewhat spinose gall on *Carya alba*, and which has been most closely studied, the impregnated egg is laid in all sorts of crevices upon the twigs and bark and in the old galls, in which last case they fall to the ground.

Up to this time they have remained unhatched, and will in all probability not hatch till next spring, thus corresponding to the "winter egg" of the grape Phylloxera.

ENTOMOLOGICAL NOTES FOR 1883. By E. W. CLAYPOLE of Akron, Ohio.

[ABSTRACT.]

THE POTATO BEETLE.—This insect, in 1882, came as usual in Middle Pennsylvania in the early summer. I was compelled to use poison as in previous years. In the later portion of the summer I observed and noted at the time, in the Canadian Entomologist, that there was no second brood or that it was so small as to pass unnoticed. It was my intention to watch in 1883 in order to determine if this second brood was again missing. But to my surprise in 1883 there was almost no first brood. The pest was gone. Here and there a few beetles appeared, or later a few brood of young were observed, but they were so few that by most persons they passed unnoticed. All the season the plants grew and flourished uninjured by their enemy.

Being called away on business to New York in May, I observed the same fact over the whole intervening country. Everywhere the potato beetle was conspicuous by its absence. In every part of Pennsylvania I find the same result, at least as far as I have enquired, and letters from my former residence in Ohio inform me that there too the plants are free from their enemy.

I know not to what cause this freedom is to be ascribed, but note the fact, for want of time has prevented my making any investigation. But whatever it is, I am disposed to date its action in the middle or early part of last summer, and not during the winter as popular opinion inclines to believe.

THE HESSIAN FLY.—During the present season, 1883, the Hessian fly has been exceedingly abundant in some parts of Perry Co., Pa. I saw many fields in which the crop would certainly not pay the cost of cutting it. I have found four, five, six and seven "flaxseeds" in a single straw. This of itself is not deserving of special remark. But on enquiry I was told by farmers that it was not as is usually the case the earliest wheat that was infested. Several assured me that in their own and their neighbors' crops, the earliest and latest wheat was free, and the rest of the crop was attacked. By "earliest" I mean wheat sown between Sept. 1 and Sept. 7, and by "latest" that which was sown after Sept. 22. I do not know if this fact has been generally observed, but I am satisfied the statement is true for Perry County.

In explanation, I can only suggest that probably the earliest wheat does not escape the fly, but being large and strong, is better able to bear it, and though suffering injury yet sprouts and repairs its loss before the spring growth comes on. The same reason — stronger growth — may also enable it to resist the spring attack.

PSEPHENUS LECONTEI; THE EXTERNAL ANATOMY OF THE LARVA. By D. S. KELLICOTT of Buffalo, N. Y.

[ABSTRACT.]

THE species referred to is found in large numbers at the rapids above the falls of Niagara, and is scattered throughout the northeastern part of North America. The author proposed to supplement the accounts given of it by earlier observers with a record of his own observations, which differed in some respects from those of Dr. LeConte. Several details of anatomical structure were brought to the attention of the members, and illustrated with wood-cuts prepared for the purpose and with specimens mounted in balsam for observations under the microscope.

NOTE ON PHYTOPTIDÆ. By HERBERT OSBORN of Ames, Iowa.

[ABSTRACT.]

THE Phytoptidæ comprise a group of very minute mites, species of which produce galls of various forms on the leaves or twigs of various trees. Recent investigation in Europe has placed the group in a different light from that in which it previously was considered. Their study is rendered difficult by their extreme minuteness, and the care necessary to discover the different stages. One of the most common species produces the little wart-like swellings which occur so abundantly on soft maple leaves. A species on ash leaves produces a swelling which is nearly uniform on the upper and under surfaces of the leaf; while another species on the same tree produces a leafy growth at the end of the twigs, the growth sometimes being inhabited also by cecidomyian larvæ. On the elm occurs a large deformed leafy growth, which also contains Phytopti; while still another form of gall occurs on box elder, consisting of a depression on the under surface of the leaf, this depression being filled with a woolly growth, and containing Phytopti.

FOSSIL CEPHALOPODA IN THE MUSEUM OF COMPARATIVE ZOOLOGY.[1]

By ALPHEUS HYATT of Cambridge, Mass.

THE nature of the evidence afforded by fossil shells is very little understood. They have been so often spoken of slightingly, as a sort of jacket, an unimportant part, etc., that all conclusions arrived at by their study alone are considered as peculiarly liable to error.

A shell to begin with ranks as a primary, essential part, and from its mode of growth it is a model of the external form, and at the same time a mould of the outlines of the internal soft parts to an extent which has not been fully appreciated. The shell may often become a permanent record of the series of changes, which the form has undergone, from the time it first began to inclose the embryo until the death of the soft parts. Among Nautiloids and Ammonoids, it contains another part, also calcareous, the tube, or so-called siphon, which exhibits remarkable and significant changes of structure and position in the early stages. This connects the septa or horizontal partitions, which with their sutures vary with the age of the animal constituting a third record of early changes and structural modifications.

All these parts are found to be in correlation with each other, and together make, when properly translated, an index to the life history of the individual, which is unequalled in some respects among other existing or extinct animals.

A single shell, either from a living or fossil form, may present, accurately, the general history of the development of the young, the stages of the adult and of old age. The results of heredity and the action of endemic or traumatic diseases can be detected, if one knows how to study and compare the remarkable and distinct series of metamorphoses often displayed by this external or protective skeleton with those of congeneric forms.

The vertebrate skeleton has long been considered a standard, and the evidence afforded by its fossil remains is very important and convincing. The series made in the case of the horses found by Marsh and those described by Gaudry are universally quoted as the strongest proofs of evolution. This evidence is considered complete, because naturalists understand and have thoroughly studied the skeleton, and because it is internal and has been assumed to be more invariable than the shell. All of these arguments have their due weight, but we should like to hear of an example of greater invariability than exists between the shells of the Nautilus now existing and those Barrandeoceras (Nautilus) of the Cambrian, or of the Triassic and Cambrian Orthoceras.

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[1] Partial abstract from a memoir now in preparation, of which two other, but distinct abstracts containing different portions of the memoir have appeared since this was read. One, "Genera of Fossil Cephalopods" was published in Proc. Bost. Soc. Nat. Hist., V, 22, Dec., 1883 to Feb. 1884, and one "Evolution of Cephalopoda," in Science, Camb., Feb., 1884, nos. 52–53, V, 3.

The conclusions arrived at by the study of the skeleton are reliable, but they really are no more conclusive in theoretical meaning than any other case of a series of equally well understood hard parts in any other branch of the animal kingdom found as fossils, and traced out in the same careful and thorough manner.

How unreasonable it would seem to a student of fossil mammalia, if he were requested to do what it would be appropriate to require from a student of the fossil Cephalopoda, viz., to describe from the investigation of the single perfect fossil skeleton of an adult, not only the characteristics of the skeleton at the stage of growth in which the animal died, but the developmental stages of this same skeleton, and in case it were the remains of an old, outgrown animal, also, the retrograde metamorphoses through which it had passed during its last stages of decline.

It might require a lifetime to make out the stages of a single species of mammal satisfactorily from the isolated specimens which could be found, and the attempt would be hopeless for all of the youngest periods of growth, while the bones were still cartilaginous.

Crustacea, especially Trilobites, afford an opportunity of doing this, as Barrande has shown, but the labor expended in gathering the necessary number of specimens demands the most extensive and persistent efforts, and one can rarely be perfectly sure that the same species is under observation all the time, since it is impossible, as among the Ammonites and Nautili and living animals, to follow out the same individual through all its transformations.

This kind of evidence, however, is as readily obtainable among fossil Cephalopods with relation to the shell and other hard parts as among living animals, and it can be obtained in good collections everywhere, whether "in situ" or in Museums. Thus it is possible to study the relations of these fossil forms very minutely and with a certainty of possessing a clew to their true relations, which is rarely attainable even among existing animals, for among these we have only the embryos and young of contemporaneous forms and necessarily lose all relations of succession in time, unless the investigation embraces a prolonged series of experiments or is more or less historical, and even then the facts cannot have a very wide chronological range.

The accepted divisions of the Cephalopods have been founded by authors wholly upon the characteristics of the adult. The form of the shell, whether straight as in Orthoceras, curved as in Cyrtoceras, coiled up with open whorls as in Gyroceras, or with the whorls in contact as in Nautilus. These modifications together with the outlines of the aperture and other minor characteristics, have, heretofore, determined the group to which any given shell was referred.

The examination of the young of all the closely coiled Nautiloidea shows them to be as a rule uncoiled, and in the earliest stages simply arcuate as in the adults of the group of the Cyrtoceratites, and having a scar on the apex which represents the beginning of the stage in which the animal commences to construct the true or secondary shell. The young of all the Ammonoids have, on the contrary, with the marked exception of some Palæozoic species and some *varieties of species*, closely coiled whorls at the corresponding stage of growth, and upon the apex is a tiny bag or embryo shell, which has been very appropriately called the protoconch by Owen.

In my Embryology of Cephalopods, Bulletin of Museum of Comparative Zoology, Cambridge, No. 5, Vol. III, the position was taken that the scar of the Nautiloidea showed that a protoconch had existed in the embryo of Nautilus, but had disappeared during the growth of the shell, the scar being uncovered by its removal. This supposition was endorsed by Prof. Richard Owen, but rejected by Barrande, who insisted that the general absence of a protoconch was a fatal objection. There exists, however, on the apex of some Orthoceratites, an excrescence or bulb of a withered and somewhat irregular and variable shape. DeKoninck, in his late magnificent work upon "Calcaire Carbonifere de Belgique," curiously enough cites these very forms as the principal proofs against my conclusion because in them the scar is absent. According to my examinations and drawings, however, the scar ought to be found in such examples underneath the bulb of the apparently complete apex, which is simply the withered and shrunken remains of the primitive protoconch. This was evidently originally a soft, embryonic shell composed of conchiolin and not of calcareous matter as in the Ammonoidea. I have seen and figured several examples in which a bulb was present on the apex and no scar visible, and one case in which the bulb (protoconch) had evidently been broken away leaving the scar visible below surrounded by the broken edges of the outermost shell layer which formerly connected the apex with and covered the protoconch. The external layer of shell and its longitudinal ridges from the apex up on to the so-called plug of the cicatrix described by Barrande, have also been traced and thus every point in the evidence appears to be complete, and the fact that the bulb is covered by a true protoconch continuous with the shell of the apex seems to be established.

Space is wanting to discuss the question of the convergence of the orders of Cephalopoda, but some of the facts will be given farther on. M. Barrande's strenuous and magnificent opposition to the view that these orders did converge in the Palæozoic rested largely upon the supposed differences of the embryo of Nautilus and Ammonites, and he figured several of the species, which carried the bulb or protoconch and described them as having the apex plugged or filled as in truncated shells. The figures referred to are in Barrande's Syst. Sil. de la Bohême, pl. 488, case 9, 11, *O. elegans* and *O. politum*, and the fact that the external shell is continuous may[2] be seen in specimens of *O. crotalum* in Mus. Geol. Surv., Albany.

The classification adopted is as follows:

CLASS CEPHALOPODA.

SUB-CLASS I, TETRABRANCHIATA.

Order 1, *Nautiloidea.*
" 2, *Ammonoidea.*

SUB-CLASS II, DIBRANCHIATA.

Order 3, *Belemnoidea.*
" 4, *Sepioidea.*

These four orders converge to one type by intermediate forms, by the embryology and development of the shells and internal hard parts, by their morphology, and by their habits which may be inferred from the forms of the apertures and build of the shell. Judging from these facts, the prototype of the Mollusca must have had a globose embryo attached to the apex, the apex composed of a living chamber opening into the protoconch or globose shell of the embryo without septa, though possibly divided more or less by diaphragms. Diaphragms precede the formation of the septa in the embryo Ammonoid and a cone formed by them appended to the bottom of the cœcum was figured in the author's Embryology of Cephalopoda, Bull. M. C. Z., and described by M. Chalmas as the prosiphon.[3] This confirms Von Jhering's opinion that Tentaculites was the prototype of the Cephalopoda, since it has similar embryo and diaphragms. The prototype of the sub-classes I and II must have been a form of the same type, with however a single septum or series of septa having closed cœca in place of a siphon. These cœca were the initial stages of the funnels, parts of the thick, hard septa, and continuous with them and not the true siphon or in any way *inclosures at first* of the fleshy walls of the true siphon; they were directly in contact with the outer side of the apex of the mantle and strictly homologous with the internal partitions found in many Gasteropoda. The cœca, becoming prolonged in descendant forms, were differentiated into the funnels, the remnants of the cœca and the thinner walls of the sheath proper, connecting them were formed by the fleshy siphon. This is the condition of the siphon in the typical forms and in tubular siphoned Orthoceras, but in some aberrant genera the fleshy siphon widens near the living chamber becoming conical and forming a sheath. These sheaths lie in the large tube formed by the true funnels and may deposit permanent diaphragms as in Endoceras. The fleshy sheaths formed also a median tube connecting the apices of the sheaths and surrounded by a layer of shell which is shown in Piloceras in a structure first demonstrated by Dr. Dawson.

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[2] Since this paper was read figures illustrating some of these facts have been published in Science, Cambridge, Mass., V, 3, No. 52, Feb. 1884.

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[3] These diaphragms, as figured by the author in the work cited pl. 1, f. 3, 4, pl. 2, f. 1, y, pl. 3. f. 3, 5–6, are apparently quite distinct from Meunier Chalmas' figures of the prosiphon which as drawn in Spirula and Ammonites in Fischer's "Manuel de Conchyliologie," 1881, pp. 336, 371, seems to be a distinct connective prolongation from the bottom of the cœcum across the vertical section of the protoconch. This prosiphon of Chalmas was not observed either by Branco or the author, though the former sought it in Spirula and many forms of Ammonitinæ. These facts suggest that the diaphragms which preceded the formation of the cœcum were variable in form and they are apparently the homologues of the calcareous plate which closes the cicatrix in Nautilus. If this be true they lose the value attributed to them by Chalmas and Fischer and do not indicate any close affinity between Spirula and the Ammonoidea.

This, the endosiphon, had the same thin covering as the sheaths themselves or secondary diaphragms, and is but rarely well preserved. The sheaths were only temporarily formed and resorbed in some genera, Sannionites, better known as Cameroceras, and in Piloceras as suggested by Dawson.

The true Endoceras, *E. proteiforme* Hall, has numerous successive sheaths and the endosiphon in its most aberrant condition, and also the siphon composed only of the funnels. Sannionites and Piloceras have, as a rule, but one perfect sheath, this being appended to the living chamber; any others are apt to be imperfect and the remnants attached to the endosiphon, as in Dawson's figure of Piloceras, Can. Nat., V, X, No. 1, p. 3, f. 2.

It is interesting to note that all of these types have the aspect of having been indigenous. They are so numerous in all the lower formations, Calciferous and so-called Quebec, and in the corresponding rocks in northern Europe that they almost exclude the types having small siphons, thus affording the strongest contrast with the Bohemian faunas, and presenting a more ancient aspect than any of these, a fact already stated by Barrande. The endosiphonate and transition types of these periods have a common character, already noted by Billings in various specific descriptions and evidently understood by that author, though not as strongly insisted upon as its importance deserved. All of them, including also the coiled types, have the septa very closely set or what would be designated as crowded together, and the siphon central or between the centre and the ventrum, with the exception of a very few species. Individuals increase the distance between the septa by growth up to the adult stage, and then in old age the distance again decreases. This peculiarity is, therefore, important and characterizes the radical types existing in the more ancient formations as well as the young of the Nautiloids in general. It is also to be noted that the young of the Nautilus has the siphon larger than in subsequent stages. While this more ancient aspect of the faunas in North America is true of the formations antedating the Chazy and Trenton, and perhaps including the Lower Silurian or Cambrian as a whole, it is not true of subsequent formations or other types in these later periods. The Devonian Cephalopoda, as a rule, have the aspect of and belong largely to our Spyroceran group and are similar to the Bohemian Silurian Nautiloidea of the same series. The Goniatites of America have also the aspect of migrants and the radical Silurian forms abundant in Europe are wanting. The group of Clymninæ are also absent.[4]

M. Barrande has shown that all the different types of tubular-siphoned straight cones may be followed by means of transitional forms into the large-siphoned, or Endoceratitic and Actinoceratitic genera, and in other directions this tubular siphoned group may be followed into the nummuloidal-siphoned groups of straight forms. All of these have been divided into genera, which are described in the twenty-second volume of Proc. Bost. Soc. Nat. Hist., but these do not interest us here. It is necessary merely to restate M. Barrande's facts, and also to note the central position of the tubular-siphoned or typical straight cones. They connect, as proved by Barrande, in other directions, notably with the brevicone forms. From these last any one may trace the series, which leads through the brevicone Cyrtoceratites, described by Barrande, into the Gomphoceratidæ, in which the lower forms, abundant in this country, have open apertures and similar siphons. The next members of this series are the so-called Phragmoceratites and others with much contracted apertures. Gomphoceras also connects with the truncated Mesosceras of Barrande, which leads into the Ascoceratites.

A good illustration of this method of classification is shown by the frilled shells, which are a singularly homogeneous group, and about which the evidence is quite complete. The longicone straight shells have a genus Dawsonoceras composed of annulated cones which have a distinct frilled layer on the exterior. These frills at intervals stand out from the sides showing the marks of having been at one time the edges of apertures.

The *Cyrt. lamellosum* Hall, Amer. Mus., is the type of the arcuate group of this series in the Silurian, and then follows a large series of gyroceran Devonian forms leading into nautilian shells, also in the Devonian, Mus. Geol. Surv., Albany, of which the type is the *Gyr. undulatum* Hall.

A parallel series in the same family group begins in the Devonian, having the same style of shell, with the *Cyrt. Jason* Hall, and may be followed into gyroceran forms. In this series we have enjoyed the criticism and assistance of Prof. James Hall, who has stated somewhat similar facts in his Natural History of New York, with the exception of the connection between *Cyrt.*

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[4] Have recently seen a specimen from Burlington, Vt., that may prove to belong to this group.

lamellosum and the Orthoceratites, which we worked out when away from Albany.

Another fine series begins with *Orth. Archiaci* Barr., and can be traced by the forms, sutures, and position of siphon through *Lit. magnificum* Bill., of the Silurian, and then into a true nautilian shell, the *Naut. ponderosum* White, and into *Naut. bilobatum* Sow., and *Naut. ferratum* Owen, both in the Carboniferous. The dorsal lobe is introduced in *Naut. ponderosum* and in the succeeding forms it becomes divided by the reappearance of a small, median dorsal saddle.

In these, and many other instances, the evidence has been accumulated which enables us to show that the genera, Orthoceras, Cyrtoceras, Gyroceras and Nautilus, as those terms are now applied, are in reality generalized descriptive terms for representative forms of different series, which resemble each other in form, but are distinct structurally. Each series can be distinguished by its own differential, structural characteristics and by the form of the whorl in section, but each one springs from some straight or arcuate form, and passes through a parallel series of transformations which can be designated by the terms cyrtoceran, gyroceran and nautilian. By the examination of these series we have learned to distinguish accurately the nautilian forms, as those having an impressed dorsal zone due to close coiling, and in this impressed zone a dorsal lobe is developed sooner or later in the sutures. This distinction enabled us to point out the extreme rarity of true nautilian forms in the Silurian. Most of those, thus named, though closely coiled, have a gibbous dorsum and dorsal saddles in the sutures and are true Gyroceratites; for example, the major number of the species described by Barrande as Nautilus, and many American species, come into the gyroceran category.

Some arcuate forms have a very slight dorsal inflection, and flattened or slightly impressed dorsum. These do not contradict but confirm our evidence, and have been fully considered. They are either degraded descendants of true nautilian shells which have become uncoiled and show their descent in their closely coiled young, or else the dorsal lobe can be traced directly to the peculiar conformation and the excessive arcuation of the whorl.

If we turn from the series of adults to the study of the stages of growth in individuals, we find everywhere a similar repetition of the arcuate, gyroceran and nautilian forms. The first stage in any nautilian shell is a Cyrtoceras, the next is Gyroceras, and the third Nautilus; and almost equally invariably the development of the sutures begins with a dorsal saddle, and is followed by a dorsal (internal) lobe after the impressed zone is formed.

After the expiration of the Palæozoic, shells with large open umbilici disappear as stated by Barrande, and this statement, as we have previously argued in our Embryology of Cephalopods, denotes the decreased importance of the arcuate and gyroceran stages in the young. This opening in the umbilicus we have called the umbilical perforation, and shown that it was an important distinction. It is found in all nautiloids and not in the ammonoids except certain ancient larval forms. We know that the straight and arcuate cones and gyroceran forms have also in large part become extinct, or are dying out at the time of the Trias, so that both the parent forms and the indications of immediate descent from them, as shown in the development of individuals, disappear simultaneously.

The evidence, therefore, derived from the study of development in the individual, teaches that the Mesozoic forms are derived from shells which have been in themselves close coiled or nautilian forms. This is carried out fully by the facts of gradation as they subsequently occur in time. Thus the typical Nautilidæ of the Trias, like *Cenoceras* (*Naut.*) *carolinus* Mojsis., are directly traceable into the striated and smooth shelled truncated forms of the same period and of the Jura, like *Naut. truncatum* Sow., and we trace the latter by intermediate shells into the Radiati group of the Cretaceous. The genus Nautilus did not begin to exist until the Jura and all the supposed similarities, which have led to the suppositions of M. Barrande and others, that it is a genus closely allied with silurian nautilian shells, are errors arising from incomplete knowledge of the structure and development. Nowhere in this series is there a sign of immediate derivation from a cyrtoceran or gyroceran ancestor. The approximations of the adults are close and undeniable, and in the development the arcuate stage is reduced to smaller proportions and occupies less time in the development than in the preceding Palæozoic series. Exception must be made in favor of those forms in the Palæozoic, which crown series, and which by descent have gradually acquired small umbilical perforations ; but these are by no means numerous and are serviceable to our hypothesis, since they stand out prominently as exceptions to the common run of palæozoic forms. A curious confirmation of this conclusion was found by the author in studying the development in time of the inner or dorsal lobe of the sutures. This lobe is, as we have said at first, in most Silurian Cyrtoceratites and Gyroceratites, and in the young of the nautilian forms preceded by a saddle or forward inflection of the dorsal sutures.

In Barrande's Nautili of the Silurian there are some with dorsal lobe, and in our American species it also occurs in company with the impressed zone, but in none of these, though no doubt it may occur in very rare examples, have we found the slightest indication of the small median, dorsal lobe, which we have called the annular lobe. In the true Nautilidæ of the Devonian, like *Nephriticeras* (*Naut.*) *bucinum* Hall, etc., the dorsal lobe acquires this addition in the shape of a median depression deepening the original shallow outline into a V-shape. In the Triassic and Jurassic forms the edge of the V-shaped lobe rises anteriorly and has a distinct, internal conical depression, the cone of the dorsal part of the septum. In the Radiati the adult never has the cone, but may have the lobe in the sutures. Nautilus, in the adolescent stage, has the cone and the sutural V-shaped lobe which I have called the annular lobe, but the cone is lost by growth, and the annular lobe alone is retained in the adult. In the Jura some adults may retain the cone, but most lose it as in Nautilus. In the Triassic forms all seem to have the cone until a late stage, and some certainly in the adults. The annular lobe also changes its form in the Jurassic species from V-shaped as in the Palæozoic to U-shaped as in modern Nautilus.

In other series of shells with distinct origins which may be traced in some series even more accurately and satisfactorily than in the above, a similar succession of phenomena has been observed. Nautilian shells in the palæozoic, until the close of the carboniferous, are generally traceable to cyrtoceran forms having similar characteristics of form, position of siphon, sutures, and aspect of surface; and by inference from these to Orthoceratites of different types.

The picture which this presents is somewhat startling at first, but we feel sure that after an examination of the facts naturalists will at any rate be disposed to respect our conclusion, that types among Cephalopoda, and in other branches of the animal kingdom, became more quickly changed in the earlier stages of their evolution than subsequently, and that this quicker evolution gave the aspect of sudden creations to these earlier forms as they appeared in the palæozoic and a distinct character to the palæozoic age as compared with subsequent ages. For example, the highest type of Nautiloidea is Subclymenia, a species with ventral V-shaped lobe and ventral siphon, so like Goniatites, that it can be separated only by its young and the gradations of forms which connect it with certain well-known Nautiloids. This springs up suddenly a species by itself in the Carboniferous, in a group which, altogether, are only of family importance and are directly traceable to arcuate forms. Many cases like this could be cited, notably M. Barrande's *Celœceras* (*Gon.*) *prœmaturum*, which is a highly complicated goniatite appearing before its proper time in an evolutionary sense, and cited by M. Barrande as a significant fact against any hypothesis of progression in time. An allied species has the adolescent stages like the adults of the Nautilini until nearly full grown, and then the sutures become suddenly complicated. This is the well known *Gon. hybridus* Munst.[5]

If any of these types occurred without their congeners in the earliest Cambrian we should have a similar picture to that presented by the Ammonoidea of these formations. These in a few cases present young which have the straight orthoceratitic character; these few cases are purely varietal and in the same species, as shown by M. Barrande, we may have the young with a straight apex or closely coiled. Sandberger and the author have added to this, instances of varieties of other species which are loosely coiled or gyroceran in form in the Devonian and in the Carboniferous, but so rare that Branco was unable to find a case, though he examined many embryos in this period. After the Carboniferous, as shown by Branco and the author, all Ammonoids are close coiled (unless it is found that the extreme degraded form of Baculites in some cases becomes straightened out again, a not improbable anticipation).

The evidence of the young of *Agoniatites* (*Gon.*) *fecundus* Barr., shows therefore, that the Ammonoidea had a pre-palæozoic history of quick development from straight Orthoceratitic forms similar to that of the Subclymenia and some other forms of Nautiloids. This like the *Centroceras* (*Discites*) *Marcellense* of Hall had ventral lobes and ventral siphons which have led many naturalists into the mistake of considering them as prototypes of Goniatites or else true Goniatites. In the Cambrian, the Ammonoidea had already attained a stage of evolution which was not reached by the Nautiloidea until after the expiration of the Palæozoic.

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[5] We are informed by Prof. Zittel that Munster's figure of this species is erroneous and that this comparison cannot be made.

There are sufficient remnants of the earlier transition forms left in the Silurian to demonstrate the former connection of the Ammonoids and the Nautiloids. There is one group of slender whorled forms like *Bactrites* (*Orth.*) *pleurotomum* Barr., Syst. Sil., pl. 296, and others on pl. 297, which by their striæ of growth, their long funnels and the positions of the siphon which varies from the centre to near the ventrum, show close affinities with typical Bactrites. This last has sutures which in some cases are deeply arcuate and have both dorsal and ventral saddles, with laterally compressed elliptical whorls. This same form is repeated in the coiled whorls of *Mimoceras* (*Gon.*) *ambigena* Barr., and *compressus* Beyr., in both of which also the whorls, though often touching, are truly gyroceran. They have no impressed dorsal zone, and the sutures are precisely similar to the arcuate sutures of Bactrites. Mimoceras is not in any sense a true Ammonoid, except in the embryo, which has the protoconch as in that order. It is evident, therefore, that these Goniatites were not derived from Bactrites in the Silurian. The same form of protoconch may be found in Bactrites, but, so far as known, Bactrites is a true Nautiloid in its earliest stage and sheds the protoconch. The facts then are only sufficient to identify them as the remnants of transitional series which were once united in some slender form of tubular siphoned straight cones existing before the Cambrian.

The Goniatites mentioned lead directly into the true Nautilian forms of the Nautilinidæ whose transitions into the true Goniatites of the Devonian are by insensible gradations. Interesting as these are, we have no space for their consideration; we have given them in the Proc. Bost. Soc. Nat. Hist., v, 22, p. 303. This evidence is, however, sufficient to justify us in attributing the origin of the Goniatites to a series which began with a straight form and passed through in its evolution the arcuate, gyroceran and finally arrived at the nautilian stage. The Ammonoidea in the Cambrian are like the Nautiloidea in the Trias after the expiration of the Palæozoic and like them possess, with the exception of the few forms mentioned, exclusively nautilian shells. These shells also as we have stated being very close coiled with an impressed dorsum and dorsal lobe and possessing a slight depression on either side of the protoconch which is a remnant of the umbilical perforation. In this respect they are even more highly differentiated than the Nautiloidea of the Trias. The annular lobe also appears, first only in *Agoniatites tabuloides* sp. Barr. of the Silurian and becomes general only in Devonian forms. Thus the type of the Ammonoidea shows decisive gradations in differentiation of important structural characteristics and forms a single series parallel to one of the series of the Nautiloidea and therefore must have evolved with great comparative rapidity.

Instead of many series springing off on all sides from a common type as do the families of the Nautiloidea from Orthoceras, there seems to have been but one evolving directly from Orthoceras which develops similar forms and structures, but these becoming at once the radicals of still higher differentiations without passing through the preparatory stages which endure throughout the Palæozoic among Nautiloids. Pathological and uncoiled forms occur in limited localities in later formations up to the end of the Jura and then the whole type retrogrades. As shown by Neumayer the normal forms have simpler sutures and the uncoiled types also become more numerous and general and the whole type ceases with the termination of the Cretaceous.

During the Palæozoic, the Ammonoidea present an intermediate character in their mode of evolution. The types produced all have nautilian shells, but the genera are distinguished from each other by essential differences of the internal organs and sutures, and some groups like the Clymeninæ present wide departures from the normal outlines of the sutures, and the siphon itself in the last is displaced, being near the dorsum instead of ventral, as in all other Ammonoids. That Clymenia is an Ammonoid is now unquestionable, Branco having shown that the young were ammonoidal, and the author having confirmed this easily verifiable fact.

In the Trias such essential and wide structural differences begin to disappear and in the Jurassic we are presented with an almost homogeneous group which naturalists, until a very late date, have insisted upon including in one genus. This error is now, however, being widely corrected, but the differences of generic importance are not such decisive structural distinctions as are observed between genera of Palæozoic Goniatites. The sutures are much more homogeneous in Jurassic forms, and we have to rely to a great extent upon the ornamentation of the whorls and the less important differences shown by the digitations of the sutures, etc.

Both the Belemnoidea and Sepioidea confirm these conclusions, the facts pointing in each of these to an independent derivation from distinct types of Orthoceratites, the former from the true Orthoceras, and the latter from the flattened forms of which Gonioceras is the nearest in affinity now known.

The truncated shells of Orthoceras have been described by Barrande; and the plug which fills up and rounds off the broken apex, accounted for as an external product due probably to the action of two arms which he supposed stretched out posteriorly from the aperture as in Argonauta and embracing the whole shell and extending even beyond the broken end filled it up by successive deposits of carbonate of lime. We have also studied this structure and can confirm M. Barrande's observation, and the conclusion that the plug is an external product with a peculiar bilateral structure, but that it was deposited by two arms extending backwards we do not regard as probable. The markings indicate that at the truncated apex the layers were laid on by an organ which was certainly double at the end, but not necessarily double all the way up to the aperture. The body of the whorl in Orthoceras does not have any prolongations of the ventral and dorsal furrows of the plug, nor are there any longitudinal rows of spines or furrows, as in Argonauta, indicating the presence of two arms, capable of secreting shells, nor does the aperture with its entire outline give strong support to this proposition.

We think it is possible to strike nearer home in homology; the fold of the mantle in Nautilus satisfies the conditions. This is an active shell-secreting organ, which was certainly present and also functionally active in the Ammonoidea and Nautiloids, and probably more important in these ancient forms than it is now in the modern Nautilus. This is also more consistent with the explanation of the structure of the Belemnoid, which, as is easily seen in the famous examples of the preserved animal, had no such pair of enlarged arms, and yet deposited externally a solid covering, the guard, which is in our opinion the homologue of the solid filling of the truncated end of Orthoceras. Branco in his admirable paper on the structure of the curious Triassic Belemnoid, Aulacoceras, shows that the secreting organ of the guard must have been open and proves it by his drawings of masses of foreign matter included in the layers of the guard. This and the channel which runs along the ventral side in the guard and is often single, and generally more persistent and longer than the dorsal channel favor this view of the homologies. Quenstedt and Von Jhering have both traced the Belemnoids to Orthoceras, and this opinion supports their views and accounts for the preservation of the protoconch, both as a useful organ containing the siphonal cœcum in its neck and because of the protective guard built up around it at an early stage of growth. The tracing of Belemnoidea back to Orthoceras accounts for the dorsal and ventral channels, both of which are present in the plug of the truncated Orthoceras. It also permits us to explain the central hollow trace or tube in the guard of the Belemnoid, as the homologue of a similar hollow trace or tube, the pseudo siphuncle which invariably occupies the centre of the plug in truncated Orthoceratites, and has not been heretofore accounted for. The Belemnoids are nearer to the Nautiloids in the Trias than any of the later periods, since Aulacoceras is in some measure an intermediate type with a less perfect guard than that of the typical Belemnoids so abundant in later formations. The Sepioids are usually joined with the Belemnoids, as a closely approximated type, and this conclusion is carried out in a very general way by geological evidences. There is a well defined palæozoic group of Orthoceratites, in which the transverse section of the whorl is fusiform, Eudoceras of Hall; these grade into Gonioceras, a flattened shell with wide acute, lateral expansions like those of the shell in fossil Sepioids. One example in Professor Hall's collection is so remarkably similar to the dorsum of the common Sepia shell in shape and lines of growth, that it would have certainly misled the author, if the dorsal aspect alone had been visible. The sutures luckily prevented this mistake, but even they have a peculiar curvature not found in any type of Nautiloid, and yet agreeing remotely with the equally peculiar curvature of the layers of the internal filling, of calcareous, spongy matter, which is a notable characteristic of the Sepia shell. If these are transformed homologues of the septa, and we think they are, there is reason for associating the Sepioidea and Goniceratidæ, just as we above associated the truncated Orthoceratites and the Belemnoidea, though the series is by no means as perfect.

The Sepioidea are, however, a more highly concentrated type rapidly parting with the adult ancestral characteristics, and also the protoconch; reducing the shell on the ventrum to a narrow band and altering the septa to a series of continuous deposits, and finally in the existing Loliginæ, fusing the primitive shell-gland and shell-sac, and in the Octopods the shell finally altogether disappears.

Whatever is the fate of this opinion, the Sepioids do not directly approximate to the Belemnoids in past times, but are distinct and originated from the Orthoceratites. The absence of the external shell or protoconch in the young of the existing Sepioids and Octopods and the generation and final destruction first of the septa and then of the shell can be accounted for as due to their great differences of habitat and to the action of the law of concentration in their development.

This position is also supported by Lankester's observations on the embryology of the Sepioidea, in which the pen sack of the mantle in Loligo is accounted for as a secondary formation and not the primitive shell-gland, but is nevertheless supposed by him to have become confluent with the true shell-gland. The remarkable embryological insight which prompted his view and led him to refer to the sac as probably due to the transformations occurring in the Belemnite, is exactly fulfilled by Aulacoceras, and sustains this hypothesis. Thus when we meet with an internal shell in the animals of the group Sepioidea, we must imagine the shell as still enclosed in the hood, although the edges of the once opened sac formed by the hood have become permanently united. Fusion with the primitive shell-gland would follow naturally the inclusion of the shell of Aulacoceras and Belemnites in a flap or hood-like prolongation of the mantle forming a pen-sac. The union of this sac with the primitive shell-gland is a natural sequence, which follows from the law of concentration in development as soon as the inheritance of the shell-sac and its tendency to close became fixed in the adult structure and inheritable by the embryo of Sepioidea.

If this be true, all the types of the Cephalopoda probably originated in the Palæozoic, and our conclusions accord with those of Barrande among various types of invertebrates, and other authors, notably Alexander Agassiz among Echinoderms. In fact the vertebrata alone present several distinct types, possibly of Mesozoic origin, and these are by no means certainly of that date in all cases, and there are only comparatively few types of the invertebrata which cannot be easily proved to have originated in or before that age. The tendency of modern discovery has been to carry back the origin of types to more and more ancient dates. Thus we find Cope claiming among his remarkable Dyassic (Permian) reptiles that the Theromorpha are not only the radicals of the Reptilia but also combine characteristics of the Batrachia and Mammalia, especially the Monotremata, and finally that the vertebræ are amphicœlian and distinctly notochordal.

The palæozoic, therefore, presents itself to us as essentially the birthplace of types, and at the same time as the age during which great structural differences originated with a rapidity equalled but rarely in later times.

The pre-palæozoic is unknown, but the inference holds for this age also, and there is great probability, that in this period the evolution of forms was proportionally quicker. This view is not affected by the theory of the alternation of continents in order to account for the accumulation of sediments, and provide homes for the evolution of types before they make their appearance in the known formations of this or other continents. It is simply as we have tried to show an inference drawn from the positive data of the series studied, and consequently applies to the phenomena of evolution in every location.

In the Cambrian, as is now well known through the labors of several palæontologists, the advent of types is sudden, and they are in themselves numerous, but in our experience the same law holds for succeeding periods. If we select the formations in which forms appear, we usually find them spreading out with wonderful rapidity, sometimes without any apparent precursors, and sometimes with radical forms immediately preceding their appearance. Our impression derived from the minuter study of series among Cephalopoda is not that the evolution of groups was slow, but on the contrary excessively rapid, and that the groups, if presented graphically, would form fan-shaped figures instead of long branches.

A notable and well known example is the descent of the larger number of the Liassic Ammonites, the Arietidæ from *Psiloceras planorbis*, a fact established by the observations of Quenstedt and confirmed by the subsequent experience of several authors and the special investigations of the writer. The family radical is here one species closely approximate in time to the family itself, which is composed of several distinct genera and different series of more complicated forms. These must have spread out with great rapidity in the successive faunas of the Lower Lias. The same is true of the larger number of all groups.

The inference, therefore, is, that types near their point of origin are more plastic and more easily changed by the forces acting upon their generalized structures.

If we adopt the hypothesis that habits forced upon the animal by the habitat are the principal means of modification, and lay aside the teleological notion that every change must in some sense be an advantageous difference requiring long periods of time for the action of natural selection, the law of battle and the survival of the strongest, etc., we can understand the origin of the orders of Cephalopoda and the curious phenomenon of the quick evolution of their successive forms.

Thus the Tetrabranchiata are crawlers and leapers, and but rarely swimmers. Their shells show this in the forms of the apertures and outlines of their whorls. The ventral sinus in the aperture, when large, indicates that the animal possessed a well-developed ambulatory organ, the so-called fleshy siphon or funnel of recent Nautilus and of the Sepioidea. When this is moderately developed and the aperture open, we can be quite sure that the animal was furnished with powerful arms for supporting the shell, and was a crawler mainly. Such were the straight shells without exception, and most of the Cyrtoceran forms, the young of the Nautiloids with coiled shells, and the open apertured adults of the same: that is, by far the larger number of all the Nautiloidea.

When the aperture was contracted so as to present unusual development of the ventral sinus through which the ambulatory tube was extruded and a narrow aperture across the centre, but at the same time a broad opening for the extension of dorsal arms, the inference is quite reasonable that we have to deal with the remains of a powerful swimmer, and that the animal was perhaps capable of crawling, but certainly not so well fitted to hold the shell in an upright position as the open apertured types. The Gomphoceratites and Phramoceratites have this description of aperture, the T-shaped.

When, as in Mesoceras and our American Billingsites, the aperture is contracted abdomino-dorsally, and elongated transversely, forming a dumb-bell-like outline, and all traces of the ventral sinus have disappeared and the shells are habitually truncated, we should infer that the animal must have been some sort of a climber or clinger, perhaps to the stems of plants, or other animals, but could not have been a ready crawler or a good swimmer.

Among the Ammonoidea we find also a variety of apertural modifications, which are interesting, but for the most part these were open apertured. Many of the Goniatites of the Palæozoic possessed the ventral sinus large, and striæ of growth similar in curvature to the Nautiloids, showing them to have had similar habits, but in the Ammonitinæ of the Mesozoic we find a significant change. The rostrum appears, and the siphon as an ambulatory organ must have almost wholly disappeared, leading us, almost, to the unavoidable conclusion that they were more confined to a crawling littoral life than any of their ancestral Palæozoic forms.

Finally, in the shepherd's crook of the living chamber of the Cretaceous Ancyloceras, one of the degraded forms above alluded to, we find an aperture turned in upon itself, having no signs of any ambulatory powers whatever; in fact, we can imagine no way in which this creature could have progressed with comfortable celerity. We can only imagine it as stationary, either as buried with the coiled end in the mud and cleaning the surrounding space for a living, or else hanging among plants with the mouth upwards and catching what was brought by the current within reach.

A curious verification of these views exists in Argonauta, one of the naked Octopods. The egg case of the female is not a true shell, *i. e.*, it does not arise at the start in a shell-gland, but is a secondary product of the arms and mantle. Nevertheless, it occupies the position and has the same relations to the body as the true shell of Nautilus, and is a hard unyielding case composed of layers of calcareous matter, and the opinions we have advanced above would certainly be seriously shaken if it did not resemble the shell of Nautilus. It belongs to an animal having a large ambulatory tube and a good swimmer, and it has consequently exactly the structure that we should expect under these circumstances. The form is similar to Nautilus and Goniatites, having a large ventral sinus and striæ of growth with similar curvature, but the microscopical structure perfectly distinct and indicative of its independent origin.

The guard of the Belemnoidea, which we regard as the further development of the plug of the Orthoceras, is a weighty solid bar and in a cylindrical body, such as these animals are known to have had, could not have aided them in floating. That it could have, as supposed by some palæontologists, been of use like the pen of Loligo, as a sort of spring in the back, to help them sustain the frequent shocks against solid objects in their backward leaps, is highly improbable. Nature never takes the equivalent of a stiff iron bar for such uses, unless it is external, and there is a protective cushion of flesh next the animal. The only inference here seems to be, that the animal of the Belemnoidea was a ground swimmer and leaper, and the guard was used to keep it on the bottom, or else as an aid to bore into the mud, or for some similar purposes.

The ground habitat is intermediate in character between the surface swimming habitat of the Sepioidea, and the crawling habitat of the Orthoceras. According to our hypothesis the structure of Belemnoids also should be intermediate between these two types. It follows also that this intermediate structure might occur independently in the Belemnoidea, and that they were not, as their structure seems to imply, transition types between the Sepioids and Orthoceras. All palæontologists have noticed the retention of the shell, which, though internal, still resembles closely that of Orthoceras, and this alone is sufficient, even if it is not admitted that the guard is the homologue of the plug in Orthoceras, to prove our statement, and separate them from the Sepioidea. Spirulirostra of the Tertiary in which the guard is much reduced was probably a free swimmer and Spirula in which the guard was wholly lost appears from the disk on the end of the mantle to have been, as first stated by Rumphius, an animal capable of attaching itself either to floating objects or other animals.

The Sepioidea are, for the most part, expert swimmers, and the loss of the shell by the later forms occurs in accordance with this hypothesis. It first becomes an open shell, with only the dorsal side preserved in the adults, at the same time the siphon being lost, and the septa changing into a light porous mass in the interior; and lastly, in the Octopods, it is wholly lost.

Characteristics are divisible, according to our observations, into two categories, which have, heretofore, except by a few writers, not been separated. They are however widely separated in their history, in the same or different types.

We have already alluded to the independent production of the arcuate, gyroceran and nautilian forms and of the dorsal, annular and ventral lobes, and ventral siphon in several types of Nautiloids, and in the earlier Ammonoids. We have now to call attention to the fact that in each case these peculiarities could not have been inherited from the radical type Orthoceras, since they do not exist actually in this form any more than the adult bird or mammal exists in the cell which becomes an egg, or the egg which becomes an adult by growth. The transformations presented by each distinct series, however, are precisely parallel and possess the same aspect of inevitable sequence which is observable in the development of eggs into adults of the same species. Thus in the nautilian shells of the Mesozoic, whether Nautiloids or Ammonoids, if we study the smaller families and divisions within each order, we can show definitely that the lower or radical ancestral shells are almost invariably discoidal and that they become progressively more and more involute in the progressive series.

These facts and those previously noted with regard to the development of the nautilian coiled shell independently in each series, notwithstanding the separate origin of these genetic series in corresponding but distinct orthoceran forms, are of the same class of phenomena and in the same line, greater involution being simply closer coiling. We have then perfect confidence in declaring that, given the form of shell, whether straight, arcuate or coiled, with which a series may begin, if a progressive series it will necessarily produce in succession more arcuate, then loose coiled or gyroceran, then close coiled or nautilian, and finally more and more closely coiled or involute shells wherever it occurs without reference to locality, or the formation in time. That the structure progresses in the same ratio with the coiling has been demonstrated by several writers beginning with Von Buch.

There are however, as is well known, species of Nautiloids and Ammonoids which become uncoiled. These forms are close coiled in their young and show conclusively by this peculiarity and the history of their adult gradations that they are descended from close coiled or nautilian forms. They are degraded forms and this can be proved not only directly by their gradations and development, but indirectly by comparing their forms, sutures, etc., with the transformations taking place in the old age of normal nautilian shells. There is the same inevitable sequence in their evolution of form, but we must begin with a higher form instead of a lower one and in most series we have to start with a nautilian shell and say that in a retrogressive series it will inevitably evolve in course of time through the more and more uncoiled forms and finally end in a straight form.

The parallel series of forms occur only in types having similar habitats and habits; they are independent of locality and time, but not of the more general physical requirements of the habitat. They are equally strong between the Silurian and Mesozoic and modern Nautiloids, and it is from not understanding this close representation in distinct genetic series, that has led Barrande and others into erroneous statements with regard to the close affinities of the modern and the Cambrian Nautiloidea, which are purely representative forms, essentially distinct in structure and derivation, and their resemblances independent of heredity. No parallelisms occur between types in different orders having distinct habitats. There are no parallelisms or independent reproductions of similar forms in different genetic series between the Belemnoidea and Nautiloidea or Ammonoidea, as there are between the last two when compared with each other. The two swimming types, however, Belemnoidea and Sepioidea, present parallelisms of a similar kind and a succession of forms which can be closely compared.

Representative forms and characteristics appear to us therefore as direct products of similar general requirements of the habitat acting upon a radical, or common organization.

The differential characteristics are quite distinct in behavior. The old idea that Ammonoids may be distinguished from Nautiloids by the ventral siphon and ventral lobe requires modification. As a rule it is true; but as we have stated, and all paleontologists know, there are exceptions. The constant difference consists in the fact that the siphon never breaks the outline of the suture of the ventral lobe in Nautiloidea, whereas in the Ammonoidea the nearness of the siphon to the ventrum causes a slight saddle in the suture, the siphonal saddle. In descendant forms this saddle remains as a constant characteristic and is found in the young at an early stage and even independent of the nearness of the siphon to venter. In the Ammonoids, also, the so-called inverted funnel of the siphon appears. This, according to our observations, is an additional part or siphonal collar and exists in some forms of Goniatites with the funnel below. It arises at first in our opinion from the passage of the siphon through the ventral suture and then becomes fixed in the organization in the same way as the siphonal saddle in the sutures. They are in our eyes both accounted for as purely mechanical results, but being once established the conservative tendencies of heredity perpetuate them independently of each other. Nothoceras among Nautiloids has the funnel actually reversed according to Barrande, but this must not be confused with the collar above described, which has a distinct relation to the siphon, as is easily demonstrated by the microscope.

These differences are begun and maintained in all the progressive forms, and are not repeated in the Nautiloidea; so also is the division of the lobes of the sutures by marginal lobes and saddles, and other differences in smaller series are manifested in the same way.

We are perfectly willing to admit, that in this class of characteristics the Darwinian hypothesis receives the fullest confirmation. They are variable and transient often when first introduced and seem in every way to indicate the action of natural selection: that is, in our way of stating this law, as essentially the action and reaction of the animals one upon another. In the determinate succession of the representative characteristics, however, we can see only the action of the *purely physical selection* of the surroundings, and think they show that a wide distinction is to be made between the purely mechanical mode of producing modifications, physical selection, and the action and reaction of organisms upon each other, or natural selection.

All of the above remarks relate entirely to the subsequent history of characteristics after they have originated, and not to their origin. We have been able to find no indications of a difference in origin between characteristics. To us they all appear in obedience to purely physical selection or action of the surroundings upon the habits of the animals, and to this we have been able to find no objections in the facts themselves which have not been dissipated by subsequent study.

This impression has become so strong and the facts we think so convincing as to permit formulation. They have led us to regard all characteristics as probably indirect or direct responses to external stimulation, as structural equivalents of the requirements of the surroundings.

It is impossible here to make any statements adequately supporting this view, but we can fortunately refer to two writers who have arrived at very similar conclusions. Dr. Dorhn has discovered one of the most important laws of change yet formulated; namely, that organs possess latent capacities for the performance of other functions than those for which they are immediately used. Thus any change of habit may bring about the development of these latent capacities, which reacting upon the organ may entirely change its structure. Our researches lead us to adopt this view and it seems to us to throw light upon the whole question of the relation of the Orthoceratites and their descendant orders.

John A. Ryder has originated the theory of the mechanical genesis of tooth forms from the direct action of strains in using the teeth upon certain kinds of food, perhaps the best illustration of the modern view of mechanical theory of evolution, which has as yet been published. This writer also uses the term mechanical evolution in the same sense that we do, and evidently regards animal reactions or effort as an important factor in the origin of characteristics.

We find it impossible to separate now the work of Prof. Cope and Mr. Ryder clearly from each other, or our own work from that of either of these gentlemen. All we can say is, that if we have appropriated any of their ideas without proper credit, it is because we have worked them out independently, and if they serve to confirm their opinions we shall be most happy to make due restitution.

Prof. Cope also has insisted upon the necessity of bringing in the action of the organism as an important factor and regards the efforts of the animal itself as the initial stage of the origin of characteristics. This law of effort was also formulated by the author independently, but considered as a purely mechanical reaction of the organism in response to the physical requirements of the habitat and used to account for the presence of representative forms in distinct genetic series. The common organization of the radical would necessarily respond to the common requirements of the same habitat by similar changes of form and similar structural modifications. Thus while we find and must necessarily expect to find representation between the Nautiloidea and Ammonoidea in the same habitat and between Belemnoidea and Sepioidea, also in the same habitat, we do not find representation between the Tetrabranchiata as a whole and the Dibranchiata, because though originating from the same common radicals they diverge into different habitats.

This representation we have designated as morphological equivalence because the forms and structures are equivalents of each other and the organic equivalents of the physical surroundings.

Instead of being led by our researches to adopt the Darwinian hypothesis that animals have an inherent tendency to vary, we deny emphatically that animals have any inherent tendencies except those of growth and heredity. The essential character of heredity is conservation, and if not perpetually acted upon by physical selection, we should never have had any variations or any animal kingdom.

There is evident to us throughout the history of the Cephalopods a persistent retention of type characters, and in the development a perpetual renewal of ancient characteristics which are unquestionably due to heredity, and are continuous from the earliest times. Any supposition, that the same force which accomplished this could also and at the same time act in opposition to itself or have any tendency like a tendency to variation, is illogical and inadmissible. That physical selection could originate characteristics, and that these should be eventually adopted and transmitted by heredity, is a sufficient explanation of the phenomena. It is not at all requisite to imagine heredity with a double capacity or that animals had any inherent tendencies whatever except the tendencies to grow and to reproduce their own characteristics in their young.

The contemplated publication strives also to show as in previous essays, that all characteristics arise primarily in the adults of any given type, and then tend to be inherited at earlier and earlier stages in the young of succeeding generations of all grades whether individuals or varieties, or species. In this respect the author is now able to quote besides those of Prof. Cope, the confirmatory observations of Würtenburger who has arrived at precisely similar conclusions with regard to the Ammonitinæ of the Upper Jura and also Haeckel and Balfour. Though differing from these authors and from Prof. Cope with regard to the part played by natural selection, we all agree, that such phenomena as the quick development of characteristics are common and can be represented as a general law of development.

Prof. W. K. Brooks has lately published an able work upon "Heredity" in which he adopts the law of natural selection, and the mixed definition of heredity. The author attributes different roles to the different sexes. Thus he regards the female as conservative, and possessing less inclination to vary and therefore the main agent in maintaining the characters of the type, and the male as more variable, and, therefore, the immediate agent in any of the changes, or transformations of the original type characters which take place. This theory has many notable facts in its favor, but it will be difficult to apply it in forms where there are but slight, or no distinctions between the sexes, and again to the lower branches of animals in some of which hermaphroditism is prevalent as it is among sponges and Protozoa.[6] Prof. Brooks, we think, has in his theory mistaken a general result for a cause. The habits of the male being more varied, he is naturally more variable while the habits of the female being more uniform she is less variable, seems to us a reasonable explanation of the differences of the sexes. The laws of heredity would secure the transmission of the variations in whatever sex they occurred.

Again, throughout long series, like Nautiloids and Ammonoids, there are no signs of such action and we have as yet not succeeded among the fossils in distinguishing the sexes. According to Prof. Brooks' theory we ought to be able to do this and also to see that the males varied from the females in those characteristics in which the later and higher forms of both sexes vary from the older and ancestral forms.

Prof. Brooks has apparently found nothing worthy of his attention in the works of Cope, Ryder, Packard or the author, though he quotes from Mivart and others upon the subject of the quick evolution of forms. He also regards the fossils as very unreliable means of information in this respect, a point of view, which is founded upon time-honored prejudice, but where will the zoologist and embryologist get relations in time or any sufficiently extended information about the succession of forms, without a resort to fossils? Among Cephalopods, for example, the information is in these respects more perfect than any living group, as such, can possibly afford to any student, even though he should attain complete acquaintance with its structure and forms and their development.

We have said above that the changes occurring in the old age of individuals are true metamorphoses, and insisted that these had a meaning of their own, and correlated with the changes which occur in retrogressive series. Thus in an old shell the whorl tends to become uncoiled, loses its costæ, etc., and this is paralleled by the uncoiled genera and species of the groups of the diseased and distorted types of the Jura and Cretaceous.

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[6] See "Larval Theory of the Origin of Tissue." Proc. Bost. Soc. Nat. History, Vol. 23, not yet published, also Science, 1884, No. 58, and American Nat., May, 1884.

These correlations are just as evident as the correspondences of embryology and the metamorphoses of the young with the adults of ancestral forms. It only requires study and attention on the part of naturalists for the laws of geratology to become as instructive with regard to the origin and development of retrogressive and distorted types (aberrant as they are now misnamed), as are those of embryology for the progressive and healthy types.

In other words there is a branch of pathology, which is applicable to the explanation of phenomena occurring as a normal condition in every animal in old age; and this, when carried farther, shows us that there are such things as pathological, diseased species, distorted types, retrogressive series. These are all apparently of similar nature and eventually die out by reason of their pathological condition, but yet exist long enough to reproduce their own awkward distortions, in genera, and series of genera. The author upon these grounds in 1866 formulated the law that there was an exact correspondence between the life of an individual Cephalopod and the group to which it belonged: namely, the young and adolescent stages having direct correspondence and repeating the past history of its own group to a greater or less extent, the adult corresponding to the present with all the peculiarities and differences of its group, and the metamorphoses of old age to the pathological modifications and changes found in the types which arose in unfavorable localities, or which were found as a rule to terminate the history of the group in time. It was also stated that the correspondences of geratology were quite distinct from those of embryology. While the former are due to heredity, the latter in all cases are due to the action of unfavorable surroundings. They are in the same category as representative characteristics and plainly the effects of similar causes acting upon similar organisms.

We have been until lately ignorant of the fact, that Haeckel had given expression to a very similar idea and had applied it to the whole animal kingdom in the same year 1866.

We are glad to be able to quote this eminent author, and to accord to him the greater knowledge and wider application of the law, which we felt at the time of our first publication was a completion of the labors of Von Baer and Louis Agassiz upon the correlations of individual development, and that of the history of the group in time. We take all the greater pleasure in this act from the fact that Haeckel has received most strenuous criticisms, and his great services to science demand a more impartial acknowledgment than they have yet received.

The action of physical changes in the surroundings takes effect upon the irritable organism and this necessarily responds to the external stimulants by an internal effort. This effort, working from within and carrying all the influences of heredity, acts in its turn upon the parts and organs producing new growths and changes and these modifications are necessarily adapted or suited to the requirements of the habitat or the physical causes which incited the effort. Thus springing from the common radical we may have distinct genetic groups which diverge by reason of transmitted differences and yet are parallel, or have similar forms and correlated structures produced in similar succession in each group.

These morphological equivalents are general in their distribution with regard to space and time, and the causes must have been equally general and equally similar.[7]

All morphological equivalents seem to us to be sufficiently accounted for without the aid of natural selection, since the obvious fact that they are advantageous to the animals is a necessary result of their adaptability, a consequence of their *physical selection*. They are advantageous in the same way that it is of advantage to a drop of liquid to be round, or a cell when equally pressed on all sides to be hexagonal.

Differential characteristics, however, are only at first identical. After their origin we find them occurring in limited series of forms, appearing and disappearing according to no traceable law of succession, limited also in space and time, and having a fixed hereditary continuity and comparative invariability, which exhibit how exclusively they have become the property of heredity.

There is a time in each type, however, when they appeared as variables, when they were found in some, and not in other closely allied forms. We can understand that, at this time, the organisms possessing them are able in their struggle with others to maintain and perpetuate themselves with greater success on account of these advantageous differences, and that by this success the differences would naturally become fixed in the organism and be adopted by heredity. Then they would assume the character of essential differences and the comparative invariability which distinguishes this class of characteristics from morphological equivalents.

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[7] It must be observed here that we have carefully avoided mentioning a large number of characteristics such as the bilateral symmetry of the shell and the unsymmetry of many spirals, etc., which are, as we have tried to show in a previous essay, due to the still more general and uniform action of gravity. These are also morphological equivalents, but we have not the space even to allude to them as their importance deserves.

Natural selection does not act according to our experience beyond the period of variability. All characteristics, according to the law of concentration, tend to be inherited at earlier and earlier stages in descendants, and the differences, when fixed are no longer variables and come under the full influence of the conservative force of heredity. They are not dependent upon the fate of battle, but are part of the invariable tendencies of the organism, so long as it remains in the same habitat. They may disappear or be dropped in some of the last of a series, out of the development of the young, in accordance with the law of concentration of development, or they may be retained, but in all cases they show the behavior of strictly hereditary characteristics of the class of differentials and serve to distinguish groups because of their comparative invariability of occurrence. They are the purest illustrations of the law, that like tends to produce like. One has only to think of the characteristics mentioned above as differentials in the Ammonoidea to understand these statements. The embryosac or protoconch, the ventral suture with its peculiar lobe, the nautilian form are all perpetuated steadily without break from the Cambrian to the Cretaceous inclusive, and the Nautiloid differentials persist in that order even in the existing animals. Similarly, in any small series, if we can pick out its differentials, we are presented with the same phenomena of comparative invariability due to heredity. In fact our classification of organic beings entirely depends upon this fact. If it were not true, we could not make a classification, which would represent natural relations. The methods pursued by all classifiers show this. While we talk freely of the need of founding classifications upon all the characteristics of animals, no one practically follows this empty wisdom. On the contrary, and of necessity, naturalists have been wiser than their own precepts, and really sought for those constant differences, which are the hereditary differentials.

We do not regard the law of concentration as an inherent tendency of heredity. We think it is universal, and that it can be traced in man, as well as the Cephalopods, to an extent which throws light on his intellectual as well as his structural development, but it appears to be a result of the action of external stimulation upon the development.

Balfour not expecting, as an embryologist, to get any information from a paleontologist naturally omitted to notice, that in a memoir upon Planorbis at Steinheim, we had already attributed the concentrated or quicker development of some types to extra protection afforded by the parents to the young, and repeated our conclusions in his "Comparative Embryology."

His observations are valuable confirmations, and we can point to this as probably one at least of the causes of concentrated or quick development. It is however not the sole cause. Perhaps no types among Cephalopoda are more concentrated or have quicker development in Balfour's use of that term than the pathological type like Ancyloceras, Crioceras, etc., and that they had extra protection from their parents is an inadmissible supposition. They are of all grades, and diseased varieties may be followed into normal series or species on the one hand, and on the other by gradations into series of distorted forms with highly concentrated young. If these were protected the normal forms into which they grade must have been capable of affording extra protection to some varieties, while not possessing it themselves. Another fact is that their shells are the smallest in diameter, mere tubes in many genera.

All our observations agree however in one important particular: protection is certainly in its effect upon the young a stimulant, and may be said to furnish the most favorable conditions for an early and premature physiological development. It will not astonish pathologists, when it is asserted, that in this respect, protection is like the action of certain pathological causes which tend to produce premature development.

In fact we think, that while the causes which can have produced a tendency to concentration are almost entirely dissimilar and irreconcilable in themselves, they nevertheless possess the general and common character of being very favorable or stimulating to the earlier stages of development and unfavorable to growth. They belong in other words to the class of pathological causes and when in excess are highly unfavorable to the continuance of life itself.

Their normal action in these retrogressive and distorted forms is unfavorable; whereas, when stimulation arises from the ordinary action of physical selection, it may be entirely favorable and we think this is the only reasonable explanation of the phenomena.

Würtenburger and Haeckel both regard characteristics as tending to be inherited at earlier or later stages in descendants. If Würtenburger and Haeckel are correct then we ought to find series with tendency to inherit ancestral characteristics at later stages, or isolated forms with characteristics appearing at later stages than those in which they appeared in their ancestors. We have entirely failed to find any such instances. If they can be adduced, then the law of concentration would, as claimed by Haeckel and Würtenburger, be a result of natural selection. Neither Haeckel, Würtenburger or Darwin, has brought forward any such examples, so far as we know, and the latter is particular in stating, that characteristics tend to be inherited in successive descendants *at the same stage or earlier than that in which they appeared in the ancestors.* We can readily understand how characteristics may tend to be inherited at the same stage because of the invariable action of heredity, but we recognize no inherent tendency to change in any phenomena of heredity we have yet studied. The tendency to earlier inheritance is the result apparently of disturbing and modifying agencies acting from without as must all other causes of change in the organism.

The Argonauta is an example of an animal protecting its young and having a concentrated, or, as Balfour and others call it, quicker development. Many of the Spatangoids are similar examples. Man is the most remarkable example, since here we can with Cope trace the effect of protection not only in the nursing of the young, but in the subsequent care of rearing, and in education as its consequent.

In pathological types the development is enormously accelerated, as in the senile characteristics of syphilitic children, and the pathological types of Ammonites. The Baculites and uncoiled cretaceous Ammonitinæ, for example, are prematurely stimulated in the development of their senile characteristics by pathological causes.

In fact the only way to account for the removal of the progressive characteristics of the normal ancestors, and their replacement by senile characteristics, is to attribute the phenomena to the action of the stimulating effects of the surroundings, just as we are obliged to attribute the premature development of syphilitic children, or premature developments and extra growths of all kinds, to overstimulation by the unfavorable surroundings acting directly on the parents and indirectly through heredity on the children.

Thus Balfour's idea of protection, as applied to the so-called quicker development of the highest forms in different types, cannot possibly apply to diseased forms, or, as we might show if there was space, to parasites, which are however the finest possible illustrations of the effects of the surroundings, in producing concentration without protection. Nor does it apply to the uniform and constant manifestation of the law of concentration which takes place in every series. The highest members, *i. e.*, the terminal ones of series, are invariably more or less concentrated, and when the development skips stages, which are common in ancestral forms, as it does almost invariably in such forms or species, we cannot appeal to protection as an explanation. The skipping is evidently the direct consequent of the action of the law of concentration. The characteristics skipped have become useless; and as both Würtenburger and the author among Ammonoids, Cope among the reptiles and battachians, and Haeckel more generally, translate the phenomena, they appear to be literally crowded out of the development by the earlier inheritance of the immediate ancestral characteristics. For example, the highest form of the Arietidæ is a small species, *Asteroceras Collenotii*, which has no stage in its development comparable with the adult of its ancestral *Asteroceras obtusum*, and yet we traced it, after several years of research, back through other forms directly to this species, and in this we have repeated the previous experience of [1]Quenstedt and Prof. Fraas. Every series of Ammonites gives similar results, which have been noted in essays by Würtenberger and the author and the writers above mentioned in larger series.

The types with concentrated development thus skipping stages in smaller series are precisely comparable with the diseased types in their relations to the old age of the individual in the larger series: they present the same phemomena as the larger series of retrogressive forms in a less degree, but still perfectly traceable by definite characteristics. They lose the ornaments which distinguish the adults of the purely progressive species in their own series, and have smooth whorls; they usually also have less robust whorls, and the shell is not so large in diameter; they often retain in respect to their smoothness and the complications of the sutures, the conditions of their own younger stages throughout life, and thus present close parallels to the purely retrogressive forms of pathological series; they are also invariably either the dying-out forms of their respective series, or when on the same level the extreme aberrant forms.

Dr. C. S. Minot's essay,[8] Is man the highest animal? shows this mingling of characteristics clearly in the highest type of the animal kingdom, the mingling being due to the retention embryonic characteristics. This retention in a higher type, is a form of degradation which tends to destroy the adult differentiations of the type. Thus the excessive and premature development of the brain in man has prevented the development of the differential, simian characteristics of prognathism, reducing the bones of the face and increasing the size and importance of the skull. We can in the case of man, clearly see that the highest physiological development tends to destroy the growth or development of purely structural differentiations of the ancestral type. Geratologous types, therefore, present the phenomena of retrogression, but have also, frequently the highest possible developments of some of the characteristics and tendencies. Thus in *Asteroceras Collenotii*, the whorl is flatter and more acute and more involute than in any other form of its series or of the family of the Arietidæ, and the keel is developed earlier. In some series, on the other hand, as in the Stephanoceran, such forms may grade into those which belong to distorted, retrogressive series like Scaphites. We think therefore we have every reason to consider them and the retrogressive forms as having in common direct correlations with the retrograde metamorphoses of the individual, and consequently feel justified in classifying them in the same category as geratologous forms. Nevertheless, here as elsewhere no hard and fast lines can be drawn, and, strange and even paradoxical as it may seem, they must be in many instances regarded like man, as the highest of their series on account of their concentrated and accelerated development, and sometimes, as in *Asteroceras Collenotii*, exhibit the most extraordinary development of certain tendencies, observable but not so highly developed in their ancestors.

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[8] Proc. Am. Assoc. Vol. xxx, Cincinnati Meeting.

They are also in smaller series often comparable with their earlier ancestors, just as the diseased Baculite as the terminal form of Ammonoidea is comparable with the Orthoceras. Thus the *A. Collenotii* can be compared in its later adolescent stages very closely with the adult of the ancestral radical of the family of the Arietidæ, *Psiloceras planorbis*, and still more closely with the immediate radical of its own genetic series *Agassiceras striaries*, which in some varieties is often confused with *Psiloceras planorbis*, and in the adult still retains the smooth whorl of that species. The extreme old age of this species Collenotii has never been seen, but if it reduces the whorl in size at that stage as others do in the Arietidæ, we should not be able to separate it from Psiloceras if the adult was unknown. The mingling of the progressive and retrogressive tendencies in the development of such forms as occur at the termini or paracme of small series, as Haeckel terms the descent and fall of forms, makes these comparisons less apparent than they are often between the purely retrogressive forms in the paracme of larger series, but that they belong to the same class is evident. The differences lie in the possession by geratologous forms of small series of a greater or less number of progressive characteristics which are developed, and prevent the tendency to the development of the senile characteristics. In the above case, for example, they permit the senile characteristic of smoothness, and also the simple acute, channelless abdomen of the young to be perpetuated in growth, but they develop otherwise a highly involute, laterally flattened whorl, which is a step in advance of all others of its own series.

All of the geratologous degraded characteristics are also similar to the larval characteristics, and consequently when the progressive characteristics disappear, we get as a result fusion of geratologous with larval characteristics, or persistent development of the latter combined with geratologous tendencies.

In purely retrogressive forms or purely geratologous types like Baculites, etc., the fusion of characteristics is complete, and the overstimulation of the pathological surroundings has to be called into account for the premature development of all the larval characteristics, the cylindrical form of the adolescent young, its smoothness, its simple sutures and the earlier inheritance of the tendency to degenerative uncoiling. We are, therefore, really presented, in Baculites, with a type comparable closely with the straight cone of the larval stage of *Gon. fecundus* of the Silurian, and with the primal adult ancestor Orthoceras, the resemblance to the radical being more complete because the process leading to the reversion is more completely carried out. The distal ancestor Orthoceras passes, as we know, through but few changes in its individual development, and has, what could be appropriately called, the direct mode of development. The higher types of the Nautiloids have the cyrtoceran, and gyroceran stages and the correlative metamorphoses of the septa and siphon in their younger stages, and have the indirect mode of development, or pass through a larger number of metamorphoses; the geratologous types have again the direct mode, on account of the skipping of progressive characteristics, or through their replacement by the senile tendencies and persistent larval characteristics. The purely adaptive nature of the indirect mode of development, which has been most fully studied among insects, is quite generally admitted by entomologists, and is prolonged or shortened according to the seasonal requirements of the surroundings, and may be regarded, in the main if not wholly, as the structural equivalent of the surroundings.

We have tried to show, that the more or less direct development of the geratologous forms among the Ammonites was possibly due to the stimulating effects of the surroundings producing premature and constant development of some of the senile characteristics in combination with the earlier development of the high progressive characteristics of the series or division, or where stimulation was excessive, replacing the latter entirely with the larval characteristics, and geratologous tendencies to uncoiling, thus producing larval types with direct development out of the ancestral progressive types. We have also tried to give sufficient reasons for our opinion that in all of these types the marks of effort are as apparent in the retrogressive as in the progressive series. This result is as clearly due to the habits of the aberrant or uncoiled types, as is the ordinary phenomenon of progression, but there is not space to discuss this point, and must refer to article in Science, 1884, No. 53, p. 148.

We can formulate our conclusions as follows:—

1. We have endeavored to demonstrate, that a natural classifi-

cation may be made by means of a system of analysis in which the individual is the unit of comparison, because its life in all its phases, morphological and physiological, healthy or pathological, embryo, larva, adolescent, adult and old, correlates with the morphological and physiological history of the group to which it belongs.

2. In the different genetic series of a type derived from one ancestral stock, there is a perpetual recurrence of similar forms in similar succession, which are usually called representative and often falsely classified together, though they really belong to divergent, genetic series.

3. These forms and their similar characteristics are not derived by direct inheritance from the common ancestor in which all the forms are necessarily similar and primitive, but originate everywhere independently of hereditary influences in the different series, and also in all formations independently of chronological distribution.

4. This evolution of similar morphological changes in the forms of different genetic series must be regarded as the similar reactions or efforts of a common organism in direct response to similar, generally distributed physical causes active in the same habitat, and are, therefore, necessarily similar to each other though in different genetic series. As a whole, they may be said to express the general tendencies of modification, due to the efforts of the common-radical and common organization while spreading in all directions and in different genetic lines to respond to similar physical causes, and meet their requirements with suitable changes. They are, therefore, structural equivalents of each other in different series, and functional equivalents of the general requirements of the environment, or habitat, purely physical selections.

5. All new characteristics, even those which are purely mechanical reactions of the tissues, arise in a similar manner as reactions to the exciting agency of the more general, or more localized physical causes, and are necessarily their corresponding, organic or suitable equivalents, structurally and functionally.

6. After their origin, however, and during their subsequent history, characteristics are divisible into two categories: those which become morphological equivalents, and are essentially similar in distinct series, and those which are essentially different in distinct series and may be classed as morphological differentials.

7. These differentials are absent in the first members of series, then on their appearance transient, but afterwards tend to become invariable, or fixed in the stock or series, being perpetuated and increased by direct inheritance in successive generations, species, etc. They may finally often disappear in the retrogressive or highest and last occurring members of each series, or in aberrant forms when on the same level.

8. They have no determinate mode of succession, but are usually more or less isolated modifications and arise first in individuals or varieties, but afterwards become characteristic of species and finally of the major part of the direct line in every descendant series.

9. They are, therefore, strictly adaptive, variable characteristics and not directed in their occurrence or development by any more or less invariable law of succession as are the morphological equivalents, and their perpetuation cannot be attributed to physical selection. Their rise and ultimate prepotency as inheritable characteristics appear to be due to the action and reaction of the animals upon each other, viz., to the law of natural selection.

10. Differentials, therefore, can be separated from other characteristics of the same parts, by careful observation and close analysis of their behavior in series, but cannot be specifically predicted from the study of other series; whereas, morphological equivalents can be predicted with the same certainty as the recurrence of any other physical phenomena.

Thus we can say of any new series of Nautiloids or Ammonoids, that habitat, *i. e.*, causes remaining similar, and the form with which the series begins being also known, they will, whenever or wherever found, tend to develop arcuate, coiled, close coiled or discoidal and finally involute forms in progressive series, and reverse this process in retrogressive series.

11. All modifications and variations in progressive series tend to appear first in the adolescent or adult stages of growth, and then to be inherited in successive descendants at earlier and earlier stages according to the law of concentration, until they either become embryonic, or are crowded out of the organization, and replaced in the development by characteristics of later origin.

12. Modifications which tend to appear in the old age of the individual of the progressive series, correlate with the modifications taking place in pathological series of all grades, and in

geratologous and retrogressive forms of all kinds, however progressive they may be in certain characteristics. Geratologous forms, therefore, show that the development of hereditary retrogressive characters has been stimulated so as to take the place of the progressive, thus either partially or completely replacing them. The partial replacement is often accompanied by the early development of the hereditary, progressive characteristics.

Geratologous forms may, therefore, be the highest members of progressive series, or the terminal members of retrogressive series, and the stimulation of the development appears to take effect upon both progressive and retrogressive characteristics, thus producing at the same time and in the same shell, (1) the earlier or premature development of some of the progressive characteristics combined with geratologous characteristics, (2) the earlier development of geratologous characteristics and their fusion with larval characteristics which occasions the complete replacement of progressive characters and occurs only in the extreme forms of retrogressive series, and in parasites.

13. The law of concentration in development seems to express an invariable mode of action of heredity, in the earlier reproduction of hereditary characteristics of all kinds and under all conditions. In progressive series it acts upon healthy characteristics and appears to be an adaptation to favorable surroundings, and in retrogressive series upon pathological characteristics, and is probably an adaptation to unfavorable surroundings usually leading to the extinction of the series or type.

14. In following up series, it has been found that the development of ancestral forms is simple and direct; that of their more specialized descendants becomes gradually indirect, acquiring complicated, intermediate or larval stages, and that of the terminal retrogressive or geratologous and pathological forms becomes again more or less direct.

15. The introduction of adaptive larval stages into the history of individual development in any series appears to be due to the direct exciting action of the surroundings, and their absence or subsequent suppression to some physical agency, changes of habit or protection, or to pathological causes.

All of these causes must, however, be considered as similar in their effects upon the young, *i. e.*, direct stimulants producing excessively rapid development of the ancestral progressive characteristics, or of the retrogressive or primitive larval characteristics, inherited from the progressive forms.

16. The law of organic equivalence may be descriptively stated as follows, the action of physical changes takes effect upon an irritable organism, which necessarily responds to the external stimulants, by an internal reaction or effort. This action from within upon the parts of the organism modifies their hereditary forms by the production of new growths or changes which are, therefore, adapted or suitable to the conditions of the habitat, or the physiological equivalents of the physical agents and forces from which they directly or indirectly originated. In so far then as causes and habits are similar they produce representation or morphological equivalence in different series and in so far as they are different they produce the differentials which distinguish these series from each other.

MYA ARENARIA : ITS CHANGES IN PLIOCENE AND PREHISTORIC TIMES.

By E. S. MORSE of Salem, Mass.

[ABSTRACT.]

AT a previous meeting of the Association, the author showed that the species of shells found in the Indian shellheaps along the coast of New England differed in their proportionate diameters from the same species living to-day. He pointed out, moreover, that species belonging to similar genera, in the shellheaps of Japan, had changed in precisely similar ways. It was important to find out, if possible, the cause of these changes. A comparison between the shells of two common species, found north and south of Cape Cod, gave indications that temperature was the inducing cause. The two species selected were *Mya arenaria* and *Venus mercenaria;* the former extremely variable, the latter very constant, in its characters. Specimens of these species had been collected in great numbers, both recent and ancient. The following are the indices of *Mya arenaria:*—

RECENT.		ANCIENT.	
South of Cape Cod,	North of Cape Cod,	South of Cape Cod,	North of Cape Cod,
61.42.	61.67.	62.	62.78.

of *Venus mercenaria*:—

RECENT.		ANCIENT.	
South of Cape Cod,	North of Cape Cod,	South of Cape Cod,	North of Cape Cod,
81.01.	81.10.	81.51.	81.81.

Since the waters south of Cape Cod are much warmer than those north of Cape Cod, it was reasonable to suppose that these changes were due to temperature, and that the higher index of the ancient specimens found in the Indian deposits might indicate a colder climate. This supposition receives some support in the fact that a measurement of specimens found in the glacial clays about Portland, Maine, and on the Kennebec river in the same state, gave the high index of 66, and a number of Norwich and Red Crag fossils of *Mya*, which he had the opportunity of measuring at the British Museum, had an index of 64; recent *Mya* from South End, England, having the low index of 58.30.

It was interesting to observe, that measurements of *Mya* in Japan gave, for the southern form, an index of 61.10, and, of a more northern form, 62.50.

A New Plan of Museum Case. By Edward S. Morse of Salem, Mass.

[ABSTRACT.]

This paper described a new plan of museum case devised and constructed and in practical use at the museum of the Peabody Academy of Science in Salem.

Conscious Automatism.[1] By C. P. Hart of Wyoming, Ohio.

The application of Nitrous Oxides and Air to produce anæsthesia; with Clinics on Animals in an experimental air-chamber.[2] By E. P. Howland of Washington, D. C.

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[1] For a short notice of this paper see Science for Sept. 7, 1883.

[2] A notice of this paper is given in Science for Sept. 7, 1883.

SECTION G.

HISTOLOGY AND MICROSCOPY.

[SEVERAL meetings of Section G were held but papers were not read before the section.]

SECTION H.

ANTHROPOLOGY.

ADDRESS

BY

PROFESSOR OTIS T. MASON,

VICE-PRESIDENT, SECTION H.

THE SCOPE AND VALUE OF ANTHROPOLOGICAL STUDIES.

EVERYTHING that comes before the human mind has to pass through a process of weighing and measuring, and receives a valuation according to the thinker's standards of merit. In this critical spirit let us pass in review those studies called anthropological, in order to form some estimate of their value according to the measures commonly applied to various departments of learning.

Anthropology is the application of the instrumentalities and methods of natural history to the study of man. The anthropologist, in this sense, is not a dilettante philosopher, who inquires about old things because they are old, or into curious things while they are curious, omitting all the great movements and needs of society, and overloading the baggage-train of progress with trumpery picked up along the march. The practical spirit of our age demands that he ask what truth, or good, or beauty comes from such investigations, and how he can make them subservient to human weal.

As to the scope of anthropology, we may be instructed by the other sciences. The natural history of any species, say of the domestic horse, includes many inquiries, such as the time and place of its origin; its ancestry; its pristine size, appearance, and mode of living. We should afterwards inquire concerning the archæology or the palæontology of the Equidae, their embryology, anatomy, physiology, diseases, abnormalities, and external characteristics. Mr. Romanes would have a chapter on the intelligence of the animal, viewing its nature and amount, supplementing the discussion with notes on the various ways in which the horse manifests its mind, its wills, emotions and opinions. Horses do not construct elaborate houses like the ants and the beavers; but the members of all species occupy their daily lives in some habitual industries by means of which they wear out the excess of muscle. Sir John Lubbock would lead us farther, and show us that horses go in droves, follow a leader, plan migrations, attacks, and defence, amuse themselves, enjoy one another's company, improve in appearance, intelligence, and usefulness by cultivation, — in a thousand ways show themselves to be social creatures. At last Mr. Mivart would insist that the horse has its habitude (ἕξις), its manner of action, its economy (œcology), and its members are affected in a characteristic manner by heat, light, moisture, winds, the kind and quality and abundance of food and drink, by beneficial or injurious animal neighbors, and by the vital, procreative, inheritable energy with which they are endowed. These and many other kindred inquiries concerning this homogeneous group would constitute the science of hippology.

The conscientious devotee to this science would frequently ask himself what practical good would result from all this expenditure of time, thought, and resources necessary to collect specimens and facts, and to formulate his science. Could they be employed on some subject more ennobling and profitable to himself, better calculated to inform, enrich, and beautify mankind?

Now, instead of horses, let us substitute the *genus homo*, laying aside all predilections; and, if possible, let us imagine the student of anthropology to belong to quite another genus than the subject of his research. He would have, in the fourteen hundred millions of human beings now living on the earth, and in the remains of their congeners slumbering in its bosom, perhaps the best defined group of animals. Calling them a genus or a species, as you like, they are so well hedged off from all other animal groups that not the least embarrassment has ever disturbed the naturalist in distinguishing the anthropos even from the anthropoid. No one was ever puzzled to tell, concerning any living thing, whether or not it was a human being. The earth has never yielded a bone concerning which the practical anatomist stood in doubt whether it had been once part of a human body.

Now, I take it for granted that any inquiry whatever which would be useful or entertaining respecting another species would be intensified in importance having man for its object. Indeed, there are few questions which naturalists are wont to propose to their groups which ought not to be carefully considered when we are studying man. Before entering upon the weighing process, therefore, it may make our task more easy if we consider the present scope of anthropology, and briefly pass in review some of the questions which are being propounded by anthropologists every day.

When did man first appear on earth, — at what time and in what geological horizon?

Have all the individuals of our race descended from a common human ancestry? in other words, are we monogenists, or polygenists?

Where was the birthplace of humanity?

What manner of creatures were the first men in specific characteristics, in size, aspect, intelligence, and social condition? and how did he get here?

To all such queries, Haeckel aptly gives the name of anthropogeny; therefore, in order to be anthropologists we must be anthropogenists.

Another set of questions relates to that stretch of time which lies between the pristine man, or the pristine condition, and the beginnings of recorded history.

Have we complete, irrefragable evidence that our race has progressed from a brute-like condition, in which it was devoid of all experience and appliances?

What application must we make of Professor Tylor's belief that civilization has progressed upward like a column of vapor, some parts advancing while other parts are being rolled downward, but the whole ascending and expanding?

Granting that there has been improvement, what paths have been pursued?

Speaking of our own peculiar province, what is the real import of such discoveries as those of Dr. Abbott and Professor Whitney in establishing the great antiquity and early rudeness of the American savage?

Who were the builders of the mounds, earthworks, and cliff-dwellings, and the stone structures of Middle America?

What were the functions of these various edifices?

What credence is to be given to the early historians of American culture?

Already we have our schools of interpretation, such as the Bancroft school and the Morgan school. Where, among these opposing schemes, does the truth lie?

In the administration of this science, there is occupation for the greatest diversity of talent. The biologists of our time are entering into the minutest inspection of the life-history of each animal form. With enthusiasm the embryologists trace the modifications of structure as they succeed one another in the germ. Before their eyes the very play of creation is dimly shadowed, and organic structure built up. They pass their work on to the anatomists and physiologists. Now, the anthropologist must endeavor to comprehend the work of all in its synthesis. As Newton and Laplace grasped the unity and organization of the stellar world, as Humboldt gazed upon all created things as elements of the universal cosmos, as Darwin first conceived the consanguinity of all living beings and their mutual help or harm; so the anthropologist seeks to unite all that can be known respecting man into a comprehensive science, and to study the innumerable correlations which bind the most incongruous actions and thoughts together into harmony. May we gain help, in solving questions of human origin, by carefully observing the evolution of the embryo?

Does a knowledge of the life history of the individual furnish a clew to the life history of the species?

What does a comparison of the anatomy of man with that of the quadrumana say respecting the genealogy of the species?

What are the proper methods and instruments of anthropometry, —observing the growth of children; the dimensions, angles, and curves of the cranium; the diversity and size of the brain according to age and sex and race; the weight of the body; the color of the skin, hair, and eyes; the muscular movements; the development of faculties; longevity, fecundity, and plasticity under change of environment; and vigor? and what are the legitimate inferences to be drawn from such investigations?

Finally, by what devices can the multitudinous correlations of structure and function in the human body find expression in graphic methods?

Another set of observers must now be brought into this great laboratory. We have to deal with a group of animals in which intelligence has manifested itself to such a degree as to dominate all other functions. Teleological inquiries can be no longer excluded. Hitherto the application of scientific methods to the mind has required that we should be satisfied with sensuous results of thought, and forbidden us to inquire into the nature of the mind itself. Now, we are met at the outset with this puzzling question: Shall consciousness, or introspection be admitted as an instrument of observation?

How are we to record its dicta? and how (to borrow a term from the astronomers) shall we eliminate the personal equation?

Or, if we are not in a position to admit introspection among our tools of observation, can we not invent some delicate apparatus by means of which the strength of feeling and the inmost thoughts may be known and measured?

Does the brain generate thought as the liver generates bile?

What can science tell us concerning the existence of a human soul, non-material, and not susceptible of measurement by the standards of well-known forces?

How does it come about that children inherit the traits, tendencies, and faculties of their progenitors?

By what routes does the mind pass on its way from infancy to maturity?

What use should be made of the multitudes of inquiries prosecuted with reference to the minds of animals, in the study of human reason?

The student of anthropology frequently finds himself in sympathy with Wordsworth, singing,—

"Our birth is but a sleep and a forgetting;
The soul that rises with us, our life's star,
Hath had elsewhere its setting,
And cometh from afar."

If, as Mr. Spencer says, that which we inherit represents the accumulated experiences of a thousand generations, is it also possible to retain the consciousness of those experiences? Will the sensitiveness of consciousness keep pace with the growth of knowledge, and obviate the necessity of laborious records? In which case we should have mental and spiritual atavism explained, and that universal sympathy felt by cultivated people for those standing on the lower steps of civilization.

Now, whatever thoughts any other creature than ourselves may have, and leaving out the possibility of mechanical mind-reading in the future, up to this time the only knowledge men have gained about one another's thoughts has been acquired from expression. The expression of thought is language. Dr. Hoffman finds language in rock paintings and carvings; Col. Mallery, in gestures; Mr. Thomas, in the Maya hieroglyphics; and the glossologists, in human utterance. Happily for us, they are a clever set, and well up in their craft. Let us hear some of the questions they are discussing:—

What are all the devices employed by living creatures to express their thoughts, emotions, and volitions?

Which took precedence in the origin of language, signs, or vocal utterances?

What is the explanation of the origin of language?

What light does language throw upon the origin of species?

Is the evolution of language a safe guide to the knowledge of the unfolding of the human mind?

By what lines have the forms of speech progressed?

How far is similarity of language an evidence of consanguinity among peoples?

Is there a genetic relationship between monosyllabism, polysynthetism, and inflection?

What credit must be given to the ear, and the invention of writing, in the conservation, and lines of progress, of language?

How should languages be classified?

Here we may leave the students of language, and take a new guide. Looking over the earth, we behold men divided into races or consanguineous groups, filled with race prejudices, and restricted by race capabilities.

What are those external and anatomical characteristics which have become transmissible by inheritance? When and how were they fixed? Are we to imagine, with Dr. Kollmann, that certain race forms were fixed far back in the past, just as the chemical elements were made irresolvable by a former state of matter?

Of these heritable marks, which is the best criterion of race,—the skull, the color of the skin, the texture of the hair, language, art, social organization, or mythology? or is it certain fixed correlations of these and other characteristics? If so, what are the laws of correlation and conservation in the races? Should the same set of structures be depended on in each race? How many races of men are there? and are these species, or varieties?

In what manner should the question of race enter into the administration of politics, economy, education, and colonization?

It is impossible to say when this subject of race first became attractive to human minds. In the oldest histories and on very ancient monuments, are to be seen attempts to classify the families of mankind. In all the encyclopædias, under the word "ethnology" will be found the schemes of modern writers. But, since the commencement of our century, the subject has been taken out of the hands of individuals, and has engrossed the attention of societies. Manuals of instruction have guided the voyager and the traveller in recording the characteristics of races. In Stanford's Compendiums, based on von Hellwald, Prof. A. H. Keane has commenced a codification and synonymy of all the tribes of men. This he proposes to follow up with a biographical dictionary of tribes. The Bureau of ethnology has collated the names, priscan homes, migrations, and bibliography of all the North American Indian tribes; so that we are in a fair way to know something about the races of men, by proceeding from particulars to a general view.

Passing from man to his works, we are face to face with æsthetic and practical art as a unique study. All art relates to human desires for food, clothing, shelter, for activity in peace and war, for beauty, for social and spiritual happiness. Mr. Tylor has taught us to look upon art products as species that have had an evolution, a life history; and this was very much the plan of Gustav Klemm. This sort of study has captivated many anthropologists, and they are asking such questions as these:—

Admitting that the arts have been progressive, what have been the lines of their elaboration?

May we, by a process of elimination, trace backward the life history of each art, as would a patent attorney or a chancery lawyer?

At what degree of workmanship may we be sure that flakes of flint, gashed bones, and wrought wood, give evidence of human handicraft?

When does similarity of art-forms indicate social or commercial contact? when, consanguinity? and when, merely the same grade of culture?

Is degenerate art a facsimile of early, progressive art?

Is it allowable to fill up the gaps in the arts of any tribe by seemingly intermediate forms from other tribes?

Whence is the sense of beauty?

The answers which we unconsciously give to these queries are the major premises of our arguments respecting the history of civilization.

By marriage in some of its forms, human beings are united into consanguineous groups, whose varied needs demand and produce many bonds of union, and widen the separation from other groups. With reference to each set of duties in the tribe, unwritten or written codes embody a system of ethics, regulating conduct in every particular. Farther on in their history, groups have relations of war and peace, and the absorption of homogeneous and heterogeneous peoples into a defined area gives rise to nationalities.

Were men ever herded together in promiscuity?

What were the earliest forms of social life?

What were the most primitive forms of marriage in groups?

Have all the tribes of men passed through the same systems of consanguinity and affinity?

Can the highest systems of altruistic ethics be explained by natural processes?

What are the most beneficial relations of labor to natural resources? and how have the present relations been brought about?

What is the history of the control of the body politic over the individual, and of the jurisdiction of corporations? and to what extent may individual freedom be controlled without discouraging private ambition?

What has been the life history of communism, crime, fashion, and politics?

Is it possible to regard and define facts in sociology by the terms of physical science?

Again, these human beings spend a great portion of their time acting and speaking as if other eyes and ears than those of mortals were cognizant of them. In the darkest nights, at the rising sun, throughout the day, at certain seasons of the year, this unseen world is invoked. In groves, in caverns, in estufas, or in costly temples, it is all the same: praises, petitions, and offerings confront the inscrutable power that can work men weal or woe.

How did man come to believe in the animation of things, fetiches, the wanderings of ghost-souls, spirits benevolent and malignant, the gods of classic mythology, and the Great Father of all?

What are the first conceptions of children respecting such things? and will these guide us aright to the childhood of faith?

Has the history of mythology run parallel with the history of material and intellectual progress?

How may we divest ourselves of the personal equation, and learn the true psychology of savage worship?

Is Dr. Brinton right in applying the rules of interpretation adopted for Aryan mythology to American Indian myths, and in assuming that their crude stories are disguised deifications of the phenomena and powers of nature?

Finally, as men wander about the earth, and certain families are to be found chiefly in certain localities, so is it with races. Longevity, fecundity, and vigor are influenced by such causes as height above the sea-level, purity of the atmosphere, amount and distribution of heat, moisture, winds, fertility of the soil, and proximities, whether they be vegetal, animal, or human, whether they be beneficial or injurious.

By what subtle chemistries of the things around us, by what exposures in this terrestrial camera, come to pass the various hues of the skin and hair and eyes, the long skull and the short skull, the long face and the broad face, and the fixed compounds called natural characteristics?

By what processes of selection and adaptation has this cosmopolitan species come to occupy the whole earth, its genial climes, its frozen areas, and its fever-cursed tropics?

Is it possible to control these phenomena, or to adjust the human machine so as to anticipate and assist nature, to expedite natural selection and the survival of the fittest? or even to subdue nature, and decide for her what shall be the fittest to survive?

From this hasty survey of the scope of anthropology, we return to inquire what benefit the world derives from the cultivation of this science.

I answer, first, that every study is improved by study. All things become clearer to him who steadily fixes his gaze upon them. The sciences all began with vain speculations,—astronomy with astrology, chemistry with alchemy, geology with cosmogonies, biology with nature-worship, and theology with mythology. Long before the word "anthropology" was employed in its present acceptation, Alexander Pope wrote, "The proper study of mankind is man." But, millenniums before his day, mankind studied mankind by the light of their time. The study of man is no new thing, therefore. Now, since human thought has run, and will continue to run, in that direction, it becomes our privilege to rejoice that the stream has in these last days run wider and deeper and clearer. The proper study of mankind is the scientific study of man, the multiplication of rigorously exact observations, the collection of thousands of well-authenticated specimens, the classifications of both observations and specimens on rational bases, and the limitation of our conclusions to the extension of our premises. Some of my hearers have worked systematically and patiently for years at American archæology, and the anthropology of the modern Indians; and you rejoice with me to-day that our science has at last attained dignity and respect. With profound veneration I mention the names of Hildreth, Atwater, Stephens, Gibbs, Schoolcraft, Morton, Gallatin, Wyman, Squier, and Davis: with what buoyant hope they looked forward to this day, and with what exquisite pleasure must such living witnesses of the beginning as Horatio Hale, Col. Whittlesey, Dr. Jones, and Mr. Hempstead now contemplate the progress of solid work! The Smithsonian Institution will have to republish Squier and Davis, with many additions and corrections by Dr. Rau; the Bureau of Ethnology will antiquate Schoolcraft and Gallatin and Gibbs; Morton's and Wyman's work will be entirely superseded by that of the Peabody Museum and the Army Medical Museum. The Archæological Institute of America will throw new light upon the researches of Stephens; and Mr. H. H. Bancroft will make it entirely unnecessary to wade through thousands of pages of ancient Spanish literature. Therefore the first and most beneficial result of modern anthropology has been the direction of an immense amount of rambling and disorganized labor into systematic and rational employment. This clearing of rubbish, correction of misconceptions, cultivation of a modest spirit, willingness to abide the result, multiplication of materials, refinement of instruments, improvement of processes, in a study which thousands are determined to pursue, must strike every thinking person as a wonderful reformation.

Secondly, the value of a study must be estimated by its effects upon human weal. Farmers, miners, fishermen, lumbermen, mechanics, are slow to recognize their debts to the man of science. But who can estimate the millions of dollars saved by such studies as those of Packard, Riley, and Thomas, on the grasshopper, potato-beetle, and army and cotton worms, and the confidence engendered by the belief that a knowledge of the habits of these animals would lead to their conquest? It would take but a few moments to show that this argument applies with manifold force to the study of man himself.

It is not enough for the good physician to know the nature of remedies, or the use of knives and diagnostic apparatus. Sad will be his use of these if he has not familiarized himself with the structure of the human body in health and in disease, and, above all, if he has not made a correct diagnosis of his patient's case. Are not all the questions asked in the first part of this discourse, and many others agitated by anthropologists, connected with human welfare? Do they not relate to the body, mind, and speech of man, to the races of mankind, their arts, amusements, social needs, political organizations, religion, and dispersion over the earth? For instance, the French in Africa, the British in India, and our own citizens in malarious and fever-laden regions,—have they not learned from loss of treasure, ruined health, and the shadow of death, that there is a law of nature which cannot be transgressed with impunity?

It is the same with sociology and religion. The pages of history glow with the narratives of crusades against alleged wrongs, which were in reality campaigns against the sacred laws of nature. Social systems, which had required centuries to crystallize, have been shattered in the effort to bend them to some new order of things. Arts and industries planted in uncongenial soil, at great expense, have brought ruin upon their patrons, who had not studied the intricate laws of environment.

What a modification of temper, for instance, has been wrought among Indo-Germanic peoples by those studies in comparative philology which have led them by the hand back to their priscan home, and demonstrated, that though they may have aggregated into antagonistic nationalities, and fostered inimical industries, the same blood courses through their veins!

The better knowledge of race and race peculiarities has revolutionized and humanized the theories of aborigines. The doctrine of extermination, formerly thought to be the only legitimate result of colonization, has become as odious as it is illogical.

The inductive study of mind has hardly begun; but how much more successfully and rapidly will education and the development of the species progress when the teacher and the legislator can proceed at once from diagnosis to safe prescription, when natural selection and human legislation shall coöperate in the more speedy survival of the fittest! The time seems to me to have arrived when our great anthropological societies and institutions should institute a systematic, coöperative study of psychology.

In a land where the archæologist may tally off most of his finds by savage implements in use at his very door, it seems like presumption to speak to you of the advantages of the most careful archæological methods. But there is a difference between the old and the new archæology. There are times in the settlement of a new country, when every man is his own carpenter, smith and physician. But how soon your energies have worked out of that! Now I speak only of professional archæology and its advantages. How many errors of his predecessors has Mr. Putnam alone corrected? We have all read with pleasure his recent correction of Dr. Hildreth's mistakes about iron in the mounds. It is so with your archæological collections: only those gathered in a scientific spirit will have any lasting value. But in the accumulation and preservation of such, you are the storers of force of the greatest value. You are recovering the scattered fragments of an ancient mosaic which will one day be reset, and its legend will be the lost history of prehistoric man.

The third benefit to which I will call your attention is the opportunity which the science affords for the exercise of every talent, even the highest. The difficulty of any problem depends upon the number or the degree of its unknown quantities. When facts were few, and the data of our science were beclouded with many sources of error, no wonder that men of logical minds left these investigations to those of a more imaginative disposition. Their crude, preliminary efforts have given place to organized work, directed by men of the greatest executive ability, assisted by skilful specialists, and endowed both by private munificence and by public appropriation. Not to go beyond the limits of our own country, we all point with pride to the Peabody Museum, the Archæological Institute of America, the American Antiquarian Society, the museums of New York city and of Philadelphia, the Smithsonian Institution, National Museum, Bureau of Ethnology, Army Medical Museum, and the Anthropological Society at Washington, the academies of Cincinnati, St. Louis, and Davenport, and the historical societies of many of our states, including the Minnesota collections.

Now, the special merit of such great centralization of resources is that everybody can study something. It is possible for every craft and profession thus to prosecute its researches and to make its contributions. During the past winter, papers were read before the Anthropological Society at Washington by comparative anatomists, biologists, archæologists, geologists, physicians, paleographers, sign-linguists, philologists, patent-examiners, artists, statisticians, sociologists, clergymen, metaphysicians, and ethnographers. And this does not exhaust the scheme. Mothers, school-teachers, those in charge of the insane, of the criminal, and of the defective classes, lawyers, mechanics, musicians, philanthropists, legislators, may all contribute to this science some handiwork which will help to make the pile complete. To be still more personal, permit me to say to each one before me, that there is anthropological work which your peculiar occupation fits you to do better than any one else on earth. For example, a distinguished ornithologist, Mr. Henshaw, has recently identified all the birds in the well-known mound-pipes. An artist, Mr. Holmes, has succeeded in bringing order out of confusion in the shell ornaments of the mounds. A patent-examiner, Mr. Seely, traces backward aboriginal art. A general in the British army, Pitt-Rivers, worked out the history of the elaboration of the implements of war. An educator, Mr. Peckham, has recently given us the result of a laborious investigation on the growth of children. The geologists must interpret for us the significance of our discoveries in the drift. Where can I stop? I will boldly avow that the day of tyros is gone. There is a great multitude of collectors throughout our states who will have to go to school to Professor Putnam, or Dr. Rau, or Dr. Thomas, before they will have the faintest conception of the significance of their treasures.

The inevitable result of special research is generalization. Kepler, Newton, Count Rumford, Kirchhoff, Bunsen and Darwin, are names that stand for these processes in material science. To Herbert Spencer we are indebted for the first effort in this direction respecting human phenomena, and his work will be revised and corrected by those who will approach the task with better instruments and more reliable material.

In this heaving mass of humanity, returning into itself ever with vast gulf-streams and eddies, each actuated by its special forces, there is, after all, orderly motion. We discover that our little circle is part of a greater circle, and for a moment the mind is satisfied in the contemplation of this wider truth. Recovering, and renewing our investigation, the fact is reached, that this and its congeneric circles are part of a greater movement more complicated and perplexing. By the pursuit of this wider knowledge the intellect is strengthened, and thereby is brought about the natural selection of the mind. While many tire, or are unable to comprehend the situation, others press on, and grow strong by the effort.

The last advantage of which my time will allow me to speak, is the assistance which such studies render to philanthropy and legislation.

Standing on the deck of a steamer, and looking at the land left behind, we seem to be but a mile or two away. We are surprised with the information, that what seems so near is many miles distant. It is so with human history. In our childhood we believed that the first man walked the earth only a few centuries ago. All the events known to us then could easily have occurred in that brief period. The increase of knowledge expands the boundaries of time, and the origin of man is now lost in the mists of the past. Could anything fill our minds with greater love for our race than the magnificent struggle they have made in these millenniums? At the other end of the journey we were little better than brutes, and now we look out upon the cosmos as something reasonably comprehended.

If "pity for a horse o'er-driven" fills the heart of the poet, with what tenderness should we look upon the savage races, and remember that the whole family of man has stopped, some time or other, at that way-side inn! Each aberrant form, abnormality, criminal, dwarf and giant, shows the by-paths of human growth into which our life-stuff may have wandered. The arrow is the parent of the cannon-ball; the stone or bone spear-point, of the bayonet; the flint chip, of all edged tools; the cave-man, of the French *savant;* the hut, of the palace; the tattoo, of regalia; the gorget, of the crown jewels; the quipu and pictograph, of the printed book; promiscuous concubinage, of holy wedlock; the hunting-party of society; the clan, of the state; the fetich, of the pantheon; and universal animism, of universal causation. Instead of our ancestral belief in a tree with roots in the earth and branches in heaven, our tree has its roots in the past, and is ever putting forth leaves and flowers in a brighter present.

All sciences are retrospective. The astronomer, the physicist, the biologist, find the bases of their prophecies in the past history of the universe. The statesman, if he be wise, will imitate their example, and feel secure of his legislation for the future only so far as it is founded upon an intimate knowledge of the past.

The value of this study to philanthropy is easily shown. With what admiration do we read of the devotion of those missionaries who have suffered the loss of all things in their propagandist zeal. Science has her missionaries as well as religion, and the scientific study of peoples has notably modified the methods of the Christian missionary. The conviction that savage races are in possession of our family records, that they are our elder kindred, wrinkled and weatherbeaten mayhap, but yet worthy of our highest respect, has revolutionized men's thoughts and feelings respecting them. The Bureau of Ethnology has its missionaries among many of the tribes in our domain, no longer bent on their destruction, but treating them with the greatest consideration, in order to win their confidence, to get down to their level, to think their thoughts, to charm from them the sibylline secrets. It sounds something like the old Jesuit relations, to hear of Mr. Cushing at Zuñi, eating vile food, wearing savage costume, worshipping nature-gods, subjecting himself to long fasting and vigils, committing to memory dreary rituals, standing between disarmed Indians and their white enemies on every hand, in order to save their contributions to the early history of mankind. You will recall the fact, that an honorable senator more than a year ago offered, as an argument against sudden disruption of tribal affinities, an elaborate scheme of the Wyandotte confederacy. Max Müller says, "He who knows little of those who preceded him is likely to care little for those who come after. Life would be to him a chain of sand, while it ought to be a kind of electric chain that makes our hearts vibrate with the most ancient thoughts of the past as well as the most distant hope of the future."

In the study of this anthropo-cosmos, as in other studies, we are brought face to face with the inscrutable. In these voyages of discovery we have no right to expect that we shall ever find a passage to the ultimate truth. As with the child, so with the man;

as with the individual, so with the race; as in the past, so in the present and the future,— the solution of one problem only prepares the way for many far more complicated. With all our sciences comes the consciousness of new ignorances. There is more known to be unknown now than when wise men knew that they did not understand many things well known to us. So will it ever be. Just about one hundred years ago, Peter Camper's measurements of the facial angle, with a few observations on height and weight, were thought to be all that anthropometry could furnish to the natural history of man. In 1881, Paul Broca laid down for the skull and the encephalon more than one hundred and fifty measurements; and the Germans go beyond that. Think you, the weighing and measuring will stop at these? We are just on the threshold of applying experienced training and instruments of precision to the study of man. Examine, if you please, the circulars for information issued by the old Paris ethnological society, Albert Gallatin, Lepsius, Max Müller, and the Smithsonian Institution, with those published for the Novara expedition, by the British Association, Kaltbrünner, Roberts, the new Paris society, or Major Powell, and you will have ocular evidence of the advance of anthropology.

But there is no Ultima Thule in science. No question propounded to nature will ever be answered. I can imagine the night of despair that would settle around any one of my hearers when he had reached the consciousness of having gathered the whole harvest of truth. On the other hand, I am sorry to hear any of our great thinkers uttering the words *ignoramus et ignorabimus* as a wail of despair. They should be to all the sweet voice of hope. They do not mean that we know nothing, or that we shall ever remain totally ignorant. Fresh, vigorous, buoyant, science feels itself to be on a pleasant journey, whose destination may remain unknown, but every mile of whose progress unfolds new vistas of beauty and variety in nature, each transcending the other.

I congratulate you, dear friends, that the American Association has delegated to you such an important trust. The illustrious names to be found among our members and fellows are a sufficient guaranty that you have lighted your torches, and that our science will not be a laggard in this grand march. Professor Henry said, in 1859, "The grand statement cannot be too often repeated, that each branch of knowledge is connected with every other, and that no light can be gained in regard to one which is not reflected upon all" (Smith. Rep., 1859, p. 15). We may go farther, and say, that, whenever any marked generalization is made in any science, all other sciences proceed at once to put themselves in line with the new order. It is the duty of the anthropologists, therefore, not only to rejoice in the growing light of chemistry and biology, but, quickened by their warmth, to put forth new life and vigor, and to apply to their investigations the most refined instrumentalities and the most subtile thought; believing with Lord Lytton "that man is a subject of far nobler contemplation, of far more glowing hope, of a far purer and loftier vein of sentiment, than all the 'floods and fells' in the universe."

PAPERS READ.

A CLASSIFICATION OF THE SCIENCES. By J. W. POWELL of Washington, D. C.

VESTIGES OF GLACIAL MAN IN CENTRAL MINNESOTA. By FRANC E. BABBITT of Little Falls, Morrison county, Minn.

[ABSTRACT.]

DURING the open winter of 1878–79 and the season immediately succeeding it, the writer of this paper gave much time to searches upon certain terrace surfaces of the Mississippi river, for palæolithic remains supposed to exist at the locality under examination. Rudely worked quartzes had previously been discovered here by the state geologist of Minnesota, Prof. N. H. Winchell, by whom they had been described and figured in the state geological report for 1877. The field thus explored lies in central Minnesota, about 100 miles northwest of St. Paul, and within the township and village of Little Falls, Morrison county.

The find reported by Prof. Winchell consists of chipped objects of a class generally ascribed to what is called the rude stone age. Of these many appear to be mere refuse, while others are regarded as finished and unfinished implements. The Winchell specimens have been assigned, upon geological ground, to a prehistoric era antedating that of the mound-building races, and reaching back to a time when the drift material of the terrace-plain was just receiving its final superficial deposit. It is found that, at intervals, the surface soil of the terrace contains these quartzes to a depth of, not unfrequently, three or four feet.

The lowest and newest formation at this place is the present flood-plain of the river. It is still in process of deposition, being yet subject to partial overflows at periods of exceptionally high water. In that portion of the town of Little Falls situated east of the Mississippi, this bottomland is limited on the east by a high ancient river-terrace, which has here an average elevation of about 25 feet above the river at the head of its rapids called Little Falls (of seven feet descent) and has a width ranging from less than a mile up to several miles. This older terrace, like the present flood-plain, has been spread out by the immediate action of water. It consists of stratified gravel and sand, and forms, according to Mr. Warren Upham, assistant on the state geological survey, a part of the modified drift which was deposited at the close of the last glacial epoch of the great ice age, by the floods that descended along this valley from the melting and retreating ice-sheet, being derived from the portion of the drift which had been contained in the lower part of the ice.

While occupied in examining the river bank at Little Falls in quest of wrought quartzes, one day during the season of 1879, I had occasion to ascend a slope lying between the new flood-plain and the older terrace, by a path leading through a sort of gap or notch in the latter (310 rods, very nearly, or almost one mile. north of the east-west road by Vasaly's hotel; 10 rods west of the road to Belle Prairie; and 38 rods from the river). For the purpose of more pronounced distinction I will denominate the lower and higher levels under consideration as bottomland and terrace-plain, respectively. It seemed that at some past period a cut had been effected here by drainage, and that the washout thus formed had afterward been deepened by being used now and then as a wagon track. In this notch I discovered the soil to be thickly strewn with pieces of sharp, opaque quartz. These were commonly of a white color, and ranged in size from minute fragments to bits as large as a man's hand, and in some instances even larger. There were many hundreds of these chips visible, scattered over an area the width of the wagon road, and ten or fifteen yards in length. They were conspicuously unwaterworn, and likewise mostly unweathered, though occasionally a bit was picked up having some one of its surfaces weathered, while fractured or wrought faces, appearing upon other parts of it, looked as fresh as if the work of yesterday. On the other hand, the mass of stone rubbish upon and among which the quartzes were strewn is much waterworn, many of the pieces being well rounded, while none of them are wholly angular.

By continued observations at this locality, I found that many of these quartz chips were brought to light at every succeeding freshet of the season, being washed out of the sand by descending drainage. Their immense and continually increasing numbers seemed to warrant the belief that they had resulted from systematic operations of some sort, once conducted, for unknown purposes, upon this particular spot. A portion of the studied specimens subsequently yielded evidence of having received shape from human hands, and therefore it was assumed provisionally that the site of exposure represented a prehistoric workshop.

Prolonged investigation ensued; and investigation established the hitherto unsuspected fact that no quartz chips or fragments were enclosed in the upper part of the gravel and sand terrace at the notch, nor within a considerable distance at either hand, though they were sought with careful scrutiny. Since November, 1879, at which time the studies here recorded were in active progress, the wagon track before mentioned has been converted to public use as a highway, and has been repeatedly graded in consequence. As might be conjectured, the ground has been much dug over, the stratum being at present almost broken up and destroyed, if not wholly so. During three successive seasons the road has been in a state of almost constant repair, owing to its being continually used for the heavy work of dragging logs from the bottomland to the top of the terrace-plain. From this cause the topography of the spot has been materially modified of late, while quartz chips from the bed of the notch, as likewise from the stratum at first undiscovered, have been much mixed up with the surrounding soil. Primarily, however, as I permit myself to reiterate by way of emphasis, I could not, with a great deal of painstaking, find any superficially inhumed quartz fragments at the point of observation, although, as shown by Prof. Winchell, and as I have myself had abundant occasion to observe, such quartz-chippings occur in the surface stratum of the terrace-plain at various isolated localities in the vicinity. So far as I have been able to judge, these appear in groups which are surrounded by considerable areas containing none. The absence of chips above a plane a foot or two superior to that of their bed at the bottom of the notch, was, of course, a marked circumstance, since they should have occurred plentifully in the superficial stratum, had the notch chips been of the same age with Prof. Winchell's find.

Ultimately it was ascertained that the notch quartzes had dropped to the level at which they were seen from a thin layer of them once lying from ten inches to two feet above it, and subsequently broken up through the wearing away of the sand underneath by drainage. This layer or stratum was still intact on the north and south and partially so on the east, in which direction it had, however, at certain points, suffered some displacement by wagoning. It extended in a nearly horizontal plane into the terrace, in the sloping edge of which the notch, opening into its west bank and truncated at its edge, is cut. The eroded gap in this stratum was oblong in shape. Its outlines, lying within the slopes of the notch, were masked from ordinary observation by thin slides of soil from higher surfaces. Both the inferior and superior planes of the quartz-bearing stratum were sharply defined. Its horizontal boundaries had not been distinctly reached in any direction at the time of excavating for the road, and they have since been partially, and perhaps wholly, obliterated by the grading and digging over to which the spot has been subjected. At the west, the stratum reached to the limit of the terrace, which is bounded by a steep slope. The quartz-bearing layer averaged a few inches only in thickness, varying a little as the included pieces happened to be of smaller or larger size. The contents were commonly closely compacted, so much so that one might sometimes extract hundreds of fragments, many of them very small ones, of course, from an area of considerably less than a square yard.

The quartz bed, so far as examined, rested upon a few inches of sandy soil, which passed downward into a coarse, waterworn gravel, immediately overlying till. Above the quartz chips, stratified gravel and sand extend up to the surface of the terrace. The pebbles of the gravel lying directly on the quartz-bearing stratum, were small and well rounded, and were noticeably less angular than those of the gravel below. The stratum of quartz chips lay at a level some twelve or fifteen feet lower than the plane of the terrace-top.

These observations show that the quartz chips were spread originally upon an ancient surface that has been since covered deeply by the modified drifts which forms the terrace. It will be remembered that the quartz chips and implements discovered by Prof. Winchell in this vicinity are contained in the upper stratum of the terrace-plain; but the notch quartzes do not occur at the terrace-top, and cannot have been derived from it, but are confined strictly to a single stratum of the lower gravels closely overlying the till. Hence the two sets of objects cannot be synchronous, though they may have been produced by the same race at different stages of its existence. The notch quartzes must, of course, be older than those described by Prof. Winchell, by at least the lapse of time required for the deposition of the twelve or fifteen feet of modified drift forming the upper part of the terrace-plain, above the quartz-bearing stratum.

Some of the quartz chips in the stratum have been distinctly arranged upon the floor of deposit in little rudely assorted heaps or groups of objects of particular types. During the summer of 1879, upon the day when the existence of the quartz-bearing stratum was first definitely settled, I collected from it about half a peck of the quartz pieces. Among these appeared a group of specimens numbering a dozen or more, which belonged to a type unknown to me at the time. The most finished implements of this shape have delicate fragile edges, formed by a single thin leaf of the quartz prolonged beyond the mass of the object in a series of irregular minute notches. Further exploration of the site continued up to the present time, has, so far as observed, failed to unearth a single specimen of the same class elsewhere. Whether we assume these small articles to be natural, or artificial productions, or whether, again, we hold them to be natural forms more or less modified by the hand of man, there is no escaping the conviction that the grouping together of these and other like objects is wholly the work of intention. Previous to the discovery here narrated, I had several times chanced upon bunches of specimens of a common figure lying in separate groups in the sand at the edge of the stratum, apparently just washed out of it. Similar assortments of specimens, having a common form, have been brought to light in many portions of the stratum. Indeed, wherever I unearthed any individual object of comparatively large size, it was very often, though by no means invariably, found to be associated closely with others of a kindred type. I have exhumed from this place groups of axe-like quartzes, of rasping stones, of broken implements, of long prong-shaped objects perhaps utilized by primitive man as weapons, and of other rudely fashioned pieces. There were also quartz-fragments of several forms, which were scattered through the whole deposit, such as hammer-stones of different shapes, sharp pieces adaptable as cutting blades, and a great many sharp and long splinters.

These quartzes include many distinct varieties of the mineral, showing marked differences of opaqueness, color, hardness, manner of fracture and admixture of foreign matter. The greater part of them were apparently taken from the quartz-bearing slate in the vicinity. The mode of occurrence of the more unusual specimens is noteworthy and leads to the inference that they may have been fashioned from drift bowlders, numbers of them, indeed, having been evidently formed of waterworn pebbles. Objects shaped from some special variety of quartz not unfrequently present themselves in loose groups, varying in number from two or three to a dozen pieces. Thus a single specimen of a tinted, mottled, or otherwise distinguishable quality, has quite commonly been found associated with a few others of the same kind of quartz. When one of these is of comparatively large size, the remaining objects are, in many cases, proportionally small; as if they were chips from the largest.

A collection of the quartz objects found in the notch at Little Falls was exhibited before the Section, and many of the specimens were considered to be unquestionably the work of man. The collection was afterwards given to the Peabody Museum of American Archæology, and is referred to in the report of the Curator of the Museum for 1883.

ABNORMAL HUMAN SKULLS FROM STONE-GRAVES IN TENNESSEE.

By F. W. PUTNAM of Cambridge, Mass.

[ABSTRACT.]

THE photographs exhibited are views of a brachycephalic[1] skull, taken by me, in 1882, from a stone-grave on Dr. W. H. Jarman's farm near Brentwood, Tennessee. It is that of an adult, probably a man. The coronal and sagittal sutures are simple and all the sutures except the basilar are open. The left parietal is divided into two quadrilateral pieces by a transverse serrated suture, crossing the bone at one-third its height above the squamous suture. The transverse arc formed by the divided left parietal exceeds that of the normal right parietal by one-fourth. The longitudinal arc of the left parietal is also greater than that of the right parietal, and the left side of the skull bulges upwards and outwards. The abnormal suture encloses a pair of Wormian bones (size 3 of Broca's scale). They occur close together a little back of the centre.

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[1] This skull is No. 27205 of Peabody Museum, and has a breadth index of 976.

The back of the skull is much flattened and the sutures of this region are crowded with supernumerary bones. In the right parietal-mastoid are three adjoining Wormian bones of medium size, and there is another of medium size in the right masto-occipital suture. A long oval Wormian bone extends from these along the lambdoid suture to near the median line, where it interlocks with a very long Wormian bone which fills the left half of the lambdoid suture to the upper posterior angle of the lower segment of the left parietal. Between this lower piece of the left parietal and the occipital bone are three large and two medium-sized Wormian bones. A large Wormian bone lies to the right of the lambda between the largest Wormian bone and the right parietal. This cranium has in all several minute, fourteen medium-sized, four large, and two unusually large Wormian bones.

All the teeth are present, and normal, except the two lateral incisors of the upper jaw, which, apparently, were never developed. The gap separating the central incisors and canines is scarcely broader than that between the two central incisors.

Another brachycephalic adult male skull[2] found in 1877 in a burial mound[3] on Mr. Wilkinson's farm, nine miles from Nashville, has the abnormal suture dividing the left parietal. This skull has simple sutures which are open except near the pterion. The extra suture springs from the left arm of the lambdoid suture at the junction of its middle and lower third, and passes nearly horizontally half-way across the parietal. It is then deflected and runs towards the pterion. Posteriorly this suture is finely dentated but anteriorly it becomes simple and is so much obliterated that it is impossible to trace its entire course. A large Wormian bone occurs in the left arm of the lambdoid suture at and above the origin of this anomalous suture.

The posterior inferior angle of the right parietal of this skull is also developed from a separate centre. It is united with the main portion of the bone by a simple suture running from the squamous suture almost horizontally till it approaches the lambdoid when it turns downward and enters this suture at a point opposite the middle of the squamous border of the intercalated bone.

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[2] No. 12797 Peabody Museum. Breadth index, 1012.

[3] This mound contained a number of stone-graves placed in several tiers.

Division of the parietal is a rare anomaly. Of more than two thousand skulls which have passed through my hands, the two here described are the only ones that I recall as having this anomaly of the parietal. Miss C. A. Studley, an assistant in the Peabody Museum, has looked up the literature of the subject for me and finds that Grüber describes one case and records eleven others as known up to 1870, and that in Flower's Osteological Catalogue two crania are mentioned as having a supra-squamosal bone.

A NEW STAND FOR SKULLS, MADE BY EDWARD E. CHICK, ASSISTANT IN THE PEABODY MUSEUM OF AMERICAN ARCHÆOLOGY AND ETHNOLOGY. By F. W. PUTNAM of Cambridge, Mass.

[ABSTRACT.]

VARIOUS methods have been adopted for mounting human crania for exhibition in museum cases, all of which, so far as I have seen, are defective in some one or more particulars. The objects to be secured are the proper exhibition of a skull without injury, and mounting it in a natural position in such a way that it may be placed in the case so as to exhibit it either from the front, back or side, as may be desirable for comparison with other skulls in the case. Further, it is essential that it may be easily removed for special study, and replaced without much trouble when done with for the time being. It is also of the greatest importance to avoid any pressure on the margin of the foranum magnum, on the bones of the palate, or base of the skull, which in crania of considerable antiquity are generally very friable. Another essential thing is to be able to suspend the under jaw in place without making holes for the passage of wires.

After many trials and modifications, all these points have been secured by my assistant, Mr. Chick, and the stand now in use at the Peabody Museum consists of a round wooden base, in the centre of which is screwed a brass rod from three to four inches high, on which a screw thread is cut. On this upright rod a circular brass plate, with a screw thread, is placed at any desired height. On opposite sides of this plate two brass wires are screwed and so curved as to support the occipital portion of the skull. These wires can be easily bent, and their curve can be changed by the fingers so as to fit any particular skull. A third wire is screwed into the front of the plate and projects from three to four inches, ending in a ring. This rod can be easily bent so as to obtain a proper bearing for the ring on some firm part of the palate, or, if need be, when the palate is broken, on the inner margin of the teeth or any other firm point. The pliability of this rod also secures the tilting of the skull into a natural position as it rests on the occipital wires and is steadied by the central rod which passes into the foranum magnum. From the forward rod hangs a small spiral wire holding a transverse piece of wire with the points turned up so as to clasp the under jaw on each side, the strength of the spiral spring being sufficient to hold the jaw in its place and clear of the shelf.

On the stand itself the label can be placed and the skull thus mounted can be turned about at a slight touch as it stands, swinging free from the shelf. In this way all ordinary observations can be made without running any risk in handling the skull. This stand may be adapted also for mounting fragile and broken crania and even portions of crania in a natural position.

THE CHARNAY COLLECTION AT WASHINGTON. By O. T. MASON of Washington, D. C.

[ABSTRACT.]

THE collection referred to contains the material obtained by the Charnay expedition to Mexico and Central America. The expenses were defrayed jointly by Mr. Pierre Lorillard and the French government. The success of the expedition showed how a gentleman of fortune might render valuable service to the cause of science, although not specially conversant with scientific lore.

In this undertaking the point was gained by obtaining, as far as possible, casts (in plaster strengthened with tow) of the objects of antiquity. By means of these casts, the drawings of this and other exploring expeditions can be verified and corrected. Great success was attained, as to casts from little-known and almost inaccessible ruins; and many new objects of beauty and curiosity were brought to light, among them a large number of interesting reliefs and statues. The casts will be preserved in the Museum at Washington, and in duplicate at the Trocadéro Museum at Paris. Numerous photographs and drawings were obtained; and measures have been taken, under the auspices of the Smithsonian Institution, to reproduce several of the more important ruins by correctly arranging the casts in position with suitable accessories. Good success has already been attained, both in making restorations of ancient temples and other ruins, and in correcting recorded measurements and drawings. A full account of the restorations will appear in Professor Baird's annual report for 1883.

OSAGE WAR CUSTOMS. BY J. O. DORSEY of Washington, D. C.

[ABSTRACT.[1]]

BY means of an illustration the preparations were shown which the tribe makes for a war-march, the order followed being that of gentes or clans in the tribe. The paper described the tactics by which the Osages camp in a circle, the war-men on one side, and the non-combatants on the other. The road travelled by the Indians forms the line down through the centre of the camp, and the division-line. The great war-tent is placed with the door to the west, the place of honor being at the east. The author detailed at considerable length, with the aid of illustrations, the method of selecting the forces, and the ceremonials preparatory to war, the decoration of the Cheezho peacemaker, the form of the dance around the village, the nature of the bravery dance, and the order of march from home by twos four abreast. Marriage ceremonials and funeral rites were also described and explained in detail. The author accompanied the paper with drawings illustrating the grouping of participants in ceremonies.

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[1] This paper has been printed in full in the American Naturalist, for Feb., 1884.

OBSERVATIONS ON THE LAWS AND PRIVILEGES OF THE GENS IN INDIAN SOCIETY. By MISS ALICE C. FLETCHER of New York, N. Y.

[ABSTRACT.]

AN elucidation of the customs and circumstances under which the gens system in Indian tribes surpersedes parental ties was the chief feature of this paper. The author had an excellent opportunity for observations of this character, during the work of placing the Omaha tribe of Indians upon their lands in severalty, and while adjusting the line of descent and inheritance according to our laws.

A child who has lost its father or mother is considered an orphan. Its particular place is gone, and it passes into the gens. When the father dies, leaving offspring, the mother loses all maternal rights. Each child, unless of very tender age, will be separated from the mother, and will go into the family of some one of the father's relatives. It may thereafter be claimed as his own child by the male head of the family to which it has been allotted. This separation of her children from a widow is permanent. She usually marries again, and in that event is not burdened with her offspring by a former husband ; but if she should remain unmarried, she would be expected to work for the family that has adopted her children, rather than for the children themselves. If she dies when her children are young, it is probable, that, at maturity, they will have forgotten even her name.

The women are not wanting in affection for the children of whom they are bereft ; but the separation is looked upon as a matter of course, none of the interested parties regarding it as a grievance, or even a hardship. It surprises an Indian to have the propriety of this arrangement questioned. No point of our law of inheritance is so difficult for him to understand as that which binds together the child and the surviving parent.

Young men whose mothers are of the same gens are accounted brothers to each other, and the brothers of the mothers are uncles. Between these uncles and the nephews and nieces there is an easy familiarity, not unlike that of parents and children.

The author has observed a decided lack of family likeness among Indians. This observation applies, however, to entire families, which include cousins, aunts and uncles; a striking resemblance between parents and children being not unusual.

The Indian may be "the stoic of the woods;" but he is neither averse to pleasantry, nor deficient in sensitiveness of a certain kind. He delights in chaffing his fellow Indian; and dreads, more than aught else, being made a laughing-stock.

SYMBOLIC EARTH FORMATIONS OF THE WINNEBAGOES. By Miss ALICE C. FLETCHER of New York, N. Y.

[ABSTRACT.]

HAVING mentioned in previous papers[1] the symbolic earth formations which appear in every religious ceremony I have witnessed among the Sioux, I will call attention to similar structures noticed among the Winnebagoes.

This tribe is admitted to be one of the older branches of the great family to which it belongs. Not only is this antiquity shown by the language, but by finding many of the religious ceremonies of different tribes referred to the Winnebagoes, and Winnebago words discovered in the religious ritual of other tribes.

The Winnebago tent used for sacred dances is long and narrow; not more than twenty feet wide and varying from fifty to one hundred feet long.

In the Buffalo dance which is given four times in the month of May and early June, the dancers are four men and a large number of women. As the dancers enter, each woman brings in a handful of fine earth and in this way two mounds are raised in the centre at the east, that is between the eastern entrance and the fire, which is about fifteen feet from the eastern entrance. The mounds thus formed are truncated cones. An old man said to me "That is the way all mounds were built; that is why we build so for the buffalo."

The mounds were about four inches high and not far from eighteen inches in diameter. On the top of the two mounds were placed the head-gear worn by the men, the claws, tails and other articles used by the four leaders, or male dancers.

The men imitate the buffalo in his wild tramping and roaring, and dance with great vigor. They are followed by a long line of gayly-decked women in single file. Each woman as she dances keeps her feet nearly straight and heels close together, and the body is propelled forward by a series of jerks which jars the whole frame, but the general effect on the long, closely packed line is that of the undulating appearance of a vast herd moving.

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[1] Sixteenth Report of the Peabody Museum of Arch. and Ethn., Cambridge, Mass.

The women dance with their eyes turned to the ground, and with their hands hanging closely in front, palms next the person. The track left by their feet is very pretty, being like a close-leaved vine. It is astonishing to notice how each woman can hop into her predecessor's track. Water is partaken of and the entire dance is clearly indicative of the prayer for increase and plenty of buffalo.

The two mounds remind one of larger structures and suggest many speculations, particularly when taken in connection with the manner of their building.

In the great mystery lodge, whence so many of the sacred societies among other tribes professedly take their rise and inspiration, the fire is at the east, and is made by placing four sticks meeting in the centre and the other ends pointing to the four points of the compass. Just at that part of the initiation of the candidate when he is to fall dead to the old life, be covered as with a pall, and then be raised to the new life, the remains of the four fire sticks are taken away and the ashes raised in a sharp conical mound; again suggesting hints of a peculiar past.

Upon the bluffs of the Missouri, on a promontory overlooking a stretch of landscape thirty miles in every direction, is a little depression cut in the ground, circular in form, with an elongated end at the east. The depression is one foot in diameter and about six inches deep. Placing my compass in the centre, the long end or entrance was found to be exactly to the east.

To the south of this sacred spot, for it is cleared and cleaned by secret hands every year, stood a large cedar tree, now partly blown down. This was the sacred tree on which miraculous impersonation of visions lit; and here the spirits tarried as they passed from one resting place to another going over the country. About every fifty miles there is one of these strange, supernatural resting places.

The history of the earth symbols is not yet clearly understood, and it is to be hoped that students will carefully observe these strange relics of customs which were once, perhaps, on a grander scale.

LIFE AMONG THE MOHAWKS IN THE CATHOLIC MISSIONS OF QUEBEC PROVINCE. By MRS. ERMINNIE A. SMITH of Jersey City, N. J.

[ABSTRACT.]

THE almost entire isolation, during more than two hundred years, of these Mohawks from the other Iroquois tribes, renders a study of their dialect, peculiar customs, remarkable habits of industry and prosperity, subjects both interesting and important. The old mission house and church at Caughnawager contain the old Jesuit library, portraits of Charlevoix and Lafitau, various presents from crowned heads in Europe, a portrait of the only aboriginal saint, Ta-gah-qui-ta,—all objects of great interest. The archives of this old church and of the seminary of the Sulpicians at the Lake of the two Mountains are the repositories of old manuscript sermons, dictionaries, grammars, catechisms, etc., some dating back over two hundred years. The dictionary of the Rev. Father Marcoux is one of the most valuable and well preserved of those extant in the Mohawk dialect, not excepting Bruges, etc. Interesting examples of its contents are the names of the months, days of the week, etc. The day is past when it could again be compiled, as the industries of these people are bringing them into so close contact with French and English that their own language is rapidly falling into disuse. The beadwork of the women is sold from Maine to San Francisco. Over ten thousand pounds of beads are kept constantly on hand in the wholesale store of one of the chiefs to supply the never-ceasing demand. The stone quarries are also worked by the chiefs, their owners, and are a great source of profit. But the principal occupation of these Indians is rafting upon the St. Lawrence. Few of the old customs are preserved, although still nominally presided over by chiefs. The clans here outnumber the other branches of the Mohawks by four, the Lark, Rock, Calumet and Dove.

The old wampum belts are still preserved.

The changes which a language can undergo, during a separation of portions of its people for only two centuries, are illustrated by the table prepared of words in use at Caughnawager, and their synonymes in use among the Mohawks of Upper Canada. This list proves conclusively that dialectical differences can be brought about in a comparatively short space of time. The old aboriginal folk-lore stories have been superseded through the influence of the church by those of a religious character, such as the Legend of the old Church bell, etc.

The other mission referred to was removed in 1717 from Montreal to the Lake of the two Mountains, where the Order of Sulpicians have a grant of seventeen miles of land and provide for the moral and religious education of the Indians of their mission. A convent of the *Congregation de Notre Dame* has charge of the school for Indian girls.

From these archives a large number of titles were secured for the bibliographer of Indian linguistics at Washington.

STUDIES IN THE IROQUOIS CONCERNING THE VERB TO BE AND ITS SUBSTITUTES. By Mrs. ERMINNIE A. SMITH of Jersey City, N. J.

[ABSTRACT.]

THE general reader upon the structure of Indian languages and the student who would attempt their investigation are sure to encounter as a conceded fact, established by the pioneers in Indianology that "the verb 'to be' does not exist in these languages."

And when we remember that it is in all languages made up of several radically differing roots the above admission would seem correct.

Notwithstanding this, the conscientious student is staggered at encountering first in adjectives, and later in many verbs and in the dissection of nearly all nouns, the fact that in their translation into English the use of the verb "to be" is imperative.

Among the distinguished philologists who have expressed themselves upon this subject we mention Major Powell who in his "Introduction to the study of Indian languages" says "In most Indian languages there is no verb "to be" used as predicant or copula." Dr. Riggs in reference to Dakota verbs also remarks "We cannot now say more concerning verbs unless we add this interesting fact that as is the case in other American languages there is no pure substantive verb in Dakota.

We can express the idea of existence in connection with ideas of place or condition, but not alone. Rev. Father Cuoq in his "Lexique de la langue Iroquoise" gives as the translation of the verb "*etre*, to be," the Mohawk verb ke-nă′-ke-re which means literally *I inhabit* and is generally applied to animals.

The unknown author of the old Onondaga Dictionary translates it as Ki′-te-roñ which literally translated is, *I am seated*, *I am placed*, *etc.*

Father Marcoux, who is undoubtedly the best authority upon any of the dialects of the Iroquois says, "The verb *to be*, which according to Levizaz is the only true verb, does not exist isolated in this language, but is found contained in nouns, pronouns, and adjectives. It is expressed in different manners when it signifies the *essentiam simpliem in loco.*"

It is rendered thus :—

1. Ke-nă′-ke-re, *I inhabit;* applied to animals, fish, birds, and snakes it is translated by "to swarm," "to be plentiful," as *a.* Ok′-si hă-nă′-ke-re ne′ Ră-weñ-ni′-yo ; literally, *always he inhabits the God free.* "From the beginning God is." *b.* Keñ-tcă-nă′-ke-re, *fish swarm*, are plentiful.
2. Koñ′-he^{e}, *I live*, I survive.
3. Ki′-te-roñ, *I abide*, dwell, am seated, am placed ; -ta-koñ a suffix and shows the cause of, means of, "dwelling, etc."
4. Kă′-yeñ, *it lies*, as kă-ni kă-nă-tă-yeñ, *where it-lies-the-city?*
5. I-kă′-le, *it is present.*
6. Yă-oñ-te, *it has;* said of conditions arising from original design or purpose as Kă-nă′-toñ-te, *it has a room* (affixed to it) ; ră-yă′-toñ-te, *he has a body.*
7. Kă′-te, *it projects*, is arrayed ; as Kă-roñ′-hyă-te, *a cloud projects*, is visible ; in this it literally represents *ex-ist.*
8. Kă-hnyo′-te (a compound word, from "7th" and o-hnyo, *a bow*), literally *a bow is present*, *i. e.*, the rainbow.
9. A-koñ′-hă, *I*, *alone.*
10. Te-kyă′-se, *I am doubled*, twice myself.
11. Keñ, or ne′-keñ, *that*, or that which.
12. I-wăt, *it is in*, it lies in ; sañ-ni-wăt, *how much is in?*
13. Wă′-toñs, *it becomes*, turns into—?
14. A-kwă′-weñ, *ours.*
15. Yo-weñh′-te, *it is joined*, is joined with $\left.\begin{matrix}\text{are}\\ \text{is}\end{matrix}\right\}$ many.

16. Yă′-oh it, *is in solution*, is in; as, Kă-hne′-ko^{c}, *water is in*, *i. e.*, there are springs; Kă-ris′-to^{c}, *iron is in*, iron-mine; Yo-hwis′-to^{c}, *a silver*, money-mine.

The Rev. Father LaCombe in his "Dictionnaire de la langue Crise," remarks in his introduction, "There are no auxiliaries *to be* and *to have* as in French, but they are replaced by post-fixes, which have the same value in making the verb."

As an interesting study, however, the following table was prepared containing mainly adjectives which in their conjugations are said to include the verb *to be*, and which are untranslatable into English without its use. This was for the purpose of separating from the *conjugated adjective* any particle representing the verb *to be*.

Hoñ′-tci, black.
K-hoñ′-tci, I am black.
S-hoñ′-tci, thou art black.
Ră-hoñ′-tci, he is black.
Yä-hoñ-tci, she is black.
Kă-hoñ′-tci, it is black.

In this conjugation, if a separable verb *to be* exists, it must be sought for in the pronouns often represented by a single letter.

The first person, present tense, of another conjugation, is as follows:

Wă-e′-tsă-nit, courageous.
Wă-a-te-wek, affable. } adjectives pure.
Wă-i-tent, poor.

Conjugated.

Wă-ke′-tsa^{c}-nit, I am courageous.
Wă-kă′-te-wek, I am affable.
Wă-ki′-teñt, I am poor.
"K" in this representing "I am."

If these languages were as well understood as the Latin, might they not possibly be found to contain a letter representing both verb and pronoun, as in the imperative of the verb io, *I go* (Latin) where "i" represents go thou?

The above examples are given to show that the *present tense* of the verb *to be*, if such exists, must be contained with the pronoun in the letter "K."

Pushing these studies farther, if one would wish to fill out the other tenses the *tense endings* must be suffixed. Taking "hăk" (the sign of past time) and suffixing it to "K" we have K-hăk′, *I was.*

But one encounters forms which must be considered irregular as in Kuñ-kwă^c-stă-kwăs-nu′-hä^c. *I-was-a-good-person.* In this example, K-nu^c-hä^c would represent *I was*, and so on through other tenses which forms however, standing thus connected, would be quite meaningless.

A study of these tense endings is at least not useless for they teach most conclusively that these languages are very old, and show that their verbs have incorporated as tense signs highly abbreviated words whose root significations have evidently been long lost. Such for example is hä^c as:—wă-kit-us, *I am asleep*, wă-kit-u-hä^c, *I am about to sleep;* "hä^c" here converts it into future time.

Again: wă-kă-rhä-rhu, *I am running;* wă-kă-rhä-rhu-hä^c, *I was running*, ran; "hä^c" here converts it into past time.

These two tense signs "hä^c" are probably from different roots, one of which meant *will* and the other *did* or *was.*

In this search we have found possible roots with which to build up the neuter verb "be," even four not more radically different than *be*, *am*, *is*, *was*, of the English.

We believe that this is, if any, the only direction in which to seek out this hidden verb *to be*, and that if it exists, it is concealed in the obsolete verbal forms containing the root significations of said tense signs.

These forms might even in themselves prove to be the missing neuter verb.

But more profitable fields of inquiry, yielding more direct results, are awaiting the harvester and *to be* or *no to be* must still continue a question.

ACCIDENTS OR MODE SIGNS OF VERBS IN THE IROQUOIS DIALECTS.

By MRS. ERMINNIE A. SMITH of Jersey City, N. J.

[ABSTRACT.]

1. MOVEMENT in these dialects which in French is expressed by the auxiliary verbs *aller* and *venir*, and corresponds to the English participle in *ing* and the adverbs *toward* and *about*, is represented in

Iroquois by final particles incorporated in the verbs making them verbs of movement. These might be termed mode and tense signs as the particles take the final inflections proper to the different time, as follows — Kninons, *to buy;* Kni-nonres, I *am about* to buy, etc.

2. Reduplication is the act of the verb repeated as repay, reconsider, etc. The mark of reduplication in several of the dialects is *S*, but in the Tuscarora Ç; as Kirh-hă, I *drink:* Ç Kirh-hă, I *drink again.* After the same manner can be explained affirmation, locality, causality, composition, frequentation, final relation, transition, habitude, resolution into contraries, partial reflexion, their change into substantives, etc., which furnish the material for the excessively long words to be found in all Indian dialects.

THE GREAT MOUNDS OF CAHOKIA. By WILLIAM MCADAMS of Alton, Ill.

[ABSTRACT.]

THE mounds referred to are in the locality known as the "American Bottom." The region so called is a strip of alluvial land in the state of Illinois, lying between the bluff and the Mississippi river, and extending from the city of Alton to a point below the city of East St. Louis. A map of the locality, showing the places and dimensions of the mounds, was exhibited before the section. The mounds are over two hundred in number, and embrace some of the largest in the United States. A group of seventy-two mounds on the Cahokia creek was specially considered. The central mound of the group is the largest: it is about a hundred feet high, and covers over twelve acres of ground. It is a truncated pyramid with terraces: its flat top has an area of one and a half acres. The surrounding mounds are thirty to forty feet high and most of them are square, in this respect differing from the conical mounds of Ohio. The mounds on the bluff seem to be of a different order, being only four or five feet high, and round or oval. Unquestionably the mounds of the Cahokia valley are artificial, being made of black alluvial earth, entirely different from the ground on which they rest.

The author accounted for the fact that there were few mounds on the banks of the Mississippi river, by supposing that the mound-builders were afraid of their enemies beyond the stream.

Numbers of relics have been found in the Cahokia mounds, mostly of flint, some of them eighteen inches long. The finest is a white flint axe, which is of a smoothness and polish like ivory. In reply to an inquiry, the author stated that there had been considerable alluvial deposit formed since the mounds were built. The subsoil is a yellow clay loam: under the mounds is a floor of white sand.

ALTARS AND HIGH PLACES[1] AMONG THE EMBLEMATIC MOUNDS. By S. D. PEET of Clinton, Wis.

[ABSTRACT.]

THE significance of the term "High Places" will become apparent when it is once considered that the custom referred to in the Scriptures, was peculiar to those tribes in the east which were in a primitive condition, and whose religious cultus was at a low stage. They were either the worshippers of the sun and moon and heavenly bodies, or in the still lower condition of animal worship, and their customs may have been quite similar to those which are found to have prevailed among the so-called animal tribes of America. The term "animal tribes" expresses much, especially when we consider that tribes bearing animal names, and having their religion closely associated with animal totems, were common at the East in Syria and Arabia, as well as in Wisconsin and other parts of this continent. The religious customs of these tribes have not been so well known as of those which were more advanced; animal worship having preceded sun worship and being peculiar to prehistoric times. Whether the custom of erecting altars on "high places" can in the East be traced back from sun worship to a more primitive cultus is a question. Here, however, they may be, and the subject becomes the more important from this fact.

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[1] The title of this paper originally had no reference to the "High Places" which are described in the sacred Scriptures. The term was used to designate a certain class of mounds which had come under the observation of the writer. The use of it, however, seemed to give rise, at the time of the reading of the paper, to the idea that the Scripture custom was held in mind while the descriptions were given. This criticism is accepted, but the suggestiveness of the title is made the more apparent from it.

There is one other point to which we call attention in connection with this hint at oriental customs. Human sacrifices were common in the East, and it has been found that these were also common among the primitive tribes of this country. It is an inquiry which we would make at the outset whether the altars and high places, of which we are to speak in this paper, were not places for cremation. Human sacrifices were common among the primitive races. Victims were seized upon and offered to cruel divinities. Such a custom was common among the tribes of Central America. There are cases where human bones have been found associated with altars in mounds. Yet we maintain that these altars and high places were used for the rites of cremation rather than of human sacrifice. There may be a danger of confounding human sacrifice with cremation, but, if we should find that ordinary burial mounds and the altars and high places were only designed for the different modes of disposing of the dead, we should consider that a satisfactory explanation of these altars had been reached. We bring out the facts contained in this paper with this distinction in mind desiring that our readers should consider that the sacrifices which were offered on the altars and high places were not true sacrifices, but cremations.

The custom of burying on high places was common with the native tribes in America as well as with the primitive races throughout the northern portion of Europe, in Great Britain, France, Germany and elsewhere. Nearly all the burial mounds throughout the Mississippi valley are in prominent localities on the summits of bluffs overlooking valleys, and in just such positions as are described as common for the cists and cairns of England and Europe. The explorations of certain gentlemen at Muscatine, Iowa, illustrate this point.[2]

Here, burial mounds are seen located on the bluffs on either side of the Mississippi river and extending for miles along the bluffs. The locality was favorable for residence, as the bluffs surround the limits of an ancient lake where there would be an abundance ot game and where the rich bottomlands furnished soil favorable for the cultivation of maize, and for a variety of products. This custom of erecting burial places on bluffs and high places has given rise to the idea that there may have been a connection between the residences and the burial places. The Aryan custom was to have the family hearth and the burial place in close proximity, the Lares and the Penates being affiliated and their tokens associated. With these races also it was common to connect the sacrifice and worship offered to their divinities with their hearths; their tombs, temples and houses being closely associated. We throw out the suggestion whether some such custom did not exist also among the so-called Turanian races. Isaac Taylor maintains that these races were distinguished for their burial customs, though he does not define and describe, except in a very general way, what these customs were.

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[2] See American Antiquarian, Vol. III.

A few words on the connection between the altars in high places and ordinary tumuli may next be in place. There seems to have been a custom among the primitive races, of erecting tumuli or burial mounds on the hill tops, and the only difference between these burial places and the so-called altar mounds is in the fact, that, in the former, bodies were buried without the use of altars, while in the latter altars have been found, and with them various tokens of cremation. At times burial mounds contain tokens of burial at their base, and then at their summit other tokens which show that they were used for beacons or signal stations. This double use of the tumuli is very common. The writer has had his attention called to it in several places in Wisconsin. The custom oftener was associated with conical mounds, and yet effigy mounds have been discovered with evidence that burials were practised in connection with beacons and outlooks. The location of tumuli on high places probably led to the double use, as it would be convenient to light beacons on a mound already in existence. The same double use has also been noticed in connection with the altar mounds and their erection on high places may have been for the same purpose.

The custom of thus erecting the burial mound was common with the emblematic mound builders. There are many localities where burial mounds are so placed that extensive views are gained from them. Sometimes these views extend for many miles. There are mounds on Lapham's Peak, a prominent point in Washington county, from which a view can be had for twenty miles distant. The same is true of a line of mounds near Muscoda on the Wisconsin river. Burials on high places seem to have been common among the emblematic mounds. In reference to the sacrificial mounds this peculiarity of being erected on high places forms a point of resemblance to burial mounds, and so we might be led to consider the altars as designed for cremation rather than for sacrifice.

That cremation or sacrifice was practised mainly on high places I think may be shown. If there is need of proof of this point we might refer to the many sacrificial altars which have been exhumed in various parts of the country. Any one who will read the volume by Squier and Davis in the Smithsonian contributions[3] will be convinced that such High Places were common. The altars described by these authors seem mainly to have been associated with burial places, as many of the mounds which contain the altars contain also heaps, where evidently the ashes and remains of cremated bodies were deposited.

We will not say that sacrificing on *high places* was universal, for there are exceptions to it. Prof. F. W. Putnam has called the attention of the author to several cases which have come under his notice in Ohio, where altar-mounds were discovered on comparatively low ground. Yet we maintain that as a rule both burial-mounds and altar-mounds were placed on high ground.

They are generally placed on high bluffs or points of land, and are also frequently associated with village enclosures and with watercourses. The sacrificial altar at Paint creek, near Hopeton, had this peculiarity, that it was on the opposite side of the river from the village enclosure, giving rise to the thought that possibly there was the same custom of crossing the water to reach the places of cremation as existed among various races in the East.

Mounds have also been found by the writer in Wisconsin, which were evidently placed on bluffs overlooking village sites, which have proved to be altar-mounds, and it is a remarkable fact that nearly all of these have been on the opposite side of a river or lake from the village sites. This may have been a mere coincidence, but it has been found to be so frequently true, that it has given rise to the conclusion.

We are, however, to consider the distinction between altars and ordinary tumuli or burial-mounds.

Altars or sacrificial places were generally surrounded by enclosures. These enclosures were not always circumvallations, yet nevertheless served the purpose; but burial mounds were rarely surrounded by enclosures. The cases referred to above at Hopeton, Mound City, Paint Creek and elsewhere were marked by this peculiarity. Here, at Mound City, were a large number of mounds, twenty-four in number, surrounded by an enclosure. The authors Squier and Davis, in describing these, state that the open temples of the ancient Britons were embraced within parapets of earth, and that the sacred places of the Pacific Islands are surrounded, if not by earthen, by stone walls, or palisades. All the mounds in the enclosures were excavated by Squier and Davis and proved to contain altars. Another enclosure is described where a single mound was found, and this contained within itself an altar or place of sacrifice. Still another enclosure, with altar mounds, was visited by these authors at a place called Clark's Works, and contained seven mounds of great size, thirty feet high, one hundred and eighty broad at the base, and together forming a line five hundred feet long. A description of the altars is given in a separate chapter and is worthy of study.[4]

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[3] Smithsonian Contribution, Vol. I, p. 54, plate XIX.

Among the emblematic mounds there are sacrificial altars which were erected within enclosures. These enclosures, like all others in this region, are formed by the effigies being grouped around an open space. The effigies do not form a connected wall, and yet they make an enclosure which is sufficiently guarded to give rise to the idea of the sacredness of the spot.

The enclosures containing altar-mounds are generally smaller in their area than village enclosures, and the altars are generally placed in the centre of the open space, making the distinction between the two classes of works manifest. Several such enclosures have been described by Dr. I. A. Lapham,[5] though their purpose was not apprehended by him.

A similar enclosure was also described by Squier and Davis. This was discovered by Mr. S. Taylor, and is called by him "The Citadel." "The figures comprising these are so arranged as to constitute a sort of enclosure of about half an acre area.[6] The ground is here prominent. From the top of the principal mound occupying the central group may be seen at least a hundred elevations forming the boundaries of the Citadel." We give an illustration of this group (fig. 1).

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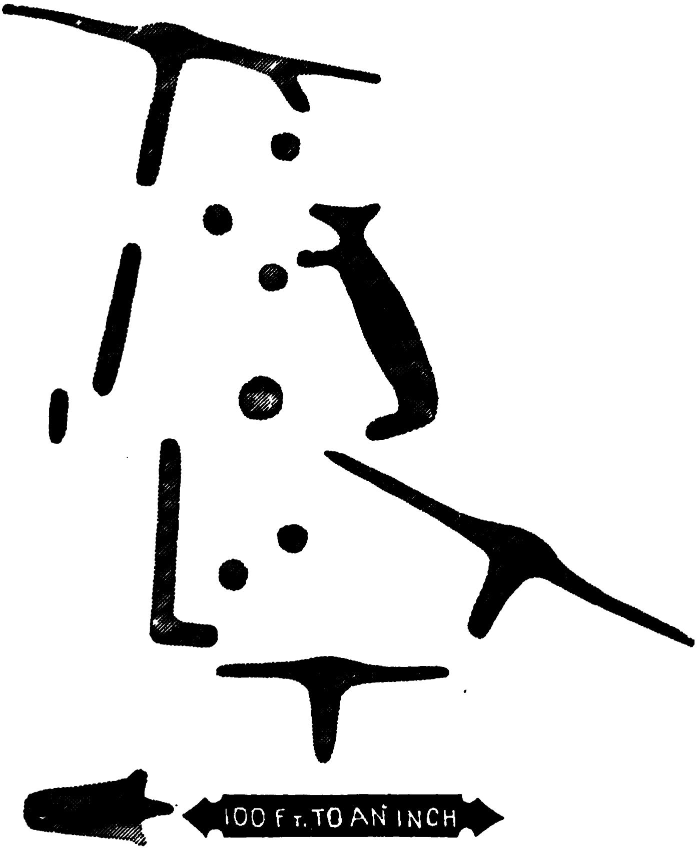
[4] See Smithsonian Contributions, Vol. I. p. 143.

[5] See Lapham's Antiquities, plate 40, plate 11, plate 10.

[6] See Smithsonian Contributions, Vol. 1.

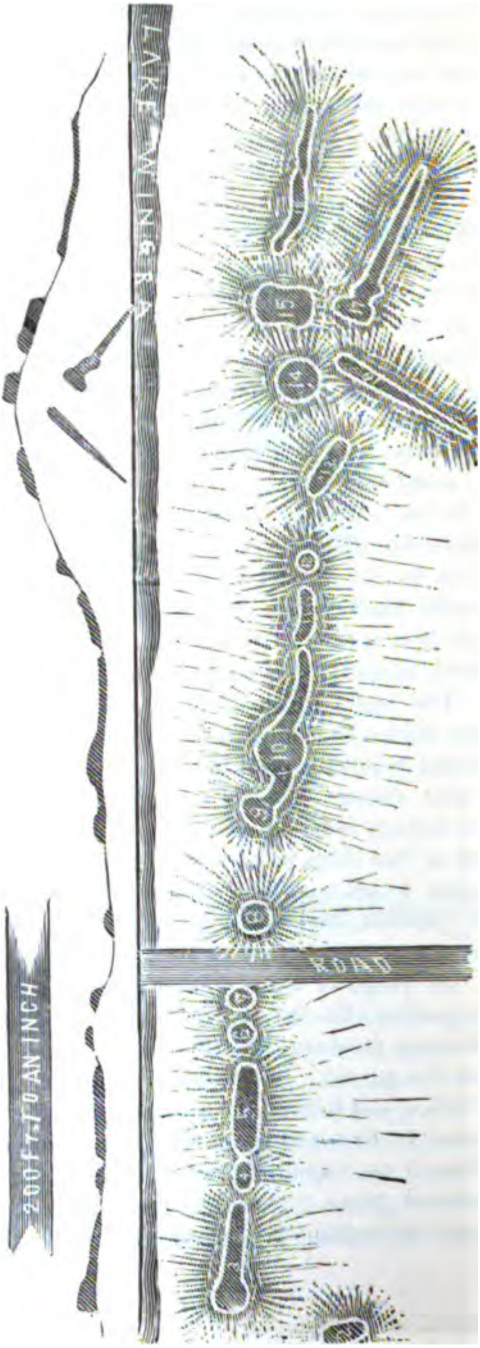
FIG. 1.



Sacrificial place at Muscoda. S. Taylor.

Another group of mounds, which was designed probably for a sacrificial place, has been observed by the writer near Lake Koshkonong, and a figure of this is also given (fig. 2). This group, like the others referred to above, is composed of a series of effigies surrounding a central space within which are two mounds, one a beacon-mound, No. 4, and the other an altar-mound, No. 3. These effigies surround the altar forming a sort of guard to it. These effigies have the shape of familiar animals; a turtle, No. 1,

FIG. 3.



Altar-mound on Lake Wingra, near Madison, Wis.

tail reaching to the very foot of the hill, its huge body lifted high up on the summit on the ridge, and its head situated at but a short distance from the altar. The long, tapering mound follows the outline of the ridge in another direction, the three effigies forming a sort of guard or enclosure for the central group. The relative position of the mounds indicates that there was a design in the mind of the builders to make them serve three uses, namely: to guard the altar, to show the shape of the ground, and to give the shape of the totem effigies.

In the centre of the group is a signal-mound or beacon. This is situated on the highest point of land and commands a view of three of the beautiful lakes which surround the locality: Lake Monona to the east, Lake Wingra to the west, and Lake Mendota to the north. Another lake is situated but a mile or two to the south, but cannot be seen as high land intervenes. The point, however, is central, and is surrounded by an amphitheatre of hills and seems to have been the centre of a large number of earthworks and effigy mounds. There are emblematic mounds on all the surrounding hills. The Capitol occupies the site of mounds formerly existing on that hill. The University is surrounded by effigies which were formerly more distinct on that hill. The Asylum, across Lake Mendota, four miles distant, is now surrounded by a large number of striking effigies. The stone quarry between this ridge and the university is surrounded by a large number of effigies. Greenbush, within gunshot, has another group of effigy mounds. The Home of the Sisters of Charity, across Lake Wingra to the west, has another series of effigies. The dairy farm and fair grounds, between the Charity Asylum and the University, have another group. Back of these, on the summit of the high bluffs to the west, is the cemetery, where there is another group. To the east, there are effigies on the hills between this ridge and the assembly grounds at Lakeside. On the opposite bank of Lake Monona, three or four miles distant, is the shooting park; here is another group. Still farther east in Mill's Woods are many other striking effigies. At the extreme east end of Lake Monona is a series of gravel knolls, on which there are also effigy mounds, and in the cornfields back of these, many burial mounds. The ridge which bounds the horizon to the east has another series of mounds. To the southeast of these, near the outlet of Lake Monona, is a very interesting group of effigy mounds and earthworks; and to the northeast, near the inlet of Lake Monona, is still another.

The locality seems to have been well chosen, as its central position makes it conspicuous, and its isolation and the peculiar shape of the ridge would make it a prominent object in the landscape. The sheets of water come so near to the foot of the ridge as to give an unimpaired view, and the height and shape of the knolls make it even now an impressive spot, just such a spot as would be regarded with peculiar awe by a superstitious primitive people. It needed only the kindling of fires on the summit to throw a glare of light across the water and to fill the whole landscape with shadows. We can imagine that it would be a very weird and wild place for the erection of a burial mound; a remarkably favorable place for the lighting of beacon fires and especially favorable for the practice of sacrificial rites. Of the group itself we do not need to give a very extended description; figure 3 will illustrate this. The beacon mound is the central object. Near this is what we call the altar; it has a peculiar form resembling that of a tortoise shell, but without any of the protuberances which would represent the limbs and other members of the tortoise. This so-called altar has been exhumed and remains taken from it. Certain members of the Wisconsin Academy of Sciences have explored this mound, and we quote from their report.[7]

"Mound No. 1 is pear-shaped, being 78 feet long and 55 feet wide. It was composed of alternate layers of mold and clay to the depth of 6 feet, then a layer 1 foot thick, succeeded by alternate layers of clay and mold to a depth of 9½ feet where the natural surface is reached."

"The penetration was carried three feet below the natural surface through layers of bowlders and coarse gravel. *A fire place two by two and one-half feet*, with a layer of charcoal and ashes 4 inches in thickness, was found at a depth of 5 feet. In this was a piece of cloth partially burnt which for the most part crumbled to powder on exposure to the air. Both on the north and south sides fragments of bones occurred of from 3 to 5 feet from surface in an advanced state of decomposition. In the centre at 2 feet above natural surface were obtained, nearly broken down by decomposition, the femurs, tibiæ and fibulæ of a single skeleton, corresponding to 6½ feet in the height in the living subject."

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[7] See Transactions of Wis. Academy of Sciences, Vol. III, p. 10, for 1875-76.

"Mound No. 2 (the beacon mound) is a round mound 40 feet in diameter and sloping to the base of No. 1. The line joining the centres is 64 feet long. Two fire-places were found: one at 3 feet and the other at 5 feet below the surface. Dimensions the same in each, 3 × 6. In the lower one were partially burnt bones and the ashes. At 6 feet were found some pieces of pottery and a mass of bones consisting of fragments of four or more skeletons in a tolerably good state of preservation, corresponding in height to 5 feet 8 inches to 6 feet 6 inches in the living subject. Photographs of the two crania were secured and figures of these are given with the report."

This is the description on which we base in part our supposition that the locality was a sacrificial place. The figures illustrating these objects may be seen in the report, and so we omit them here.

One other locality where altar-mounds have been discovered may be worthy of mention here. It is a mound situated on the north side of Lake Mendota. This is also one of a group which was located on high ground and attended with a lookout mound. The first description of it was given by Dr. J. N. De Hart.[8]

The largest pyramidal tumulus of this group (fig. 4) measures one hundred and eighty-eight feet in circumference, and thirty-five feet from the base to its summit. From its summit we have an extended view of the surrounding country several miles in every direction. The altar-mound, to which we refer, is located a few yards from this beacon mound. This mound is about five feet in height and one hundred and twenty-five feet in circumference. We give a representation of this mound and quote from Dr. De Hart's report. "After removing the surface a layer of gravel was found one foot in depth, and on the summit a layer of sand dipping downwards into the layer beneath it. Following this was a deposit of black loam three feet deep, and underneath this was another layer of gravel, and then a deposit of earth. A thin stratum of gravel was then removed, when ashes, charcoal and flints were found lying upon an altar of stones. The latter rested upon a bed of yellow clay. The altar was about one and a half feet high, three and a half feet in length, and two feet in width.

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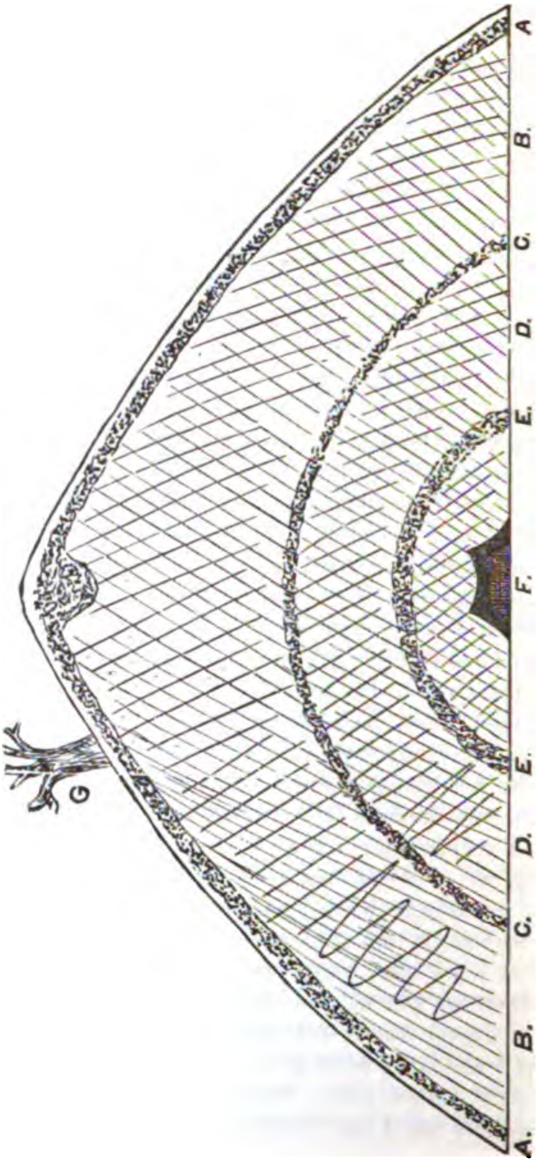
[8] See Transactions of Wisconsin Academy of Sciences, Vol. IV, 1876, p. 188. See also American Antiquarian, Vol. I, No. 4, p. 190.

The excavation was continued downward to the depth of three and

FIG. 4.



Altar-mound on northeast shore of Lake Mendota, near Madison, Wis.

a half feet below the surrounding surface, but nothing further was found."

AN ANCIENT VILLAGE OF THE EMBLEMATIC MOUND-BUILDERS; CACHES GUARDED BY EFFIGIES; EFFIGIES GUARDING THE VILLAGE, AND SACRIFICIAL PLACES NOT FAR AWAY. By STEPHEN D. PEET of Clinton, Wis.

TYPICAL SHAPES AMONG THE EMBLEMATIC MOUNDS; THE DIFFERENT ATTITUDES EXHIBITED BY THE SAME ANIMAL. By STEPHEN D. PEET of Clinton, Wis.

GAME DRIVES AMONG THE EMBLEMATIC MOUNDS; DEER DRIVES, APPARENTLY CONNECTED; BUFFALO DRIVES WITH EXTENSIVE OUTLOOKS; HIDING-PLACES FOR HUNTERS, AND EMBLEMS ACCOMPANYING THE DIFFERENT DRIVES. By STEPHEN D. PEET of Clinton, Wis.

THE CORRESPONDENCE BETWEEN THE PREHISTORIC MAP OF NORTH AMERICA AND THE SYSTEM OF SOCIAL DEVELOPMENT; THE GRADES OF PRIMITIVE SOCIETY, AND THE CONDITION OF THE NATIVE TRIBES COMPARED. By STEPHEN D. PEET of Clinton, Wis.

PERSONAL OBSERVATIONS OF THE MISSOURI RIVER MOUNDS FROM OMAHA TO ST. LOUIS: CONSIDERED FROM A GEOLOGICAL STANDPOINT. By E. P. WEST of Kansas City, Mo.

[ABSTRACT.]

THE mounds extend from Omaha, Nebraska, to St. Louis, Missouri, in an unbroken chain and are invariably associated with the Loess and Terrace formations on the Missouri River, and are probably coëxtensive with them or their equivalents in other localities. But this fact has but little significance without other facts which stand associated with it. The black vegetable mould is as thick on the mounds as on the adjacent land. The trees growing on them are as large and as old as those of the surrounding forests. But all this might be covered by a period of 500 years. Other facts which may be superadded are more conclusive. Stone implements have been frequently found in the Loess from three to thirty feet deep under conditions that leave but little doubt of their having been lost in the old Champlain lakes and covered in where found by the super-accumulation of their deposits. Human bones were found at Kansas City in a Loess hill, near its summit, eighteen feet below the surface and within ten feet above the base of the deposits under conditions to lead to the belief that they were engulfed and buried in the same way. A vase of antique pottery was found in a Loess hill near its summit, fifteen feet below the surface, in clay which had every indication of having remained undisturbed from its primitive deposition, at White Cloud, Kansas. Another vase precisely similar in every respect was found in an artificial mound on the summit of the bluff overlooking that place and the Missouri River about half a mile distant, but at a greater elevation. A vase of the same antique work was found in sinking a well on a broad low terrace at Manhattan, Kan., at a depth of nine feet. Dr. Parr of Weston, Mo., found a similar vase in opening a mound on the summit of the bluff overlooking that place and the Missouri River, which contained the vertebræ of a fish and some shell beads. The modern Indian erects his wigwam on the recent alluvium, and in the valleys and upon the plains wrought out by all the geological ages, with equal indifference, showing no preference whatever for the Loess bluff or the terrace tables. Not so with the Missouri River mound-builders. They chose these formations to the exclusion of all others. The preference could hardly have been from any peculiarity of the soil or of the formations themselves, and we must look to extrinsic conditions to explain their choice.

The Loess was deposited in lakes of still water and the terraces were formed by shore washings, and exposed by subsequent elevations of the land or subsidence of the water. May we not, then, look to these lakes for an explanation of the preference? May we not regard the Missouri River mound-builders as a race of lake-shore dwellers, made so from considerations of food supply? It is probable that all of the mounds in Kansas, Nebraska, Iowa, Missouri, Illinois, Wisconsin, Michigan, Indiana and Ohio north of the 40th degree of north latitude are of about the same age and correspond within a few hundred years, at most, with the age of the Missouri River mounds, and stand associated with equivalent geological formations, *i. e.*, that they were all erected fringing on lakes which had a synchronous existence in the Champlain epoch.

The occupancy of the mound-builders began before the subsidence of the Missouri River and Kansas lakes, otherwise their remains and implements would not be found, as they are found, in the undisturbed primitive deposits. And the changed conditions which followed the subsidence preclude the idea of their long continuance afterwards without changed habits and modes of life. Outward physical conditions in a great measure determine the modes of life of a primitive people, and this simple race, turning northward after the close of the ice reign, found the warm Champlain lakes filled with fish, inviting an occupancy of their hospitable shores. Here they erected their mystérious abodes and drew their principal food supply from the lakes. But time moved on with its inevitable changes. The lakes were drained and the supply of food they afforded was diminished, while the productions of the land were increased in a corresponding ratio. Conditions were totally altered and the mound-builders suffered extinction or else were forced to change their modes of life. At any rate, their distinctive character was lost long before the European touched foot upon this continent.

Were they exterminated by nomadic neighbors, or did they become themselves nomadic in their habits? We may never know, but their total and tragic extinction is most probable.

The Mound Builders Identified. By John Campbell of Montreal, Canada.

[ABSTRACT.]

In this paper it was taken for granted that the ruling tribes of Mexico and the Mound-builders belonged to the same race. This parent race is alluded to in the Mexican annals as that of the Citin. The author derived the American Citin, whose works first appear on the borders of Alaska and British Columbia, from the Khitan of the Chinese and Corean historians, who, together with

the Tchoutchis, once occupied a great part of Siberia, took possession of northern China, and sent colonies into Corea. Tracing the Khitan backwards he found an earlier seat of their empire in the great mound country at the head-waters of the Yenesei in Siberia, the mounds of which bore among the Tartars the traditional name Li Katei, or the sepulchres of the Cathayans. Through Tartary and Western Thibet the Punjaub was reached and a more ancient habitat of this race indicated in the country possessed in the time of Alexander the Great by the Cathaei, whose capital was Sangala. Thence along the line of the Hindoo Koosh mountains and the southern shores of the Caspian, he arrived at the original home of the race in the Caucasus, Armenia, Syria and Mesopotamia, where it was known to the Assyrians and Egyptians as that of the Khita or Kheti. The Khitan empire existed on the borders of Palestine and Syria in the fifteenth century B. C., and was destroyed by the Assyrian Sargon in the year 717 B. C. Soon afterwards it was reëstablished in northern India where Alexander found it still flourishing in 327 B. C. Aryan encroachments did not succeed in wholly overturning this empire till the fourth century A. D. Then it revived in Chorasmia and after a short existence removed its seat on account of Tartar invasions to Siberia. Another eastward movement, the result of foreign pressure, marked the eighth and ninth centuries, during which period Corea and Japan must have been colonized and America discovered. In the eighth century the Toltec empire in Mexico began, and in the eleventh the Aztecs left their Asiatic home.

The writer called attention to the agreement of the various historical peoples identified as members of the Khitan family in physical features, language, religion, government and arts, as mound-builders and the possessors of a peculiar form of writing. He then proceeded to compare the Khita hieroglyphics of Hamath and Carchemish in Syria, with those of the Siberian inscriptions from the Yenesei, the Mound-builder characters on the Davenport, Brush-Creek and Grave-Creek stones, and the well-known Aztec hieroglyphics, exhibiting their identity as syllabic types, and confirming it by comparison with the Cypriote and Corean alphabets derived from the same source. In this connection he referred to translations of several of these inscriptions, Hittite, Siberian and Mound-builder, published by him, the key to their transliteration being the Aztec hieroglyphic system and to their translation the

Japanese language, which he regarded as the standard of Khitan speech.

He then presented a classification of existing peoples in the Old World and in the New whose primitive stock as evinced by language, religion and tradition was Khitan, however modified by hybridism in physical feature. The following is the classification:—

In Europe.	Basques and Picts.
In Asia.	Caucasians, including Georgians, Circassians, Leshgians, Mizjeji, and probably Ossetes.
	Turanian Indians, including Pulindas, Quasi-Pulindas, Kariens.
	Siberians, including Yeneseians, Yukahirians, Koriaks, Tchuktchis and Kamtchatdales.
	Japanese, including Loo-Choos, Ainos, Coreans and Japanese proper.
In North America.	Dacotahs.
	Huron-Cherokees.
	Choctaw-Muskogees, including Natchez, etc.
	Pawnees
	Paduca-Pueblos.
	Yumas.
	Aztec-Sonoras.
In South America.	Lencas of Honduras.
	Chibchas of New Granada.
	Peruvians.
	Chilenos.

The writer referred to the former existence of a hieroglyphic syllabary among the Creeks and suggested that the Cherokee alphabet of Sequoiah had been derived from some such source. He maintained that the Sanscrit of American speech, by which languages of apparently diverse origin might be unified as to source, must be looked for not in America, but in the old world.

METRICAL STANDARD OF THE MOUND-BUILDERS, DEDUCED BY THE METHOD OF EVEN DIVISORS. By CHARLES WHITTLESEY of Cleveland, Ohio.

[ABSTRACT.[1]]

AFTER giving an account of the principal surveys of the ancient works of the state of Ohio, the author stated that these large groups of earth walls mark the sites of ancient cities. In their original state their greatest height was probably from fifteen to eighteen feet, generally without ditches. Some of them are now fourteen feet above the surface of the ground, extending at various heights more than a mile in length. Those at Marietta, North Bend, Portsmouth, Newark, and near Chillicothe, are yet so well preserved that they can easily be traced. They show long lines of embankments, rectangles, squares and circles, with entrances or gateways ; and also mounds, with a square, rectangular or circular base, and frequently truncated summits. There are at least fifty combinations of this kind constituting pueblos or ancient towns within the limits of Ohio, and many hundred single mounds, forts and isolated works, the objects of which are still conjectural. There are rarely effigies in earth, and a few enclosures and mounds of loose stones. The engineers who laid out so many mathematical figures must have had a common unit of measure.

The average of the outstretched arms, from finger tip to finger tip, has long been recognized to be a fathom of *six English feet* generally used in nautical matters. It can be traced as far back as the Romans. There is a name for it among the ancient Teutons and Scandinavians. The origin of the English yard is not known, but the probabilities are that it is based upon the fathom divided in half.

If we look to the human arm as the datum of the mound-builder standard, and assume for the purpose of discussion that a rod or stick of two and one-half or of *five English feet*, represents very closely the measure used by that people in laying out their earth works, we have sixty English inches.

The best determination of the average human arm gives about thirty inches from the shoulder joint to the tips of the second (or long) finger, which is half of sixty inches.

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[1] Printed in full in the Tracts of Western Reserve Historical Society, Cleveland, 1883.

If the outstretched arms truly represent the fathom of two yards, the average breadth of the chest, between shoulder joints is *one foot*, or twelve inches. Laying the palms flat, with the ends of the thumbs touching each other, the spread of the hands is a close approximation to a foot; one palm and its thumb is a *half foot*, or six inches.

Is this coincidence accidental? That any mound-builder unit should correspond to the English foot, or inch, must be accidental, unless there is something in common in the derivation of the standards. Some of the banks, platforms and forts, where the forests still remain over them, appear to be nearly perfect; but, of course, the upper crests have been shortened by exposure, and the base lines increased. The surveyor generally finds them in such a condition that fractions of feet are not reckoned, except in regard to height. Of course the superior lines are all short, more or less, and the lower ones are all too long, as compared with the originals. Where the measurements are taken along the middle line of the present summits, from angle to angle, the distances must be nearly the same, as the originals, which were no doubt measured on the natural surface, along the middle of their bases.

A survey along the middle of the surface of the present banks would be in a vertical plane directly over what I presume to be the guide line of the mound-builder engineers. A perimeter determined in that way cannot materially differ in length from the original, but a very large proportion of the works are not regular figures, with equal homologous sides. For the purpose in view, only such as are very near perfect squares, circles or rectangles will be used.

Recently Professor O. H. Mitchel, of Marietta College, at the instance of President Andrews, made a survey of two prominent earth platforms, or truncated pyramids, in the group of works at Marietta, O. Professor Mitchel reports that their flat surfaces are in good preservation — one 146½ by 138.20 feet, the other 118.14 by 200. The slopes are not of uniform grade. There is no difficulty in making exact measurements, which were determined by two sections of his engineering class, but they differed half a foot in the position of one of their corner stakes.

Selecting from all the authorities within my reach such lines as appeared to have been measured with care, I found them to be *eighty-six* in number. *Sixty-seven* of them are the sides of seventeen platforms, squares or rectangles, varying in length from 45 to 1,150 feet. *Eleven* are the radii of circles 88 to 860 feet in diameter. Portions of regular works that are so much injured that good measurements of all the parts cannot be made, having 50 to 950 feet in length, furnished the remaining *eight* lines. Long lines are more reliable than short ones, because there is always some doubt as to the points where to begin and to end; and also as to the angle where the middle line is used.

In Ross county there are three square embankments of the same size having 1,080 feet on all sides. Below is the result of the division of 87 lines, showing the results in a tabular form:

Percentage of lines divisible by 2½, and its multiples.

2½ feet	fifty-three per cent.
5 “	fifty-five “
10 “	fifty-seven “
15 “	forty-five “

Divisible by 3 and its multiples.

3 “	thirty-six per cent.
6 “	twenty-three “
9 “	twenty-three “
12 “	four “
Divisible by 4	twenty-five “
“ “ 8	fifteen “

It would seem to be a certainty that the ground plans should have been laid out by a rod, which was not used in fractional parts. If a large number of base lines are found to be divisible by one figure, it would be a very strong inference that the measuring rule of the builders was about that length. But, because we use feet and inches to make our measurements, that affords no proof that the ancients made use of feet and inches. We have no other mode of making comparisons, but there were many other units which they could have used.

The length of the average human arm, and of the pace, are very near to *thirty inches*, or *two and one-half feet*. In some countries the pace is reckoned from the track of one foot to the next one of the same foot, or *five feet*, equal to sixty inches.

Because the rule or measuring stick of the mound-builders appears to have been equal to a given number of our inches, and derived from the arm or pace; it by no means follows that its subdivisions were equal to our own, or that they knew of either of them. Admitting that their metrical standard was thirty inches, it may have been divided in *half*, equal to fifteen inches; or into *quarters* of 7½, and *eighths* of 3¾ inches. These spaces are divisible by *half* an inch, by *three-fourths* of an inch, and by an *inch and a half* as readily as by an inch; except the eighth part. What the subdivisions really were, we are left wholly to conjecture.

METHODS OF ARROW RELEASE. By E. S. MORSE of Salem, Mass.

[ABSTRACT.

THE author recited the rules at present applied in the American system of archery; the bow being drawn with the right arm, the arrow being placed on the left side of the bow, and three fingers being used to hold the arrow. Among the Japanese, a different system prevails: the arrow is placed on the right side of the bow as it is held perpendicularly, and the drawing of the bow is performed with only the thumb and one finger on the shaft. China, Japan, and the Corea are alike in this manner of drawing the bow. Among Indian tribes, the methods of arrow-release differ very widely. In general it may be stated that our system of arrow-release (which the author designated as the Saxon method) is substantially the same as that of the majority of European races, the modifications of the system among them not being important.

The Japanese use a glove with a heavy thumb, and sometimes a heavy ring on the thumb. Mr. Morse exhibited the Japanese archery-glove. It has a filling of wood and pitch in the thumb, which aids in grasping the arrow. He considered this glove the best of its kind.

Our system of three-finger release is certainly as good as any other, and probably is the best. With this system our archers — for instance, some in Ohio — are able to outshoot any Indian, tried by all the usual tests. As to the methods of stringing the bow, the author had not been able to find much uniformity. A number of different modes were exhibited.

THE KITCHENS OF THE EAST. By E. S. MORSE of Salem, Mass.

[ABSTRACT.]

THE author, during his travels in eastern Asia, had made some observations on the cooking-apparatus there in common use. The Japanese largely employ a mere fireplace, over which the vessels containing food are suspended by hooks: they have, however, two or three kinds of regular stoves of different designs. In China, stoves of a definite character are in use: one was found in Canton which was very elaborate; it was long, and had numerous openings. In Singapore, there appears to be only one kind of stove, and it is of decidedly primitive construction. In fact, it is little more than a rough trough filled with earth and sand, on which are laid rough stones selected with reference to pots of various sizes; and the fire is built among the stones. The kitchens in which these constructions are found are invariably very dark and dirty. In northern Java the author found a stove made of arched clay, as half an earthen pipe would be if cut through the axis of the cylinder. This half-cylinder is set with the open part down, and fire is built under its arch. Holes are cut through the crown of the arch, to hold some of the pots, while others are merely set upon the surface.

IN-DOOR GAMES OF THE JAPANESE. By E. S. MORSE of Salem, Mass.

[ABSTRACT.]

AMONG the simple in-door games of the Japanese are some that are played with balls, jackstones, and cat's-cradle; but all these are more elaborate than with us, and the cat's-cradle goes through a far greater variety of changes. The author believes that the greater intricacy of Japanese simple games is due to the fact that older people take more interest in them. Among these games, there is one similar to "Simon says thumbs up;" there are tricks with the hands, much like our own; and there are numerous games of striking hands, which appear easy, but require much practice to acquire adroitness. There are many games that test strength or endurance: among them are some in which ears and noses are pulled, and others where the competitors each hop on one foot, and try to push their rivals over.

They have a very elaborate game entitled Yo. The pieces are placed on intersections instead of on squares. It frequently takes a month to play one game, and the players often deliberate over a move for an hour or two. Experts in the game acquire a wide reputation. Japanese chess is probably the most intricate game in the world. The board has eighty-one squares, and twenty pieces are used, which have moves somewhat like our own, though none are exactly similar. The pieces change in grade when they arrive at a certain position on the board. The strangest feature of the game is, that either player can take any piece which has been captured from him, replace it on the board, and use it against his adversary. This makes the game utterly bewildering to a foreigner.

The Japanese have no games with spotted and court cards like ours; but they have a card game of "authors," which compels players to cap verses of classic poetry.

SECTION I.

ECONOMIC SCIENCE AND STATISTICS.

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penses that may be incurred while in their hands, — the chances of loss, or of change in value, — and, finally, the extent of the demand for whatever may be the product of their skill, industry, and investment, its probable permanence, and its tendencies to change.

These questions, being well considered in the beginning, will enable the careful operator to avoid losses from imprudent investment, from over-supply of the markets, or from the depression of receipts below the limits of cost.

By a train of reasoning analogous to this, those intrusted with the government of towns, cities, or states, may determine as to how far the cost and maintenance of public enterprises will be justified by the results; but with this difference, that the benefits or profits, instead of being measured by a money value, are often to be found in an advancement of the public welfare, and in the security, convenience, and prosperity that may ensue.

But whether in private enterprise or public undertaking, we may attribute success alike, in both, to an attentive notice of the facts and circumstances upon which they depend; and, if loss or failure follow, the reasons may very generally be traced to ignorance or inattention as to the facts and probabilities that should have been known beforehand.

These thoughts lead us directly to the point we are first to consider, viz.: How shall the knowledge of the required information be obtained? In the primitive way (and for a small business this may be the best one), the person will, from his own observation, "look over the ground," and consider the various points to be taken into the account. He will make inquiries of others, as to the supply, demand, prospects of competition, and the like; and thus accumulate a certain amount of information, upon the extent and accuracy of which, his success or failure will in a great degree depend.

Advancing a step farther, we find, in most great industries and interests of the country, that those in the same business or pursuit, whether in the arts or sciences, or in financial operations, however they may be influenced by local rivalries or petty jealousies, are constantly tending to the formation of associations or societies, for the advancement of their common interests. They meet for the discussion of methods by which expenses may be saved, or profits increased. They inquire of one another as to their experience or observation upon doubtful points. They seek to gather light and aid from science to stimulate and reward invention, and to excite rivalries in the comparison of improved products. They discuss financial and national questions that may effect their welfare; and not unfrequently they appoint committees or agents, from their own number, to gather statistical facts and details for their own use and guidance.

We consider the information thus obtained, as deserving high rank in point of accuracy. It is chiefly taken from records, without a motive for concealment or evasion, and with a full knowledge that self-deception and loss would result from error, whether above or below the truth.

From this combined experience, each member who participates obtains a standard for comparing his own results with the general average. He cannot afford to fall below it, and he has the strongest motives for reaching the highest limits that have been reached by others.

Still these statistics, however accurate they may be, are necessarily special, and often technical in their nature. They cannot be compared with those of another business, and may be incomplete within themselves, as naturally relating to methods, rather than to financial details. They might show *how*, rather than *how much*. They will seldom contain a balance-sheet of profit and loss, or anything that would advance the fortunes of a rival in business, or reveal the secrets of an unprofitable enterprise. We must receive them as we find them,—good only as far as they go.

Besides these associated business inquiries, prompted and guaranteed by self-interest, we find various others, voluntarily undertaken with reference to particular subjects, often for the promotion of a moral, religious, benevolent, educational, or political object and ranging in value all the way between the accuracy of statements taken from records, or gathered by faithful inquiry, by chosen special and zealous agents on the one hand, and the random conjectures carelessly returned by those who know but little, and care still less, about the subject of inquiry, on the other. It would be wholly impossible to assign a scale of value to statistics thus obtained, where everything depends upon the circumstances of the case, and the accuracy of information on the part of those who make returns,—the fulness with which they are reported and the care with which they are combined.

We have another class of non-official statistics collected and published by private enterprise, for the information of particular trades or professions, or for use by the general public; their reputation and success depending wholly upon their accuracy, and being brought to the test of local and personal knowledge every day and everywhere, we may naturally expect them to be as accurate as they can be made. In this class, we may include directories, trade and market reports, financial transactions, and the current commercial statistics generally.

There may be instances where they are tainted with a suspicion of private or speculative motive: but such is the vigilance of rival enterprise, that detection will quickly follow, and an exposure would at once degrade a reputation for independence and impartial statement, to the rank of a private job for a speculative end.

Exhibits openly made, for the avowed purpose of presenting the favorable side of a business enterprise, may be taken for what they are worth, and are often trustworthy; but, when concealed under a false pretence, they deserve suspicion, and, when exposed, they generally injure the interest that they represent.

The best of these statistics are taken from records, and are entirely correct; others are collected by special agents, and should be approximately near the truth; and there is still another class, made up from the estimates of those supposed to know the facts, and which must wander more or less from the actual conditions that they attempt to represent.

It may be said of all of them, that their greatest value is for present use. They quickly pass by, to give place to the next issue, and remain only as historical records; but, as such, they still afford a most valuable means of comparison between the present and the past, and become landmarks of progress, ever instructive to those who may be seeking to trace the origin and growth of our industries and our resources; and now and then they are recalled as precedents, where new questions arise, under circumstances deemed similar to the past.

We will next consider some points relating to inquiries undertaken by authority of government, either for the intelligent discharge of its own functions, or for general information, the good of its citizens, and the advancement of knowledge among mankind.

We may, in general, remark, that nothing can be properly done, in the machinery of government, without leaving its record. If money or property is received, there is an entry; if a payment is made, or if property issued, there is also an entry, and a receipt to prove it. In short, the whole theory of our government involves the necessity of a record of every official transaction; and it is only in the cases of intentional fraud, or gross neglect, or unavoidable accident, that the history of every public act cannot be traced from these records.

A record, to be trustworthy, should be made at the time of transaction, and while all the facts as to time, subject, and amount, or other points of statement, are fresh in mind. Nothing should be trusted to the memory, and for record at more convenient season. It should be concise, and easily understood, and may often be very greatly assisted by tabular arrangement.

The summaries of these records, as published by the government, are, we believe, with few exceptions, entitled to great confidence, as far as they present transactions done by authority, or passing under the notice of government agents.

We may classify the official statistics of the government under the following heads:

First, Summaries of current business, published annually or at shorter intervals.

Second, Periodical inquiries made at wider intervals, as in the census, and requiring special agencies for their execution.

Third, The inquiries made by experts, or by special commissions or agencies created for a particular purpose. This class is sometimes associated with one or the other of the preceding.

Taking from among these classes the census, as one of the most important, let us notice some of the methods by which it has been taken.

The earliest returns that I have found, in colonial times, were made by sheriffs and constables. At a later period, the national census was for a long period taken by the marshals of the district courts, or their deputies,—officers whose duties are quite analogous to the former; and this practice of assigning the task to sheriffs still prevails in several of the states.

In many other cases, assessors discharge the duty. In New York, before 1855, special agents were appointed by local authorities; and, commencing with that year, they have since been appointed by the secretary of the state. The appointing power has been vested in state boards, in boards of county commissioners, and in the judges of inferior county courts. Assuming what I think all will admit, that census inquiries should be made entirely free from any suspicion that some tax or some personal liability is to be incurred, it is evident that an assessor cannot question an ignorant man about his property and his crops, without exciting his fears that some tax is to be laid. The sheriff or the constable seldom makes a professional call, except to serve some papers or make an arrest. There is, therefore, a strong reason for appointing persons who are to make the census inquiries their only business, and for making it widely known that there is no taxation, enrolment, or other liability incurred by giving full and true returns, and that there is no sectarian or political end to be served by the inquiry.

This excellent end is now well enough secured under the national law, and in several of the states. They have a still better method in Great Britain, where a system of registration of births, marriages, and deaths, has its districts and its agents under constant organization, and to which, once in ten years, the census can be assigned, without creating new offices. In Sweden, where a system of registration, including also a record of change of residence, is in charge of the parish-clerks, they take a census whenever they choose to post the books, without any special inquiries being made, more than what these records contain.

In the national census before 1850,— in New York before 1855, —and in some of the states still, each family had one line upon the blanks; and the number of persons of different ages, sexes and color, were entered in columns provided. The limit of classification was of course restricted to these columns; and, although the totals of each class were easily obtained by adding, the results were meagre and unsatisfactory.

The change that allowed a line for each name, one column for the exact age, and other columns for native country, profession or occupation, etc., while it simplified the labor of taking, allowed ample field for classification; and it made it necessary to employ a large force of clerks, in a central office, for the reduction of the returns for publication.

By a method now generally used in Europe, the census is taken upon "householders' schedules," which are distributed one to each family, some days beforehand, filled out by the head of the family, and collected upon *one day*. The only instance in which this has been done in the United States, within my knowledge, was in the

District of Columbia, upon the eleventh of November, 1867. This census was taken by the metropolitan police, under my own direction, and with entire success. It was attempted in the city of Baltimore some months afterward, and failed, apparently from want of proper management on the part of those in charge.

For all kinds of official inquiries, relating to business, as well as to personal statistics, I think the true and proper method is, by means of *special blanks*, carefully prepared, simple, and fully explained. These should be distributed some little time beforehand, and should be taken up, if not in one day, within a short period of time, but with reference to a given day. The chief difficulty to be encountered is the illiteracy of those who should fill the blanks; but in the District of Columbia, which in 1867 contained a large number of colored families, but recently freed from slavery, the blanks had been, in almost every case, filled out by some one to whom they had been carried.

In following our subject, the "Methods of Statistics," we may notice some points in the condensation and arrangement of facts that may be of interest.

With a vast amount of information before us, as, for example, in the returns of a census, let us consider what is to be done, and how it can be done with the least labor and greatest certainty. After inspection to make sure that the work is all together, in proper order and condition, it will be found that several distinct operations are necessary, in preparing the results for the press. Columns of figures must of course be added, and carefully revised. As the totals of several sheets will often be consolidated into one sum, it is best to use spare sheets of the same schedule for entering the totals of pages, so that these partial totals can be easily combined. It is always a good practice, where long columns, of many figures in each, are to be entered in adding, to provide paper with narrow vertical ruling, that shall allow of but one figure in a space. In cases where the first two or three right-hand characters are generally ciphers, they may be left out altogether, the significant figures only being entered in their proper places. It saves a little time and labor, and does not lead to error.

Where a great amount of statistical material is reported,— as, for example, the names in a census,— the blanks should always be plainly divided by horizontal and vertical lines, *printed in* *preference to ruled.* The horizontal lines should be numbered from the top downward, upon both margins. This numbering is the more important where an entry is carried across to another page. Each line should contain but one entry, and there should be, if possible, no blank lines except at the end. Then, with a little multiplication-table at hand, showing the number of lines in a full sheet, and for each number up to the highest that are likely to be found in a return, the totals can be rapidly and accurately ascertained, as follows: The number of sheets are first counted on the back edge, and the number of entries they should contain, if full, are set down. Then, by glancing over each page separately, it is easy to notice whether there are any lines with two entries, any blanks, or any lines in excess. The deficiencies are set down in one column, the excess in another, and their difference is added or subtracted, as the case may require, when the true sum is at once found. This operation, which is the first thing done, should be repeated by another person; and, when found to agree, it should be kept as a test-number for verifying the accuracy of much of the work that is to follow. In measuring parts of pages, a scale made of a strip from the margin of a blank schedule, and pasted upon a card, will save all labor of counting.

In statistical labors, where the same returns afford material for a considerable number of deductions,— as, for example, the population sheet of the census,— it is generally best to take *one thing at a time.* Thus, the ages, professions, nativities, civil condition, etc., should be taken by separate operations, and not two or more at once. There is not, however, the least need of confusion in keeping the subdivisions of these subjects, in two or four classes, — as, for example, ages by sex and color,— by a simple arrangement of heavy and light horizontal lines upon the tally-sheet, and a little practice in its use.

There is much to be gained, both in time and accuracy, by a proper arrangement of a tally-sheet. The grouping together of tally-marks, by making four down and one across, so as to divide the work into groups of fives, is so natural and obvious a method, that few who have had occasion for this kind of work could have failed to adopt it. By an arrangement which I have used to a large extent in census work, I have had my tally-sheets printed off into squares, so that each compartment should receive one group of five, and no more. Then, by a series of numbers with a common difference of five, printed across the top of the sheet, at the head of each vertical column, the number of tally-marks in a horizontal row can be known at once, by glancing along the vertical column containing the last full group of fives, to the number printed at the top, and then adding the marks in excess, but less than five, in the next compartment. This saves all counting, and a considerable amount of time. There is also an advantage, on account of the eyesight, in having the tally-sheets of some other color than white: a neutral tint might be best, but I have found common manilla paper answer every purpose.

Plans have been proposed for using cards of different sizes and colors, properly inscribed or numbered, as counters, for classifying a variety of facts, forming together a definite whole. By using colors, the eye becomes, without mental effort, a guide to the hand, in their distribution into piles or cells in a case; and, when the work is done, their number may be accurately known by weighing, or by measuring the height of each pile. Those of different sizes could be separated by mechanical devices, without handling, and a little practice, wi'hout liability to error.

It may be said generally, that the chief, indeed the only real, difficulty, in the preparation of statistical data, consists in getting the facts correctly. There is nothing in the operation of a central office that needs to involve error; or, if an error is committed, there should be no difficulty in tracing it to the clerk who is responsible for it. An efficient way to secure accuracy in work would be, to make a money-charge against the clerk who commits an error, to be paid to the one who finds it. I believe that something of the kind is done in some of the statistical offices in Europe, a class of revisers being employed, who are paid by the fines thus imposed upon the careless.

With respect to statistics obtained by circulars addressed to persons supposed to have the information desired, we have every grade of value, from good to good for nothing. The result depends upon many circumstances; as, for example, whether the person making the return is paid, or is under some obligation to, or expects some favor from, the person or office making the inquiry; whether the inquiry can be answered by reference to a record, or by some research more or less conveniently made, or is to be supplied from personal opinion, and a general knowledge of the subject; or, finally, whether the question can be answered by any thing better than a guess, by one who knows perhaps very little about it.

I would hold it to be the general rule, that where the inquiries are few and simple, exact as to their object, and, if they refer to a record, exact as to time and subject, and especially if they can be returned upon the same blank, and without expense for postage or otherwise, a very large percentage will be answered without a second application. A repeated call would probably bring a third or a half of the remainder; but there will be, now and then, one who will fail to reply, unless under official or personal obligation to do so.

We have thus far considered the dealing with statistics that have been gathered from the whole of a given field of inquiry; there are other methods that deserve notice, and the first of these is that "by samples." A portion of some whole is carefully studied, and the results obtained are deemed applicable to the entire field.

The French statistician Moreau de Jonnès has given some instances of this method, as applied in times past, by persons who had acquired eminence, and whose work gained confidence; and very properly asks, "What is such work worth?"

Vauban, distinguished as a military engineer, at the beginning of the eighteenth century, wishing to know the agricultural production of France, and the revenue it would yield, resorted to a method which would appear strange enough now, but still may be called ingenious. He attempted to reach his object by taking an exact account of the production of a square league, reckoning the arable land, vineyards, pastures, and woodlands, with their products in quantity and value; and then, by the simple "rule of three," he said, "as 1 is to 25,000, so is the result to the whole of France."

The English agriculturist, Arthur Young, sought to ascertain the proportions of meadow-land, mountains, and the like, in France, by cutting up a map by lines following these features of the surface, and weighing the parts.

In 1790 Lavoisier, distinguished in science, and for this reason consulted by the national assembly upon a question of imposts, found no existing data that applied to the internal resources of the country, until he himself supplied them, by a method that is now altogether neglected in statistical researches. He proposed to ascertain the number of ploughs in the country, and from this to calculate the quantities, production, and consumption of agricultural crops.

In 1784 M. Necker, the distinguished statesman, deduced the population of France from an assumed percentage of the birth-rate; and this was taken for a census!

But coming down to a much later period, we find a remarkable application of the law of induction in a work upon the industries of France, by the minister Chaptal. He presents agricultural tables, which have been received with great confidence, since they bear the appearance of official statistics, and were executed under the Empire. His tables are found to have been computed, without acknowledgment, from a statement addressed by M. Hennet, director of the cadastral survey, in 1817, at a time when not more than a seventh part of this work had been finished. The other six-sevenths were obtained by a simple multiplication of the finished part.

Many years ago, a "distinguished statistician" published, with great apparent precision, the yield of potatoes in France. There had been no official inventory taken; but when one came to be made, some time afterwards, it was found that this deduction had been obtained by multiplying the yield of a single commune by 37,000, the number of communes in France.

These examples might be multiplied indefinitely; and we need not cross the Atlantic, nor go far back in time, to find them. There is scarcely a day, but that we see passing through the newspapers, estimates, deductions, and statements, that have no more solid foundation than those we have cited. Nevertheless, we must not wholly disregard the inductive method in statistics: there are many cases in which we can get nothing else.

The chemist must analyze the soils and the ores from samples. In every operation of testing the quality and the value of any commodity whatever, we must select from the material before us what appears to be the average quality. And so of statistics generally: if there is no actual and general inventory made, we must collect from what is deemed a fair average, and, from these data, obtain such conclusions as they afford. The result in this, as in everything, will depend upon the intelligence and honesty of the person who makes the estimate, the extent of his opportunities, his experience, and his skill.

Returning to the field of exact statistics, we may remark, that we can never have an accurate census of the population until we have a thorough and uniform registration of births, marriages, and deaths; a measure which this Association undertook to promote, more than a quarter of a century ago, but which has not made successful progress.

We cannot have a faithful statement of the industries, without a record kept of the production, the consumption, and the cost of operation. This is already done by most of the important ones, as an incident of business; but we lose the advantages by the hurried manner in which the official inquiries are made. Yet upon these returns we rely for all that is collectively known about them.

It follows, that, until we can realize these desirable features, the best we can expect is, to afford more time for previous preparation, by submitting beforehand the questions that are to be answered; which can only be done by the aid of "householders' schedules" for population, on "special blanks" for each of the industries, or other subjects, that come within the range of inquiry.

It was my intention to dwell at some length upon the illustration of statistical facts by graphic methods; but time will not permit, and opportunity for full preparation has not been found. For more than thirty years I have been accustomed to note down the principles involved in these methods, whenever, in the course of a wide and varied range of opportunity, a new one was found; and it has been with me a cherished intention to present the whole subject in a systematic form.

We may concisely state, that graphic illustrations, using lines, areas, or angular spaces, often supplemented by colors, may be employed for representing either —

1. *Quantities*, with or without reference to time.
2. *Time*, in recurring, interrupted, or progressive periods.
3. *Direction*, or *relative position;* and
4. *Intensity* or *force*.

In general, but two elements can be clearly presented at once; but by a skilful use of different colors, or kinds of lines, subjects of the same nature may be admirably compared, and the relations of cause and effect not only illustrated, but even discovered and proved. It is often admissible to introduce subjects having dissimilar notation, — as, for example, degrees of temperature, and height of barometer, — in the same drawing; but in these cases each must have its own scale, and, in a general way, every diagram must have a scale for every element of the subject that is represented, either expressed or implied.

Quantities may be shown either as they exist at certain periods of time, or as they form parts of a general total; and they may be presented so as to exhibit successive subdivisions, down to any desirable degree. If the divisions of a general total do not require subdivision, they may best be shown by angular spaces, as sectors, which together make up the whole of a circular area. If the divisions have some qualities in common, the shades of color may be of different intensity, significant of the degrees of quality that may exist. But where there are successive subdivisions, or *parts of parts* of a whole, there is no way so exact as by means of rectangular areas, which may also be shaded in different tints, as well as to separate them one from another as to show differences of intensity or degree.

In both of these methods, as well of angular spaces as of rectangular areas, we can only show quantities as they exist at a given point of time. We catch, as it were, the conditions, as does the light, the image in a camera. They admit of no such thing as motion or change; but these changes may often be strikingly represented by a series of diagrams, presenting the conditions at different periods of time.

Where time and quantity are combined, we have an easy and striking means of illustration; and in this the time may be in recurring periods, such as the hours of a day, or the months of a year, or it may be progressive, as in a series of years.

For the recurring periods, I think there is nothing so convenient and instructive as the *circle*, in which the quantities are measured along the radii, from the centre as their base. The entire radius may sometimes represent the whole of that of which these partial measurements are a part.

For a progressive series, the ordinates representing quantities should be measured from a level base-line, and the scale of graduation shown upon the side margin, while the time is measured from left to right by a scale along the upper margin.

For a simple comparison, a series of bars or lines, measured from a common base, and either horizontal or vertical, is a convenient and striking mode of illustration, and has now come into very common use.

A rectangular area, with parallel divisions, amounts to the same thing as a line; but with this difference, that a secondary subdivision may sometimes be represented with great effect.

Having thus stated some points in reference to graphic illustrations upon a true principle, I should not leave the subject without word of censure for some that are false. I will specify, particularly, such as attempt to represent comparative quantities by *concentric figures*, such as circles or squares. The eye has, in these cases, no means of just comparison; and they are very apt to mislead, where they are intended to instruct.

The same objection may be made against similar geometrical solids; for, although they may be literally true, their contents being to each other as the cubes of similar lines, the eye does not readily see the difference. It would be better, in such cases, to use cylinders or prisms of the same base but proportioned in length to the quantities that they represent.

ADDRESS[1]

BY

MR. EZEKIEL B. ELLIOTT.

VICE PRESIDENT, SECTION I.

[*Delivered at the Montreal Meeting,* 1882.]

THE SECTION OF ECONOMIC SCIENCE AND STATISTICS: ITS SCOPE AND ITS LIMITS.

TWENTY-FIVE years ago, in 1857, at the meeting of the American Association for the Advancement of Science held in Montreal, I had the honor of presenting a paper involving elaborate computations showing the law of mortality in the state of Massachusetts, United States, with corresponding tables of annuities and other monetary values, and comparing the results with those of other communities; a paper which would probably have been assigned to, and read before this section had this section then existed.

The British Association for the Advancement of Science, after the plan of which this Association was in a large part formed, was organized in 1831. Its section of Statistics was formed two years later in 1833, and the Section of *Economic Science and Statistics*, was established in 1856, 23 years later.

With us, the Section of Economic Science and Statistics was formally established in 1881 and is put into effective operation at the present session, in 1882.

Papers of a character entitling them to be read before this Section have hitherto in general been assigned to section A, that of Mathematics and Physics.

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[1] The author was unable to prepare the address in time for publication in the Montreal Proceedings. It is therefore printed in the present volume by vote of the Standing Committee.— ED.

The methods of investigation employed in considering these subjects were more closely allied to the methods of section A, Mathematics and Physics — than to those of Section B, Geology and kindred sciences.

Economic — pertaining to man and his welfare; man in his domestic and social relations; man, what he is, his property or what he controls, and his surroundings, over which he has little or no control — like climate, and the changes of the weather and of the seasons.

Economic Science.— The subjects which properly pertain to this section are scientific not speculative. The section is to deal with facts, known facts; not scattered, heterogeneous, unrelated facts, but known facts reduced to order.

Statistics.—The subjects considered must be truly scientific, that is, knowledge reduced to order, pertaining to the interests of man, either singly, as an individual, or associated in communities, or related by some common law.

The subjects to be submitted to this Section are not exclusively those which belong to economic science, but may pertain to any facts to which the application of statistical methods are peculiarly adapted, to facts susceptible of numerical expression and which may be arranged in tables; to any facts susceptible of such arrangements and groupings that the numerical laws which underlie may be elicited: numerical laws based upon the past, and which will afford facilities for confident and trustworthy predictions as to the future,— for example, the construction of life and annuity tables, correctly deduced from authentic records of population and deaths.

Among statistical methods useful and necessary to the eliciting of the more recondite and higher laws and results, the mathematical doctrine of probabilities plays an important part. It may be deemed desirable in the not very distant future, for this section to create a sub-section for the special consideration of the subjects and processes to which the mathematical doctrine of probabilities now specially applies.

Mr. Quetelet, the eminent Belgian astronomer and statist, whose acquaintance it was my good fortune to make at Berlin, in 1863, on the occasion of the convening in that city of the fifth session of the International Statistical Congress, there said to me that he had made an effort at a previous session of the congress, in London, to establish a section to be devoted to the application of the mathematical doctrine of probabilities to statistics—but that he had failed, that he had been making a like effort at the session in Berlin, then holding, but should fail, much to his regret, the number of trained mathematicians among the statisticians assembled, being small,—but that he was confident in his hope that at the next session of the International Statistical Congress he should succeed.

The next session of that congress was held two years later in Florence, Italy, and Mr. Quetelet did succeed; such a section was there formed, and has since continued a feature of that very important and useful organization.

A wide range of subjects properly belongs to this section of our Association — a section destined, I doubt not, to be one of great importance, and to be the promoter of practically useful results not merely to America, but to the entire world, — for the valuable results of economic and statistical inquiries are not limited to the people who foster them, but are as far-reaching as humanity itself.

This Section, as has been mentioned, embraces the consideration of a vast extent of special subjects relating to man, his nature, his possessions, and his surroundings of all kinds, including facts tending to show the condition and changing condition of communities, races, individuals, and interests.

An early and continuous knowledge of the condition and changes of condition of a community, and a prompt digest and publication of the facts, may tend greatly to its welfare, — not merely in time of peace and prosperity, but also and especially in time of war and of epidemics. The varying state of the community, or interest in question, may thus be brought clearly to the attention of the authorities and the people, so that evils as they arise may be instantly known and suppressed.

We may note as within the province of the consideration of this Section, units of measurement in general, as of longitude, time, space, force and energy, and other constants of nature and art.

PAPERS READ.

THE INCREASE OF THE COLORED POPULATION OF THE UNITED STATES.

By CALVIN S. MIXTER of U. S. Census Office, Washington, D. C.

SINCE the publication of the statistics of the population of the United States as ascertained from the returns of the census of 1880, it has been frequently asserted that they indicated very clearly that the colored people of this country are increasing more rapidly in their present state of freedom, than they did when in slavery. While it is true that the census returns of 1880 when compared with those of 1870, seem to justify this statement, it is nevertheless true that there is reason to doubt the accuracy of the census of 1870, in this respect. When the general results of the tenth census, so far as they related to population, were published in the summer and fall of 1880, it was charged by numerous influential journals that the returns of the colored population of the southern states, especially those relating to South Carolina and Mississippi, exhibited so large an increase over those of 1870, that they were more or less fraudulent. This was reiterated so frequently that the Superintendent of Census, after consultation with the Secretary of the Interior and the President, sent two capable officials from his office in September, 1880, to investigate this matter, the results of which were embodied in an official report to the Secretary of the Interior, dated October 5, 1880, wherein he expressed the opinion that they "satisfactorily settle the question as to the fairness of the tenth census in South Carolina."

Another and fuller investigation was made by five other officials detailed from the Census office, in November, 1880, which continued for nearly two months and led "to the conclusion that the census of South Carolina was correctly taken."

These reports may be found in Appendix C, pages liv to lxxvi of the Introduction to the Compendium of the Tenth Census of the United States, 1880. An investigation was also made in Mississippi, which resulted in like manner.

The reports relative to South Carolina give details as to the manner in which the investigations were conducted, and also as to the course pursued in 1870, by the enumerators who took the ninth census in certain localities, which will account for some of the principal defects in their returns. "The universal testimony obtained is that (in 1870) the names were obtained at court sessions, at musters and other like occasions, where considerable portions of the population were brought together, and not by a house-to-house canvass."

The following table exhibits the returns of the colored population, *i. e.*, persons of African descent, in 1880, 1870, 1860, 1850 and 1840, according to the U. S. Census reports, giving in detail the numbers reported from those states where the largest portion of such persons resided.

RESIDING IN	1880	1870.	1860.	1850.	1840.
Alabama	600,103	475,510	437,770	345,109	255,571
Arkansas	210,666	122,169	111,259	47,708	20,400
Delaware	26,442	22,794	21,627	20,363	19,524
District of Columbia	59,596	43,404	14,316	13,746	13,055
Florida	126,690	91,689	62,677	40,242	26,534
Georgia	725,133	545,142	465,698	384.613	283,697
Kentucky	271,451	222,210	236,167	220,992	189,575
Louisiana	483,655	364,210	350,373	262,271	193,954
Maryland	210,230	175,391	171,131	165,091	151,815
Mississippi	650,291	444,201	437,404	310,808	196,577
Missouri	145,350	118,071	118,503	90,040	59,814
North Carolina	531,277	391,650	361,522	316,011	268,549
South Carolina	604,332	415,814	412,320	393,944	335,314
Tennessee	403,151	322,331	283,019	245,881	188,583
Texas	393,384	253,475	182,921	58,558	....
Virginia	631,616	512,841	527,763	526,861	498,829
West Virginia	25,886	17,980	21,144	....	....
Total Southern states	6,099,253	4,538,882	4,215,614	3,442,238	2,701,791
Total all other states	481,540	341,127	226,216	196,570	171,857
Total United States	6,580,793	4,880,009	4,441,830	3,638,808	2,873,648

An inspection of the foregoing table will show that in several states, and especially in Arkansas, Louisiana, Maryland, Mississippi and South Carolina, the population reported in 1870 is but slightly in excess of that reported in 1860, while that returned for 1880 shows a large increase over that given for 1870. In Kentucky, Missouri, Virginia and West Virginia there was a smaller population reported in 1870, than there was in 1860, but in these states there was also a considerable increase shown by the returns of 1880 over those of 1870.

The magnitude of this increase seems to indicate first, that, as has already been pointed out, the census of 1870, so far as relates to these states, was seriously defective, and, second, that the methods adopted in 1880 secured better and fuller returns than had been previously obtained. This result was mainly due to the fact that a much larger number of enumerators, in proportion to the population, was employed in 1880 than ever before, each of whom had a much smaller territory to canvass, and a better knowledge of the families and residences included within the limits of his district, and who, in the performance of his duties, was under the immediate direction and control of an official that had been selected and appointed for this purpose.

An examination of the rate of increase of these people since 1840, seems also to indicate defects in the numbers reported in the census of 1870, and further that they are not increasing any more rapidly now than formerly.

The rate of increase throughout the United States has been as as follows:

From 1840 to 1850 the rate was 26.6 per cent.
" 1850 to 1860 " " " 22.1 " "
" 1860 to 1870 " " " 9.9 " "
" 1870 to 1880 " " " 34.8 " "

When however periods of twenty years are taken the results are as follows:

From 1840 to 1860 the rate of increase was 54.6 per cent.
" 1860 to 1880 " " " " " 48.2 " "

which shows that there was a falling off of 6.4 per cent during the second period as compared with the previous period of twenty years.

Owing to the disturbed state of affairs which prevailed during the war period from 1861 to 1865, it is impossible now to tell to what extent their increase was retarded, but as the adults among them now feel the burden of the support of their children as they did not in the days of slavery, and as they are not all as mindful of the rules of health as they should be, the probability seems to be, as the figures above given indicate, that their future increase will not be any greater than it has been in the past, but rather that it will not be as great.

ENHANCEMENT OF VALUES IN AGRICULTURE BY INCREASE OF NON-AGRICULTURAL POPULATION. By J. R. DODGE of Washington, D. C.

[ABSTRACT.]

THE paper begins by showing that national industry is prosperous in proportion to its diversity. The productions of agriculture would be unsalable if all the people were agricultural producers. A civilized nation, with the smallest proportion of non-agricultural workers in it, will be low in the scale of prosperity: with the largest proportion of non-agriculturists consistent with proper food-supply, the nation will be most prosperous. To a great extent, this is true also of the subdivisions of this country. It may be illustrated by the statistics of geographical sections that embrace groups of states, or by comparison of individual states. The author is even prepared to show, that, in a partly agricultural community, the increased employment of labor in industries that are non-agricultural stimulates improvement, compels higher culture, and makes the products of land, and the land itself, more valuable.

The states and territories are grouped for this comparison in four classes, which are thus designated: first class, having less than 30 per cent of agricultural workers; second, having over 30 and less than 50 per cent; third, having 50 and less than 70 per cent; fourth, having over 70 per cent, *i. e.*, being almost exclusively agricultural states.

Classes.	No. of states and territories.	Acres.	Value.	Value per acre.	Per cent of workers in agriculture.
First class.......	15	77,250,742	$2,985,641,197	$38.65	18
Second class............	13	112,321,257	3,430,915,767	30.55	42
Third class..............	13	237,873,040	3,218,108,970	13.53	58
Fourth class............	6	108,636,796	562,430,842	5.18	77

In the first class are Massachusetts, Connecticut, New York, New Jersey, and Pennsylvania in the east, and some of the mining states and territories of the west, diverse in many points, yet alike in the fact of a large non-agricultural population. This class has only one-sixth of the population in agriculture. The fourth class consists of North and South Carolina, Georgia, Alabama, and Mississippi. It has nearly four-fifths engaged in agriculture, on lands worth only an average of $5.18 per acre. The states having only two-fifths in agriculture have farm-lands valued at $30.55 per acre, while those having nearly three-fifths in agriculture have lands valued at $13.03.

When individual states are compared, the results are equally marked. The author compares pairs of states,— Virginia and Pennsylvania, Kentucky and Ohio, Iowa and Illinois,— as follows:—

Items of comparison.	Virginia.	Pennsylvania.	Kentucky.	Ohio.	Iowa.	Illinois.
Per cent of agricultural workers.....	51.41	20.68	61.67	30.97	57.46	43.65
Value of farm lands per acre.........	$10.89	$49.30	$13.92	$45.97	$22.92	$31.87
Income of workers per annum........	$180.00	$431.00	$199.00	$394.00	$448.00	$467.00

Similar computations give figures for the State of New York not widely different from those of Pennsylvania. The average value of farm-lands per acre in New Jersey is greater than in any other state, viz., $65.16: this is owing to a position between two great city markets, with proximity and easy access to both. Her percentage of agricultural workers is only 14.92; their average annual wages are $501. It will be seen that the wages of agricultural labor are subject to the same law. If computed for the first table in this paper, the wages per month of the agricultural laborer would be found, for the first class of states, fully $25; for the second, nearly $25; for the third, less than $20; for the fourth, about $13.50.

An application of the same test to the value of annual production for each man engaged in agriculture brings equally interesting results in the following table:—

Classes.	No. engaged in agriculture.	Value of products of agriculture.	Value per capita.	Per cent of workers in agric.
First class	1,060,681	$484,770,797	$457	18
Second class	1,566.875	616,850,959	394	42
Third class	3,017,971	786,681,420	261	58
Fourth class	2,024,966	324,237,751	160	77

The states with less than 30 per cent in farm-labor realize nearly three times as much per man as those which have over 70 per cent in farm-work. In other words, one man in the first class realizes as much as the three men who are competing with each other, having little outlet for surplus production. Three brothers in Alabama, laboring through the year, get as much for their aggregate produce as one farmer receives in Pennsylvania, simply because that farmer has a brother engaged in manufacture and another in mining. It is because in one case there is a market for one product only, thousands of miles away: in the other, there are markets at every door.

It appears evident that the proportion between agricultural and non-agricultural population is a measure of the values of the land, of the production, and of the labor of the farm. These values are rapidly enhanced by the increase of non-agriculturists. This is the lesson of the most authentic statistics of our own and of other countries.

BUILDING ASSOCIATIONS. By EDGAR FRISBY of Washington, D. C.

BUILDING Associations have a twofold object. 1. They answer the purpose of savings banks in enabling members to lay by certain sums monthly and receiving interest on the investment. 2. They enable persons of limited means to borrow money at a fair rate of interest by paying back the principal in small monthly instalments. It is proposed in this paper to see if we can discover any mathematical principles by which an association can be conducted that will make it equitable to both classes.

We shall assume that six per cent is a fair rate of interest, and allow a member to buy out on any month $200.00 or less from the association according to competition, for which he pays $1.00 interest monthly, and we will also suppose that he pays $1.00 monthly on the principal (this amount however may be varied); it will simply be paying interest monthly, and when the amount paid in on the principal (which must itself bear interest) is equal to the amount borrowed, the stock will be cancelled.

Let us take the most general case.

Let R = interest of $1.00 for one month (half a cent) and 1.005 = R = amount for one month.

We will suppose further that the member pays $1.00 per month for y months, then buys out at the maximum and pays afterwards $2.00 monthly; the value of his payments will be all the dollars he has paid in with interest, together with his prospective payments discounted, that is:

$$1+R+R^{2}\quad+R^{y-1}+2\left(R^{-1}+R^{-2}\ldots\ldots+R^{-n+y}\right)=200$$

by summing these series we obtain

$$R^{y}-1-2\,R^{-n+y}+2=200\,(R-1)=1 \text{ because } R=\tfrac{201}{200};$$

this reduced gives

$$R^{-n+y}\left(R^{n}-2\right)=0 \quad\text{or}\quad R^{n}=2$$

This equation is entirely independent of y and herein lies the absolute equitability of the whole system. If the person bought out on the first month $y=1$, and he must pay six per cent compound interest if he bought at the maximum; if $y = n$ the person is an investor and receives six per cent; if y has any other value he receives six per cent until he buys out, and pays six per cent afterwards. This equation also shows that it is precisely as advantageous to buy in one month as another (on the supposition that money sells at a uniform rate) and also that all classes are equally benefited; and again, if a surplus of money is on hand and a member is drawn out, he has the option of taking the amount paid in with interest or an advance at the maximum rate. In the first case he receives six per cent interest, in the second case he pays six per cent interest.

If we solve the equation $R^n = 2$ we obtain $n = 138.9532$ which shows that $200 borrowed can be paid up in about 139 months.

Example.— A person buys at the maximum and settles on the 101st month; we may suppose he is indebted to the association $200 and credited with a compound annuity for 100 payments; or we may suppose he is indebted to the association $200 with compound interest and credited with a compound annuity for 100 payments at $2.00 per month.

In the first case we have

$$200 - \left(1 + R + R^2 \ldots\ldots + R^{99}\right) = 200 - \frac{R^{100} - 1}{R - 1}$$
$$= \$200 - \$129.3366 = \$70.6634$$

In the second case we have

$$200\,R^{100} - 2\left(\frac{R^{100} - 1}{R - 1}\right) = 200\left(2 - R^{100}\right) = 200(.353317)$$

$= \$70.6634$ which is the amount of indebtedness; if, however, as is usual, he was only allowed simple interest on stock, he would have to pay not six but 6.6 per cent per annum.

If again the investor only received six per cent simple interest he may pay in not 139 months but somewhat more than 146 months, unless the association has wound up before. If a person pays $1.00 monthly as interest and pays $1.50 or $2.00 on the principal, the case will be precisely the same as far as the equitability is concerned, but we should have $R^n = \frac{5}{3}$ or $\frac{3}{2}$ and the association would wind up more quickly; instead of 139 months we should have 102.4 and 81.3 months respectively. We have made all the computations by compound interest for two reasons. 1. Because the association invests the money monthly. 2. Because it is the only philosophical way, other methods leading to numerous fallacies; it is however usual to allow only simple interest on stock in settlement. Many methods may be devised to provide for running expenses. 1. Five per cent instead of six may be allowed to investors and in settlement, but if we consider the difference between the simple and compound interest and also the fact that money is generally advanced considerably below the maximum, I think we have a sufficient margin.

Suppose a building association starts with 2400 shares, cancels 200 every year and runs twelve years: the difference between the simple and compound interest will be over $10,600 in the twelve years, and this difference comes out of the shareholders very nearly in a proper ratio for the money invested. It is altogether more equitable even in this particular than the plan of taxing each share proportionately, independent of the time it has been in, or of the benefit to be received.

LIFE INSURANCE AND SELF INSURANCE, WITH ILLUSTRATIVE TABLES.

By ELIZUR WRIGHT of Boston, Mass.

[ABSTRACT.]

THE intent of this paper is to show that whenever the net natural annual premiums by any table of mortality, covering the whole life or a long term of years, are commuted into a constant or level premium, the policy or contract involves, or implies, a banking accumulation, which diminishes, by so much, the risk borne by the company, and is therefore a SELF INSURANCE, causing the company to insure in any year only the difference between it and the face of the policy.[1]

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[1] The paper was presented by its author in print, and copies may be had of him, by application to Box 109, Boston, Mass.

THE GERMAN CARP AND ITS INTRODUCTION INTO THE UNITED STATES.

By CHAS. W. SMILEY of Washington, D. C.

[ABSTRACT.]

THE German carp which has been introduced into the United States is the *Cyprinus carpio* of the family *Cyprinidæ*. There are three varieties: the scale, the most edible; the leather, the most prolific; and the mirror, which is intermediate between the others.

Although a few carp were brought from Holland in 1830, and put in a pond in Newburgh, New York, whence some were washed into the Hudson river, and produced no practical result, the introduction proper dates from May 26, 1877. At that time 345 carp were imported by the United States Fish Commission, under the direction of Professor S. F. Baird. The first distribution from these was of 6,300 fish in 1879, followed by 31,443 in 1880. During the past season 113,605 have been scattered in nearly every state of the Union.

The proper habitat of the carp is a pond of warm water with a muddy bottom, and some suitable aquatic plants. It does not prey upon other fishes or the higher forms of life, but feeds upon crustaceans, seeds, certain leaves of plants, etc. It may also be fed to advantage upon lettuce, cabbage leaves, grain, meal, bread, crackers, and in short, most anything suitable for chickens. Its regular growth depends largely upon its environment. If the climate is suitable, the food abundant, and its enemies scarce, its growth will be surprisingly rapid, reaching easily three pounds in one year or six pounds in two years. If food is scarce and the climate cold, the growth will be less rapid, and from two to four months will be spent in hibernation during which there will be no growth. This fish is consequently exceedingly well adapted to the Gulf States. It may be reared unquestionably in the northern states but more slowly. It is hardy enough to endure an intense degree of cold if the lowering of temperature be quite gradual. Carp usually spawn in cool latitudes when three years old, in temperate latitudes when two years old, and under the most favorable circumstances in southern Texas it is believed to have spawned at the age of one year. Among the enemies of the carp from which it should be protected are all kinds of carnivorous fish, frogs, snakes, turtles, terrapins, kingfishers, bitterns, cranes, herons, and fish hawks. A snake recently killed was found to contain twenty-five young carp and numerous undigested skeletons. A marsh hen was examined whose stomach contained thirty-eight young carp while in a night heron were found the heads of seventy-eight young carp. When about to spawn, one should separate the carp from all other animals in order to protect their eggs and the young until they reach such maturity as to be able to protect themselves. As food, carp is superior to all ordinary native fish such as buffalo, catfish, suckers, mudfish, croakus, mill roach, perch, sunfish, etc. Although by many who are cultivating them, it is declared equal to bass, trout, and shad, the Fish Commission makes no such claim. The flavor of the carp as of all other fish will be affected by its food and environment. If the water is polluted or the food offensive, the flesh of the carp will partake thereof. A carp thus reared should be removed to pure water for a week and kept without food, during which its system will be purified, and the flavor of its flesh immensely improved. Salting over night is also said to largely destroy the muddy taste when present. During, and for a time after spawning, the carp like all other fish, is soft and unsuitable to eat. Most complaints against carp have arisen from eating it at this period. The bones are not offensive as the flesh flakes off very nicely in the adult carp.

Between October and February of each year, young carp are distributed from Washington to those who have previously filed applications. Car loads are sent in the cars of the Commission which are suitably fitted for carrying all kinds of fish to central points. There they are either delivered to State Fish Commissioners for distribution, or are distributed by express to interior parts. The latter method is by means of a tin pail containing two quarts of water in which fifteen or twenty carp will survive from thirty-six to forty-eight hours and even longer if the water is changed meantime, always provided that water enough remains in the pail to cover the backs of the fish. The commissioners of several states undertook to propagate some of the first carp distributed by Professor Baird, and are now beginning to distribute the young which they have reared therefrom. Speculators have also entered the business, one of whose price-lists states that he can sell mirror carp, ten months old, at seventy-five dollars per hundred, and scale carp, ten months old at seventy dollars per hundred. Larger fish have been sometimes sold at five dollars a pair. A gratuitous distribution by the United States Commission has alone prevented prices becoming much larger.

The cultivation of fish is destined to become as important to American farmers and planters as that of cattle, poultry and fruit. The latter left to shift for themselves and to cope with nature come off second best, or succumb. If fish should receive the same care and attention, as to artificial propagation, and the improvement of varieties, the result would be equally gratifying. The practice in carp culture will teach some of the methods, and perhaps foster a taste for the culture of other fish. Every rural community is destined to have its fish ponds as abundantly as its poultry yards. Farmers will then have fresh fish, at will, no matter how remote from market. The carp may be tamed so as to come to a given place for its food at call, and if a shallow place or a suitable platform under the water be arranged, it can be induced even to come there and feed from the hands. It can then be captured easily. Success in carp culture, like success in any other useful enterprise, comes not of itself, but as a reward for careful attention.

OYSTER-FARMING IN CONNECTICUT. By HORACE C. HOVEY of Minneapolis, Minn.

[ABSTRACT.]

FOR many years oyster-dealers obtained their supplies from what are termed "natural beds." These are found of various dimensions, and usually exist in the brackish water at the mouths of rivers, such as the Quinnipiac and Housatonic, in Connecticut, and the Delaware, Susquehanna, and other streams emptying into the Atlantic Ocean. The early settlers of New England resorted to the natural beds without restriction, and regarded them as inexhaustible. Their implements were of the most primitive sort, such as shovels, rakes and the familiar oyster-tongs. Dredging by hand and by steam has been more recently introduced. The first law for the protection of oysters was enacted in Connecticut, in 1784, and remained substantially unchanged for the next seventy years. The necessity of a close season during spawning-time was recognized; but each town was left to make and enforce its own rules. When "the law was off," which was usually about the first of November, so vigorous was the assault upon the beds, that the more accessible of them were often scraped clean in a single day. It finally became evident to observing men that this process was ruining the natural beds. For various reasons the oysters deteriorated both in size and quality. This led to the importation of oysters from southern waters, which were found to fatten in the waters of Long Island Sound, although not propagating there. Capt. Eliphaz Gillette, sixty years ago, brought the first cargo of Virginia oysters to Fair Haven, Conn.; thus originating a trade that grew until, in 1858, there were 250 schooners engaged in it, with an average carrying capacity of 3,000 bushels each, and, according to De Broca, they handled annually 2,000,000 bushels of oysters. About that time (in 1856), Mr. D. D. Mallory opened his wholesale house in Detroit, and that was the beginning of the interior trade in shellfish in the United States. He subsequently fixed his headquarters in Baltimore, and, together with other men from Fair Haven and native Baltimoreans, worked up the national oyster trade. The immediate tendency of this, combined with other causes, was a steady decline in the business, so far as Connecticut was concerned. There was also a marked decline in the productiveness of the great natural beds of the Delaware and Chesapeake Bays. This made it desirable to introduce systematic oyster-culture to meet the constantly increasing demands of the market.

In this connection mention should be made of the brilliant experiments conducted by Prof. W. K. Brooks, Lieut. Francis Winslow, U. S. N., and others, who have tried, with some success to facilitate and regulate the propagation of the oyster, so as to enable the old and exhausted natural beds to be restocked.

My purpose, however, is to describe the more practical, and perfectly successful plan of oyster-farming; by which is meant the cultivation, on a large scale, of ground outside the natural beds. Let me first refer again to the preliminary experiments at transplanting southern oysters to northern waters. Within the last five years nearly seven-eighths of the oysters commercially handled at Fair Haven were what are known as Virginia plants. The process of transplanting was attended with risks and expense, but the profitable result was that oysters which were tough and tasteless while growing in still waters, became improved both in size and flavor by lying for a few months in running waters or on springy bottoms, like those at the mouths of the streams emptying into Long Island Sound. Then again, while the native beds, which are public property, are preyed upon by swarms of enemies, such as the skate, winkle, drill and star-fish, these pests may, to some extent at least, be removed from lots under the control of individuals. Hence originated a series of acts passed in successive years by the legislature of Connecticut, unlike those of any other state, except as they have been recently imitated, and that appear to be fragmentary and disjointed, until one understands the emergencies they were intended to meet. The popular feeling was that whatever grew in the sea was common property. Planted beds were treated as if they were natural and open to all. No title could be held to submarine property. So far as can be learned, the first artificial bed of natives was cultivated by Mr. Willard Mallory, about fifty years ago, on the West Haven flats. Having raised several hundred bushels, his unscrupulous neighbors gathered in the harvest by night for themselves. This was regarded merely as a rough joke, for which there was no legal redress. The first step toward a better state of things was the legal recognition of riparian rights, by which land-owners along the shore were protected in planting as far as low-water mark; or in estuaries, as far as the channel of the stream. In 1855 a law was passed allowing towns to designate to each applicant two acres for planting oysters. In 1875 growers were allowed to buy more than two acres, provided they did not infringe on natural beds. This made oyster-farming possible.

It may be stated here that Connecticut is probably the only state that grants permanent deeds of this description. The system in Massachusetts is to license oystermen for a limited term; and the regulation of the business is mainly left in the hands of the selectmen of the several towns. In Rhode Island, oyster grounds may be rented at the rate of $10.00 per acre for a limited term. New York, Virginia and Maryland, while enjoying vast and lucrative oyster grounds, grant no permanent deeds, but restrict growers to small tracts for immediate use, and not exceeding five acres to one citizen. Delaware, since 1881, leases ground in deep water, on terms favorable to oyster-farming.

In 1878 a special commission was appointed by the states of

New York and Connecticut to fix the boundary line running through the Sound. This division, ratified by the U. S. Congress, and geographically fair, left to Connecticut a marine territory of 565 square miles, including nearly all the best oyster-ground in the Sound. The average depth over the whole area, not including bays and estuaries, does not exceed seventy-two feet. The portion west of Norwalk averages about sixty feet; east of Norwalk to the Connecticut river is a strip sixty miles long by four miles wide, that averages a depth of but forty-two feet. To the south of Stratford is a plateau including sixteen square miles, only twenty to twenty-five feet deep, and entirely free from mud and slime.

In 1879, an oyster-commission was appointed to inquire into all the bearings of the oyster question, and devise a uniform system of laws. It was found that the available flats and shallows belonging to the several towns had been taken up, while a few bolder spirits had ventured to stake out lots in the deeper waters. Conflicting grants had, in some cases been given, and there was serious danger of matters becoming entangled in inextricable confusion. Hence, in April 1881, the state conferred on her Fish Commissioners the power to act also as Shellfish Commissioners, and to adopt a plan for utilizing and distributing the state oyster-grounds. They ran an inshore line from point to point, within which the grounds should remain under town control. The most accurate survey possible was next made of the territory lying between the inshore line and the state line, involving 118 triangulations, the triangulation sides varying from one to seventeen miles in length. This work is so exact that the Commission are able to define promptly the limits of any oyster-ground within the state jurisdiction. Mr. Bogart, the surveyor, informs me that the entire area includes 325,000 acres, of which about 15,000 acres are rocks and reefs, while a large portion, perhaps two-thirds, of the remainder is fit for oyster-cultivation with modern appliances. The entire acreage thus far apportioned is 100,000 acres.

In order to appreciate this rapid stride, one has only to reflect that, according to the Census of 1880, the total acreage of Connecticut oyster-grounds included but 6,934 acres. And now, three years later there are several individual oyster-farms each exceeding that large area! The largest of all belongs to Mr. Henry C. Rowe, who holds deeds for more than 18,000 acres. The whole number of oyster-farms staked out during the last three years is

376, varying in size from two acres to the large tracts already mentioned.

The natural beds have also been surveyed. Their area does not exceed 5,000 acres in all. They are free to all citizens of the state, and are now chiefly resorted to for what are termed "seed-oysters," *i. e.*, those not more than two years old, which age is regarded as the best for transplanting. All other ground is for sale at the nominal price of one dollar an acre, with ten cents added to cover expense of designation. If not cultivated within the next five years, it is forfeited. If found, on trial, unsuitable for cultivation, the money is refunded. Farms are taxed, but the oysters produced are exempt.

The oyster-farmer having secured his ground next cuts it up into smaller lots in order to till it systematically. This is done, of course, by buoys in deep water, and by stakes in shallow. Next he carefully examines the nature of the bottom. Much of the bottom of the Sound is smooth, hard sand. Here and there is a patch of gravel, cluster of rocks, or a bit of muddy ground. It is to be noticed, however, that there are no oysters, and his problem is to make them grow. Oysters cast their spawn at various times between March and November, but mostly in July and August. Each female contains from ten to sixty million eggs. Few of them, proportionally, mature. The young oysters are for a few days free floaters, and then they are ready to be attached to any clean substance that may offer. The plan has been tried in the vicinity of Groton, in the Pequonnock river, of thrusting down small trees, white birch being preferred, letting them lean at an angle of forty-five degrees, sloping with the current. The young oysters set on these very readily. They are harvested when mature by the simple process of pulling up the bushes and stripping off their singular fruit. As many as 1,000 bushels of superior oysters have been gathered from an acre in a single season! There are objections, however, to bush cultivation. Oysters will set on almost any object that they come in contact with. Clean gravel is preferable to anything else, where it can be had. Old, dry shells are generally used for the purpose; and the fact is significant that, whereas they were to be had gratis for road-making, five years ago, they now command from four to eight cents a bushel. That is as much as veteran dealers used to pay for live oysters by the cargo! Shelling begins in June and continues into August. In shoal water spawning is earlier than in deep, owing to difference in temperature. Planters average about 300 bushels of shells to an acre, and scatter among them spawners in the proportion of 100 bushels of spawners to 1,000 of shells. Some idea may be had of the magnitude of the business from the fact that Mr. H. C. Rowe has planted this season more than 300,000 bushels of shells, at a cost of $25,000! Crops correspond to the planting. The Commissioners say, "One cultivator alone looks for no less than one million bushels of marketable natives from his own ground this year!" Yet there are possible reverses. One firm lost last year from a single bed $20,000 worth of oysters by the ravages of starfish. Owing to the same cause, as I am informed by Mr. Bogart, nearly all the natural beds have proved a failure this year, so far as large specimens are concerned.

All the large farms are now cultivated by means of steam. This advance on the old methods was led by Capt. Decker, of Norwalk, who put a donkey-engine into his sloop, the Early Bird, for the purpose of turning the drums on which the dredge-lines were hauled. That was in 1867. The boat is still in service, rigged as a steamer, hauling two dredges, and having a capacity of 200 bushels. Mr. W. F. Lockwood constructed the first steamer built expressly for the oyster business. Mr. Joshua Leviness, of City Island, endeavored to introduce the same improvement into the New York waters, but a law was passed forbidding steam-dredging in that state. Efforts to secure similar prohibitory action in Connecticut were unsuccessful. There are now thirty or more steam-dredgers at work there, with a carrying capacity of from 200 to 2,000 bushels each. The largest catch made in one day was by the steamer Smith Brothers, namely, 1522 bushels in six hours in water forty feet deep.

The traffic in seed-oysters (two-years old) has lately become very lucrative. They go to New York, Rhode Island and elsewhere outside the state, and command from fifty cents to a dollar a bushel. A single firm sold this year about 100,000 bushels. They are used for planting. The third year of an oyster's life witnesses its most rapid growth. I have known instances in which seed-oysters have increased in six months from a length of two inches to a length of five inches! Hence buyers, as well as sellers find it a profitable business.

Comparatively small oysters are preferred for the foreign market;

while the demand for large ones is constantly increasing in this country. Four grades only are recognized by the oyster growers: viz., "cullentines" which are from two to three years old; "culls," three years old; "boxes," from four to six years old; "extras," from five to seven years old. Those over seven years of age are generally tough and undesirable. The oldest specimen I have seen was estimated to be twenty-five years of age. Culls and cullentines are shucked and canned. Boxes and extras are shipped in the shell in barrels tightly packed. The export trade is in its infancy; yet there were 500,000 bushels of oysters in the shell shipped from Connecticut to Europe last year.

NOTE:—In connection with the above paper, Mr. Hovey exhibited specimens of living oysters varying from one day up to twenty-five years of age. He also, by the courtesy of the Shell-fish Commission, exhibited their official chart of the oyster-grounds of Long Island Sound.

IMPROVED METHOD OF SPRAYING TREES FOR PROTECTION AGAINST INSECTS. By C. V. RILEY of Washington, D. C.

[ABSTRACT.]

THE paper gave a summary of results obtained from experiments made during the past two years at the U. S. department of agriculture. An ordinary barrel is used as a reservoir, in which is inserted a force-pump with automatic stirrer. A long rubber hose extends from the pump, and is attached to the spraying apparatus. The nozzle has been called the cyclone or eddy nozzle; its action carries out new principles of spraying. It is a shallow, circular, metal chamber, with two flat sides, in the centre of one of which is a small circular outlet. The fluid is forced into this chamber tangentially, producing rapid rotation, and a spray which is easily regulated from a mist scarcely visible to a strong shower according to the size and form of the outlet. This nozzle is adjusted to the end of a bamboo rod (of varying length, according to requirement), through which the rubber hose has been passed; or several nozzles may be attached, in different positions, to the side of a stiff metal

tube, sufficiently slender to be handled by the operator, and thrust among the branches of the tree. The metal tube is made in sections that screw on to each other. By these means, trees from twenty to thirty feet high can be rapidly sprayed without the use of a ladder. The substances used are either London purple, one-half pound, and flour, one pound, in from forty to fifty gallons of water; or Paris green, one pound, to the same amount of flour and water; or petroleum emulsions made as the writer indicated at the last meeting of the association.

CABLE-CARS FOR CITY PASSENGER TRAFFIC. By E. T. COX of New Harmony, Ind.

[ABSTRACT.]

THIS paper described the success of the cable system as a substitute for horse-cars; and urges its general adoption, on the ground of its convenience and comfort to humanity, and the diminution of suffering to the horse. Numerous statistics were given relating to the number of horses now employed.

A NEW SYSTEM FOR THE TREATMENT AND REMOVAL OF SEWER GAS, THE VENTILATION OF BUILDINGS, AND UTILIZATION OF HEAT. By T. E. JEFFERSON of Hudson, Wis

[ABSTRACT.]

THIS system chiefly consists in making sewers approximately air tight, by sealing the sewer inlets, so as to admit sewage but exclude the air, and by making suitable pipe connection between sewers and buildings and different heating apparatus, arranged to permit the inflow of atmosphere, and the products of combustion into the sewer, and at the same time prevent the backflow of gas, when by the connection of a powerful suction apparatus with the

sewer near its outlet, the removal of sewer gas, and smoke from furnaces and fires and the thorough ventilation of buildings, is positively effected and regulated as desired. By employing mechanical force for creating draft for fires, the large per cent of heat heretofore required for this purpose is retained, effecting a corresponding saving in the consumption of fuel. The main portion of these important discoveries, including the removal of sewer gas and the positive means for ventilating buildings and cities, and carrying the vitiated atmosphere and poisonous vapors away from contact with the people, were recently made by Hon. John Comstock of Hudson, Wis., and first introduced in one of the districts of Paris, France, the present year, where its great utility and success are fully demonstrated and established.

"HEALTH FOOD." By STEPHEN S. HAIGHT of West Farms, New York, N. Y.

EXECUTIVE PROCEEDINGS.

REPORT OF GENERAL SECRETARY.

THE THIRTY-SECOND ANNUAL MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

IN accordance with the action taken at the Montreal meeting in 1882, the AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE met in GENERAL SESSION in the Chapel of the State University of Minnesota, in East Minneapolis, at 10 A. M., August 15, 1883; the retiring President, Dr. J. W. DAWSON, of Montreal, in the chair.

On calling the meeting to order, Dr. Dawson completed his official duty as President by calling to the chair the President-elect, Prof. CHARLES A. YOUNG of Princeton, N. J.

On taking the chair, Prof. Young said:

Ladies and Gentlemen of the Association:—I should certainly be sadly wanting in all proper appreciation of the circumstances, if, in receiving this chair from my distinguished predecessor, I did not express my sincere and hearty thanks for the high favor you have conferred in electing me to its occupancy.

In taking it, I can only promise what is in my power—not high skill in the conduct of business, nor any thorough knowledge of parliamentary form and practice, but only an honest and patient fidelity to your interests, and strict impartiality. I must rely to a great extent upon the assistance of our permanent officers, and upon your own kind indulgence.

We are met here for an important purpose—to do what we can, in the short interval allotted us, for the "Advancement of Science." To this everything else should be kept subsidiary. Not only in the meetings of our sections, the reading of our papers, and our discussions, but in all our more agreeable occupations here, let us keep an eye single to this aim, and strive so to carry ourselves and so to use our opportunities, that we, and all our friends here, shall be stimulated and encouraged in the honest pursuit of truth.

Prayer was then offered by Bishop CYRUS D. FOSS of Minneapolis.

The Hon. GEORGE A. PILLSBURY, chairman of the Local Committee, then introduced the Hon. LUCIUS F. HUBBARD, Governor of the state of Minnesota, who delivered the following address of welcome on behalf of the state.

Mr. President, Ladies and Gentlemen of the American Association for the Advancement of Science:—I trust that you have already realized the fact that you are cordially welcome in our midst. We assume that one of the first impressions that a stranger forms here is that he has come among a people that are glad to greet him, and expect him to make himself thoroughly at home. Such is our hope in your case, and we shall regard it with disappointment if it shall be necessary to make a formal tender of our hospitality in order to make you feel that it is freely yours. We welcome you to our state, and we feel honored by your presence. We recognize in your association a body of ladies and gentlemen who are devoting their talents and energies to the advancement of the greatest material interests of our race, and whose efforts should command the cordial recognition and reward which they deserve. The achievements of science in this generation are an epoch in the history of the world. These achievements have wrought essential changes in the material condition of man. We hardly approach to a realization of the latest scientific wonder before we become almost transfixed by some new surprise. Scientific investigation has proven a most inviting field for the labor, and commands the service of the brightest intellect and genius of our time. At the same time it has returned a rich harvest to the laborer and has dispensed untold and unmeasured benefits to mankind. We expect to derive much advantage from your presence at this meeting. The great northwest offers, no doubt, a grand field for scientific inquiry, and will furnish ample material to illustrate your investigations. We shall seek to profit by your deliberations here in common with the country at large, which will regard your proceedings with an interest commensurate with their importance.

The Hon. A. A. Ames, Mayor of Minneapolis, was then introduced and in the following words welcomed the association on behalf of the city:

Ladies and Gentlemen of the American Association for the Advancement of Science:—You have honored the city of Minneapolis by selecting it as the place for this your thirty-second meeting. Our citizens are not unmindful of the honor conferred. On behalf of our people, I bid you welcome, and I take pleasure in extending to each of you the freedom and hospitality of our city. You will find them as broad as our streets and as cordial and courteous as men with western ideas can make them. You must not suppose, gentlemen, in looking over our city casually, that we are devoid of science — that we are wholly given up to a chase after trade dollars. Here, in the West, we combine business with pleasure. Many of us took Horace Greeley's advice, and here we are grown up with the country. Our science consists of means devised for tilling the vast and fertile prairies which surround us, levelling the gigantic pines of the north, and building mammoth mills, workshops and blocks, and constructing beautiful homes. You find no ancient castles or fortresses here: everything wears a modern air. We, as a city, have sprung into existence as if by magic and we stand before the world one of the wonders of the nineteenth century. Within my own observation Minneapolis has grown from a wild waste inhabited by Indians and wolves, to be a city of 100,000 people. This comes of western energy and western thrift. Here you have a problem not solved by any known scientific formula. It is a case which, like the great trees of California and the wonders of the Yellowstone region, still remains to invite the theories of scientists.

Again I welcome you to our city and homes. You will find the latch strings hanging out, and plenty of hospitality and cheer within. If, among your other scientific researches, you will solve and make plain to us the science of local government, you will confer a lasting favor, and for your labor you shall not go unrewarded. Between your hours of research we invite you to see how we have harnessed up old St. Anthony's Falls and utilized the great water power of the Mississippi river; to go through our mills, manufactories and workshops and see the results of skilled labor and improved machinery; to visit our beautiful lakes and pleasant resorts and view the most gorgeous scenery ever spread before mortal eyes; to see Minneapolis and her people as they exist in their primitive sereneness. Gentlemen, I bid you make yourselves at home among us.

In behalf of the Local Committee, COL. WM. W. FOLWELL, President of the State University, made the following address:

Mr. President and Gentlemen of the American Association for the Advancement of Science:—His Honor the Mayor, and His Excellency the Governor of the state, have bidden you welcome to our city and commonwealth in due form, with fitting words. You are accordingly welcomed. Yet, I am commissioned to offer you still further welcome. This surprises, perhaps amuses you. It did, I confess puzzle me at first not a little, to guess what my townsmen intended when they charged me with this duty. A little reflection resolved my doubts and convinced me that my colleagues of the local committee, acting for our good people of Minneapolis, knew what they were about. The chief magistrates of the city and state would give you as in duty bound, their formal official salutation, which you, being all Pickwickians, might interpret in the Pickwickian sense: as the prudent traveller interprets that oriental courtesy which lays at his feet the house, equipage and whole wealth of his host. Our people wished more than this; they meant more than to open to you the doors of the capitol, the city hall, the police court, the waterworks and other public institutions. So they send one of their number in no official capacity or relation at all, simply as one of themselves, to clap atop of the most polite yet official greetings of the Mayor and His Excellency, a hearty, informal, old fashioned " how d'ye do," and beg you to " make yourselves at home with us." Accept then, I beg you, the greeting and welcome of the people of Minneapolis.

I think it is further expected of me that I attempt some explanation, I need not say apology, of the condition of things you must already have observed in our city. Look where you will, you see the streets blocked by piles of building material and the throng of loaded wagons. Here are sewers in construction, there pavements. Hotels, stores, manufactories, dwellings, are rising in all quarters. Vast railway improvements are in progress to meet the demands of a commerce already colossal. You wonder, I am sure, that there should be any city at all, where so lately the terrible Dakotas were masters of the soil. We Minneapolitans live in a chronic state of amazement, as wealth and population flow in upon us, as suburb after suburb springs up, and vast structures lift themselves in our central precincts. As for population, most of us have lost the count, but no one, I think, would place our number at the next census in 1885 below one hundred thousand souls.

For this magnificent natural development the causes are not far to seek. On the part of nature they are threefold: first, a vast area of arable land of great natural fertility, unincumbered by forests, ready for the plough, lying behind us in our own state, in Dakota and the territories beyond; second, the great pine forests, covering the broad plateau between us and the Canadian line, whence flow the Mississippi and his upper tributaries; third, the Falls of St. Anthony furnishing, it is estimated, the largest available water power of the world. All these, however, would avail little but for the intelligence, foresight, enterprise, faith and trust of men. It is these human qualities which have brought hither the capital of older states and nations to spread over the wheat lands of the great northwest a vast network of railways in advance of population. The faith which opened these vast lines of travel has not been vain. Year by year an increasing, ever-augmenting tide of immigration, chiefly of North European peoples, sober, austere, virtuous and deeply religious, has been flowing past our doors to settle on the prairies — God's reservation for the free and the poor. Such a movement of population has not been seen since the days of Theodoric. Towns and villages by hundreds spring up as if by magic, and, yearly, millions of acres of virgin soil spread their golden harvests to the unaccustomed heavens.

The same apparatus which distributes immigration follows it up with those supplies which civilized man cannot forego, and returns to the markets of the world the products of the territory occupied. To intercept at this point the flow of cereals from the wheat lands, to assemble here the multitudinous drives of pine logs banked each winter in the Mississippi pineries and by means of the power of these falls to convert them into food and building materials, no philosopher, economist or legislator was needed to suggest. The voluntary principle was all-sufficient, and already for years the rumble of the flour mill, the buzz of the circular saw and the keen whirr of the planer have drowned the thunder of the Father of Waters.

Under these circumstances, Mr. President, you will not, I am sure, look for the finish and elegance of the older and forwarder cities. It would be impossible for us, after the fashion of the spry housewife, to spring about and suddenly "tidy up things" for your visit. The bricks and mortar, the stone and timber which bar or embarrass your passage cannot be removed till they are built into the structures whose foundations you see on your right hand and on your left.

But I should do a wrong to my city were I to leave with you the impression that we are so overwhelmed and engrossed with our material labors as to have no care for things of the mind and the higher life. If that were true, why should we welcome with so much sincere ardor the assembling of your association? From the villages of New England and the farmhouses of the middle states our people have brought that perennial curiosity, that thirst for knowledge, that intense though sombre imagination, which have given American civilization and American literature a cast and hue of their own. I must, in a word, praise our system of public schools, both city and state, which under able management and a steady popular support cannot, we believe, be ranked below those of any community of our size in the Union. Minnesota is the first state which has organized its secondary as well as its primary and superior education, and offers to every child in the state a free course of studies, from the alphabet to the degree of master of arts. Our churches, goodly in size and number, may speak for the interests of religion. The future will attest the diligence and the fidelity of those who love music and the sister arts, of whom our older cities might be proud.

It is thus, however, Mr. President, that we Minneapolitans, alert, preoccupied, pause in the midst of our labors to welcome your already venerable association.

We hail you as the survivors of a generation of great investigators, the Sillimans, the Baches, the Henrys, the Morses, the Rogerses, who have made their country famous and their own names as imperishable as science herself. We hail you as the worthy successors of such a generation, perpetuating and enlarging their work.

In common with civilized people we recognize the immense debt of the modern world to science; yet often no doubt when we are filling the sky with applause to some lucky inventor we are not remembering the years, perhaps generations, of inconspicuous and painful labors, carried on in dim studies and laboratories which made the invention possible. Let the inventor have his glory and his profit without envy and without stint, but let us not forget to build the cenotaph of a thousand nameless geometers, star-gazers and natural philosophers, who working in silence and obscurity, without thought of fame or hope of reward, put it in his power to bless and captivate the world.

We are grateful, therefore, to science for the telescope and the microscope, for chloroform, for the photograph, for all the marvellous applications of electricity. To science we owe that magnificent apparatus of transportation which is the crowning and distinctive feature of modern material life. By its means men and goods flow in vast masses up and down and around the world. These movements are like to the currents of the ocean, the flow of mighty rivers and the majestic march of the heavenly bodies. To science we owe the thousand appliances which yield comfort and even elegance to the humblest household.

Immense as are these contributions of science to material comfort and happiness, she has, I still think, performed greater services to mankind. The scientific method developed in the study of nature has spread to all branches of investigation. It has permeated all our education, it has boldly leaped the boundary between physics and metaphysics. It has even penetrated into industry and business and common life. The modern man first collects what knowledge he can about his enterprise or adventure and assures himself of their value. He then makes the best guess he can in regard to the future. Then he assembles new facts and as the facts require, amends his theory, till at length it becomes a working rule, maxim or principle. He knows not merely how to know but how to guess. The penetration of the scientific method into the operations of trade in great commercial cities is very conspicuous. We endeavor to gamble scientifically. No Drew or Armour or Gould ever forms his corner without a most careful study of the situation, and his venture is his bet on the correctness of his theory. The further extension of the scientific method till it shall become the guide of conduct in the every-day life of all men, is now a chief problem in education.

In the next place I think science may at length fairly claim to have wrought out under great difficulties a working hypothesis of our universe in the nebular hypothesis and its almost necessary corollary, "evolution." It cannot be denied that we are all in some sense evolutionists, some of us against our prepossessions, some of us by insensible but progressive lapses. I am not competent to argue on this great theme. But I feel bound to admit that the evolution doctrine in one form or another has quietly taken possession of the modern mind. Why may we not gladly accept it as a most useful working hypothesis of the mode of creation? I say of the mode of creation, for that mystery of creation itself will forever mock the powers of man. Only this we know, that unless human consciousness is a juggle and human language a mockery, there can never be to man a creation without a creator, nor an evolution without an evolver.

Another great service of science is the maintenance in the world of a body of men, a lay priesthood, devoted to the search for truth for its own sake and its own value. In a mercenary age, when in the opinion of a distinguished contemporary, mercantilism has become a disease and excrescence on society, the example of such a body of men is of supreme value in the training of the new generations. Youths are formed, a wise Greek has taught us, not so much by schools as by the example of distinguished men. To welcome you as a body of earnest lovers of and seekers after truth, from the love of it and your kind, would be well worth our while, were it our only motive to impress and inspire the children and youth of our city. In doing you honor we give them a lesson no books nor masters can impart. For their sake we then renew our welcome.

A still greater benefit of science to mankind is the emancipation it has wrought for us in the last generations from superstition and the dominion of imaginary powers. It is no long time since it was generally believed by civilized men that human affairs were under the control of the spirits of the air, good or evil. Men walked in cringing terror by day and night, of demons and goblins damned. The earthquake, the tornado, the lightning's stroke, they looked upon as instruments of punishment for the sins of rulers and peoples. Thanks to science the modern world has emerged from this cloud of gloom.

We have some certain knowledge. Our knowledge is not merely qualitative, but quantitative. Truth ever makes free. Above all, we know that all things in nature are governed by law — law, "whose seat is the bosom of God, whose voice is the harmony of the world." The beautiful conception of the Greeks of the universe as a kosmos, that is, an embodiment of divine and all-pervading order, is the grand presupposition of modern thought. We now know that the phenomena of nature have no relation to human conduct, the impartial rain falling alike on just and unjust. Men walk the earth erect and free, fearing no bogies nor demons. How vast and how blessed the relief to childhood!

In dispelling superstition, science has incidentally wrought her greatest services to mankind in the purification of religion. The time is coming when grateful thanks will be rendered by the ministers of religion for the emancipation which science has wrought for the faith, and then the conflict of science and religion will only be remembered as the antagonism of crude theories on the one hand and cruder superstitions on the other.

Grateful we are for all the knowledge which science has collected and collated, and perpetuated to our use. All honor to the men who are consecrated to truth in her service. We may not know what marvels, far surpassing all the gifts of the past, the science of the future may reveal. Still we must remember that the human mind is finite, while the truth is infinite. The vast unknown engirdles our little circle of light. The mystery of life and death no son of earth has ever penetrated. Welcome all truth, all knowledge then! welcome to the faith which points to the continuance of life in a land where study will be no weariness to the soul, where no veil of flesh will cloud the vision, where science and religion shall be forever one, where men shall know even as they are known!

To the above addresses of welcome President YOUNG responded as follows:

Gentlemen: On behalf of my fellow-members of the association, I return you our sincerest thanks for the hearty welcome we have received to this magnificent state, this young and beautiful city, this vigorous, energetic, warm-hearted community. When you first invited us here it was not in our power to come, but your second invitation we have accepted most gladly, and hope and believe that our meeting here will prove a benefit and pleasure to all concerned.

Some of us have known you personally before, and most of us have long been more or less familiar at second hand with your state and city, and yet I think to many of us it is something like a new revelation to see for ourselves what a few years have accomplished. I am not enough of a Latin scholar to quote my Virgil well, but I have been all the time most forcibly reminded of the passage in which Æneas first comes in sight of rising Carthage—most emphatically "the work boils here." We see no drones or sluggards, but every shoulder is at the wheel and everything is moving.

It may perhaps seem to you sometimes, when in our sectional meeting we discuss some question about the stars, or some hypothesis as to the formation of rock strata, or the structure of some worm or insect, that we are not of the current of life, and contribute nothing to the advancement of the world. But you *know* it is not so, and your invitation to hold our meeting here shows that you know it. The world advances not on one line only, but on many; on lines material, intellectual, spiritual. To some extent the movements are indeed independent, but not very far. Any true advance on either line implies corresponding movement on each of the others if not absolutely simultaneous yet swiftly consequent. There is no need to ask you here how much this city owes to modern science, when I see on every side, in all your streets and storehouses and mills the practical application of the highest engineering, mechanical and electric art. And in the future it is almost certain that science is to contribute still more liberally to business. But not mainly for this reason do I claim your regard to science, but because, made in the image of God as we are, knowledge and understanding are as truly wealth and power as lands and food and money.

I need not add that as you have invited us here, so we on our part most cordially invite you to attend all our meetings and listen to our papers and their discussion. We cannot promise that every paper will be interesting to all, but each one I think will be able to select certain ones he will be glad to hear. And if any one of you choose to join us and enroll yourselves as promoters of the advancement of science, our membership is open on easy terms.

Once more, gentlemen, we thank you for your cordial welcome and address ourselves to our business, in hope and confidence that our meeting here is to be in the highest degree pleasant and successful.

The regular business of the General session was then taken up.

The GENERAL SECRETARY read a list of *seventy-three* persons recommended by the Standing Committee for membership in the Association and they were duly elected.

The PERMANENT SECRETARY read the financial report of the Association.

The PERMANENT SECRETARY also read the following list of twenty members, notices of whose death had been received during the past year.

STEPHEN ALEXANDER, of Princeton, N. J., past president. Joined the Association at the first meeting. Born Sept. 1, 1806. Died June 25, 1883.

LOUIS AUFRECHT, of Cincinnati, Ohio. Joined at the 30th meeting.

Sir REDMOND BARRY, of Melbourne, Australia. Joined at the 25th meeting.

G. W. BELFRAGE, of Clifton, Texas. Joined at the 29th meeting. Died Dec. 7, 1882.

ROBERT BRIGGS, of Philadelphia, Pa. Joined at the 29th meeting.

HORACE BROWN, of Salem, Mass. Joined at the 27th meeting. Died July, 1883.

L. S. BURBANK, of Woburn, Mass. Joined at the 18th meeting.

Paul A. Chadbourne, of Amherst, Mass. Joined at the 10th meeting. Born Oct. 21, 1823. Died Feb. 23, 1883.

Miss **Margaret S. Cheney**, of Jamaica Plain, Mass. Joined at the 29th meeting. Died Sept., 1882.

Josiah Curtis, Washington, D. C. Joined at the 18th meeting. Died Aug. 1, 1883.

Henry Draper, of New York, N. Y. Joined at the 28th meeting. Born, 1837. Died in 1883.

O. H. Fitch, of Ashtabula, Ohio. Joined at the 7th meeting. Died Sept. 17, 1882, in his 80th year.

A. P. Garber, of Columbia, Pa. Joined at the 29th meeting. Died Aug. 26, 1881.

G. S. B. Hempstead, Portsmouth, Ohio. Joined at the 29th meeting. Died July 9, 1883, in his 89th year.

James Orton, Poughkeepsie, N. Y. Joined at the 18th meeting. Died Sept. 24, 1877.

William Schley, of New York, N. Y. Joined at the 28th meeting. Died in 1882.

Joseph S. St. John, of Albany, N. Y., acting General Secretary of the 7th meeting. Died Nov. 23, 1882.

A. F. Taylor, Cleveland, Ohio.

Thomas W. Tobin, of Louisville, Ky. Joined at the 30th meeting. Died Aug. 4, 1883, in his 41st year.

R. S. Williamson, of San Francisco, Cal. Joined at the 12th meeting.

President Young gave a brief account of the life of Prof. Stephen Alexander of Princeton, N. J., who was an original member of the Association and was President in 1859.

After formal announcements by the Permanent Secretary and officers of the Local Committee the General Session adjourned.

On the adjournment of the General Session the Sections met in their respective section rooms to complete their organizations with the following result.

SECTION A.

Vice President: W. A. Rogers.

Secretary: G. W. Hough.

Member of Standing Committee: E. S. Holden.

Sectional Committee: J. R. Eastman, H. T. Eddy, E. W. Hyde.

Member of Nominating Committee: S. J. Coffin.

Sub-committee on Nominations: A. B. Nelson, C. J. Smith, E. Frisby.

SECTION B.

Vice President: H. A. Rowland.

Secretary: F. E. Nipher.

Member of Standing Committee: Le Roy C. Cooley.

Sectional Committee: T. F. Jewell, W. A. Anthony, H. S. Carhart.

Member of Nominating Committee: H. R. Goodnow.

Sub-committee on Nominations: T. C. Mendenhall, C. F. Brackett.

SECTION C.

Vice President: E. W. MORLEY.

Secretary: W. MCMURTRIE.

Member of Standing Committee: C. LEO MEES.

Sectional Committee: W. G. MIXTER, R. B. WARDER, A. P. S. STUART.

Member of Nominating Committee: A. P. S. STUART.

Sub-committee on Nominations: A. SPRINGER, H. H. DINWIDDIE, J. A. DODGE.

SECTION D.

Vice President: DE VOLSON WOOD.

Member of Nominating Committee: C. M. WOODWARD.

Sub-Committee on Nominations: T. R. BAKER, L. L. MCINNIS, W. A. PIKE.

SECTION E.

Vice President: C. H. HITCHCOCK.

Secretary: A. A. JULIEN.

Member of Standing Committee: E. W. CLAYPOLE.

Sectional Committee: J. W. SPENCER, T. C. CHAMBERLAIN, W. UPHAM.

Member of Nominating Committee: N. H. WINCHELL.

Sub-committee on Nominations: E. ORTON, J. F. JAMES, J. W. POWELL.

SECTION F.

Vice President: W. J. BEAL.

Secretary: S. A. FORBES.

Member of Standing Committee: O. S. WESTCOTT.

Sectional Committee: C. E. BESSEY, W. R. LAZENBY, E. D. COPE.

Member of Nominating Committee: H. OSBORN.

Sub-committee on Nominations: D. S. KELLICOTT, J. M. COULTER, W. G. FARLOW.

SECTION G.

Vice President: J. D. COX.

Secretary: W. H. WALMSLEY.

Member of Standing Committee: W. LILLY.

Sectional Committee: W. J. BEAL, T. C. MENDENHALL, W. A. ROGERS.

Member of Nominating Committee: C. V. RILEY.

Sub-committee on Nominations: W. H. WALMSLEY, C. LEO MEES, D. S. KELLICOTT.

SECTION H.

Vice President: O. T. MASON.

Secretary: G. H. PERKINS.

Member of Standing Committee: E. S. MORSE.

Sectional Committee: P. R. HOY, F. W. PUTNAM, N. S. TOWNSHEND.

Member of Nominating Committee: NATHANIEL PAINE.

SECTION I.

Vice President: FRANKLIN B. HOUGH.

Secretary: JOSEPH CUMMINGS.

Member of Nominating Committee: C. W. SMILEY.

Sub-committee on Nominations: J. R. DODGE, WM. W. FOLWELL, JOHN HICKS.

The addresses of the Vice Presidents were given as follows:—

Vice	President	ROGERS	before	Section	A	at	2.30	P.	M.
"	"	HITCHCOCK	"	"	E	"	2.30	"	"
"	"	HOUGH	"	"	I	"	2.30	"	"
"	"	ROWLAND	"	"	B	"	4	"	"
"	"	BEAL	"	"	F	"	4	"	"
"	"	MASON	"	"	H	"	4	"	"

No addresses were given by the Vice Presidents of Sections C, D, and G.

The address of the retiring President, Dr. J. W. DAWSON, was given at 7.30 P. M. in the Westminster Church.

After the address of Dr. Dawson the LOCAL COMMITTEE gave a reception to the Association in the parlors of the Nicollet House.

THURSDAY, AUGUST 16.

The Association met in GENERAL SESSION at 10 A. M., President YOUNG in the chair.

The PRESIDENT announced that Section C would hereafter meet in Room 89 on the second floor of the University and that a meeting of Botanists would be held in the room of Section F at 2 P. M. on Thursday.

The LOCAL COMMITTEE gave notice that arrangements had been made to enable members of the Association to visit the lumber and flouring mills of Minneapolis on Saturday morning.

The same committee also announced an excursion for members of the Association on Saturday to Lake Park Hotel and Lake Minnetonka, via the Minneapolis and St. Louis railroad, leaving Minneapolis at 11 A. M.

A list of twenty-nine persons which the Standing Committee had recommended for membership in the Association was then read by the GENERAL SECRETARY and they were duly elected.

The following communications were read by the General Secretary.

NORTHWESTERN DIVISION ERIE TELEPHONE AND TELEGRAPH CO.
MINNEAPOLIS, AUGUST 14, 1883.

PROF. N. H. WINCHELL, Secretary of the Local Committee for the Minneapolis Meeting:

DEAR SIR: During the session of your Association we shall be pleased to extend the service of our local exchange to members and guests and would appreciate an inspection of our operating department by any of your people.

Yours truly,
ARTHUR S. TEMPLE,
General Superintendent.

YOUNG MEN'S CHRISTIAN ASSOCIATION,
MINNEAPOLIS, MINN., AUG. 9, 1883.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

GENTLEMEN: We very cordially invite you to visit and inspect our new quarters in the Syndicate Block 519-521 Nicollet Avenue, at your leisure during your sessions in our city. Our rooms are open daily to visitors from 9 A. M. to 9.30 P. M. The Secretary, or his assistant, will cheerfully give information or show any attention within his power.

Yours truly,
H. E. FLETCHER, Pres.,
JOHN H. ELLIOTT, Gen. Secretary.

The General Session then adjourned.

FRIDAY, AUGUST 17.

The Association met in General Session at 10 A. M., the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of thirty persons recommended by the Standing Committee for membership in the Association and they were duly elected.

The following communication was read by the GENERAL SECRETARY

ST. PAUL, MINN., AUGUST 15, 1883.

TO THE PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DEAR SIR: At the Meeting of the Board of Directors of the St. Paul Chamber of Commerce held on the sixth instant, a resolution was adopted, appointing the gentlemen whose names are given below, looking to the entertainment of your Association in St. Paul. We sincerely trust that you will give us the pleasure of showing you our city and beg you will name a day for that purpose.

Respectfully,
R. O. SWEENY, Chairman Special Committee,
C. A. MCNEALE, Secretary.

Committee on Entertainment, Messrs. R. O. SWEENY, H. N. ELMER, F. A. FOGG, H. A. CASTLE, L. W. RUNDLETT.

By a vote of the Association the invitation was accepted for Monday afternoon August 20; the members of the Association to leave East Minneapolis at two o'clock.

Several announcements were made by the Local Committee.

In accordance with the vote of the STANDING COMMITTEE, the paper by Dr. T. STERRY HUNT of Montreal, on a Classification of the Natural Sciences, was read, after which the General Session adjourned.

On Friday evening, Prof. F. W. PUTNAM of Cambridge, Mass., gave a lecture illustrated by diagrams and lantern pictures, on "Altar Mounds and their contents," before a large and highly interested audience in the Church of the Redeemer. The lantern was furnished by Dr. E. P. HOWLAND of Washington.

SATURDAY, AUGUST 18.

No meetings of the Association were held to-day. During the morning many members of the Association availed themselves of the privilege of visiting the lumber and flouring mills and found much to admire in the wonderful mechanical improvements which astonished them at every turn.

At 11 A. M. about four hundred members of the Association and guests took an extra train on the Minneapolis and St. Louis R. R. for the Lake Park Hotel at Lake Minnetonka. On arriving at the hotel they were furnished with an excellent dinner by the LOCAL COMMITTEE. After dinner the party embarked on the steamer City of St. Louis, and made the grand tour of Lake Minnetonka. Most of the party landed at Chapman's, and were highly edified by speeches from Mr. D. BLAKELY, the Hon. W. D. WASHBURN and the Hon. IGNATIUS DONNELLY. Returning by the same route the excursionists reached Minneapolis about 6 P. M. highly pleased with the zeal and skill of the Local Committee, and the beauties of Minnetonka.

MONDAY, AUGUST 20.

The Association met in GENERAL SESSION at 10 A. M. the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of twenty-three persons recommended by the STANDING COMMITTEE for membership in the Association, and they were duly elected.

The PRESIDENT announced that the reports of Special Committees were now in order.

The "Committee on Weights, Measures and Coinage," made no report and was continued.

The "Committee to Memorialize the Legislature of New York for a new survey of Niagara Falls" made no report and was discharged.

The "Committee on the best methods of Science Teaching in the Public Schools" reported progress and was continued.

The "Committee on the Registration of Marriages, Births and Deaths," made no report and was continued.

The "Committee on Standard Time" reported that the Railway companies had taken such action as left little for the Committee to do. The committee was discharged.

The "Committee on Stellar Magnitudes" reported progress and was continued.

The "Committee on State Geological Surveys" made no report and was discharged.

The "Committee to confer with committees of Foreign Associations for the Advancement of Science, with reference to an International Convention of Scientific Associations," reported progress and was continued.

The "Committee to confer with a Committee of the British Association for the Advancement of Science on the Annual Records of Science" made no report and was discharged.

The "Committee on indexing the Literature of the Chemical Elements reported through its Chairman that a paper reporting progress would reach the Association before the day fixed for adjournment. The Committee was continued. [The report was received and is printed on p. 147 of this volume.]

The "Committee on a Primary Meridian and an International Standard of Time" made no report and was discharged.

The "Committee on International Congress of Geologists" reported progress, and was continued with power to add to its number.

The "Committee to confer with the United States Geologist in regard to coöperation between the Government and State Geological Surveys," reported progress and was continued with power to add to its number.

The following Constitutional amendment proposed at the Montreal Meeting was then read and adopted by a vote of the Association.

Add to ARTICLE 7 of the Constitution the following:—

"The Standing Committee shall have power to exclude from the Association any Member or Fellow, on satisfactory evidence that said Member or Fellow is an improper person to be connected with the Association, or has in the estimation of the Committee made improper use of his membership or fellowship."

The following report of the Committee on interchange of courtesies between the American and the British Associations for the Advancement of Science, in 1884, was read by the GENERAL SECRETARY.

The Committee report the following recommendations:—

That a cordial invitation be extended to the members of the British Association to attend the meeting of this Association, and take part in its scientific proceedings.

That in order to facilitate the exchange of courtesies, the meeting of this Association for 1884 begin on September 3. [Afterwards changed to Thursday, Sept. 4.]

That the committee on the Berlin Congress of Geologists be requested to correspond with the officers of the Congress and of the British Association with the view of arranging for such a date as may be suitable to the delegates from the two Associations.

That a committee be continued to attend to such matters as may arise in connection with the meetings of the Associations.

The Association voted to accept the report and to adopt the recommendations, leaving the date of the meeting to be recommended by the Nominating Committee.

The following committee was appointed and continued in accordance with the report: C. A. YOUNG, J. P. LESLEY, J. W. DAWSON, T. STERRY HUNT, E. S. MORSE, T. C. MENDENHALL, F. W. PUTNAM.

The report of the Committee appointed to consider the question of duty on scientific books, was then read. On motion of Hon. J. D. Cox it was voted that a committee of five be appointed in accordance with the recommendations of the report to present the matter to Congress.

The following committee was appointed: H. A. ROWLAND, C. A. YOUNG, J. W. POWELL, J. D. COX, E. S. MORSE.

The following report of the Committee on Privileges of Members of the Association, with respect to meetings, was read, and adopted by vote of the Association.

That every member of the Association shall have the privilege of registering members of his family, not including men over twenty-one years of age, by paying the sum of three dollars for each person so registered, and shall receive for them specific badges which will entitle the holder thereof to such privileges as may be extended to the members generally by the local committee for the meeting.

The NOMINATING COMMITTEE were notified to meet at the Nicollet House at 9.30 P. M. Monday.

After several announcements by the LOCAL COMMITTEE a paper on The Evidence for Evolution, in the History of the Extinct Mammalia, was read by Prof. E. D. COPE of Philadelphia.

On the completion of Prof. Cope's paper the General Session adjourned.

MONDAY AFTERNOON.

Soon after lunch those members of the Association who accepted the invitation to visit St. Paul took a special train of Chicago, Milwaukee and St. Paul R. R. cars for the excursion. The train stopped at Minnehaha Falls giving ample time to see this beautiful and historic sheet of water, and after several considerable delays the party arrived safe in St. Paul where they took carriages provided by the citizens' committee and drove to the State House. Here, speeches of welcome were made by Mayor C. D. O'BRIEN of St. Paul, Col. EDWARD DANIELS of St. Paul and Governor L. F. HUBBARD. Responses on the part of the Association were made by Pres. C. A. YOUNG, Gen. J. D. COX and Major J. W. POWELL.

The unexpected delay of the train had deranged many of the plans of the St. Paul reception committee, but most of the party were able to see a large portion of the city and reach the Metropolitan and Merchants' Hotels in season for the excellent supper provided by the reception committee. At 8.30 P. M., the return train left St. Paul and soon reached Minneapolis without delay or accident.

On Monday evening, at Plymouth Church, an illustrated lecture on The Fish and Fisheries of the United States, was given by Mr. C. W. SMILEY of the Smithsonian Institution. The lantern illustrations were by Dr. E. P. HOWLAND of Washington, D. C.

TUESDAY, AUGUST 21.

The Association met in GENERAL SESSION at 10 A. M., the PRESIDENT in the chair.

A list of twenty persons recommended by the STANDING COMMITTEE for membership in the Association was read by the GENERAL SECRETARY and they were duly elected.

The following list of members recommended by the STANDING COMMITTEE to be Fellows of the Association was then elected by ballot.

Abbott, Dr. Chas. C., Trenton, N. J. (29).
Anderson, Rev. Joseph, Waterbury, Conn. (29).
Arthur, J. C., Charles City, Iowa (21).
Ashburner, Charles A., 907 Walnut St., Philadelphia, Pa. (31).
Barnard, Edward E., Nashville, Tenn. (26).
Bartlett, Prof. Edwin J., Dartmouth Coll., Hanover, N. J. (28).
Bausch, Edward, Rochester, N. Y. (26).
Billings, Dr. John Shaw, Washington, D. C. (32).
Blackham, Dr. George E., Dunkirk, N. Y. (25).
Burr, Prof. William H., Troy, N. Y. (31).
Carpmael, Charles, Director of Magnetic Observatory, Toronto, Can. (31).
Case, Col. Theo. S., Kansas City, Mo. (27).
Chandler, Prof. Chas. Henry, Ripon, Wis. (28).
Clapp, Miss Cornelia M., Mt. Holyoke Seminary, South Hadley, Mass. (31).
Crandall, Prof. A. R., Lexington, Ky. (29).
Curtis, Dr. Lester, 1558 Wabash avenue, Chicago, Ill. (29).
Davenport, B. F., M. D., 751 Tremont St., Boston, Mass. (29).
Dorsey, Rev. Jas. O., Bureau of Ethnology, Washington, D. C. (31).
Dudley, Prof. Wm. R., Ithaca, N. Y. (29).
Eaton, Hon. John, U. S. Commiss'r of Education, Washington, D. C. (25).
Fairchild, H. L., 153 E. 47th St., New York, N. Y. (28).
Fernald, Prof. M. C., State Agricultural College, Orono, Me. (22).
Fletcher, Miss Alice C., care Peabody Museum, Cambridge, Mass. (29).
Fletcher, James, Library of Parliament, Ottawa, Can. (31).
Freeman, Spencer H., Cleveland, Ohio (29).
French, Prof. Thomas, jr., Urbana, Ohio (30).
Gallaudet, Edw. M., Pres. Nat. Deaf Mute College, Washington, D. C. (28).
Gray, Dr. Elisha, Highland Park, Ill. (32).
Gulley, Prof. Frank A., Agricultural College, Oktibbeha Co., Miss. (30).
Hall, Prof. C. W., Univ. of Minnesota, Minneapolis, Minn. (28).
Halsted, Byron D., 245 Broadway, New York, N. Y. (29).
Hanaman, C. E., Troy, N. Y. (19).
Hardy, Prof. A. S., Dartmouth College, Hanover, N. H. (28).
Holman, Silas W., Mass. Inst. of Technology, Boston, Mass. (31).
Holmes, Wm. H., Smithsonian Institution, Washington, D. C. (30).
Hosea, Lewis M., Johnston Building, Cincinnati, Ohio (30).
Hotchkiss, Jed., Staunton, Va. (31).
Hovey, Rev. H. C., New Haven, Conn. (29).
Jordan, Prof. David S., Bloomington, Ind. (31).
Kellicott, David S., Buffalo, N. Y. (31).
Kershner, Prof. Jefferson E., Lancaster City, Pa. (29).
Kinnicutt, Dr. Leonard P., Worcester, Mass. (28).

Kunz, G. F., care Tiffany & Co., New York, N. Y. (29).
Landreth, Prof. Olin H., Vanderbilt Univ., Nashville, Tenn. (28).
Larkin, Edgar L., New Windsor, Mercer Co., Ill. (28).
Mackintosh, James B., School of Mines, Columbia Coll., New York (27).
Moses, Prof. Thos. F., Urbana University, Urbana, Ohio (25).
Osborn, Henry F., S. D., Garrisons, N. Y. (29).
Payne, Prof. Wm. W., Carleton College, Northfield, Minn. (30).
Pickering, Wm. H., 84 Mt. Vernon St., Boston, Mass. (29).
Robinson, Prof. S. W., Univ. of Ohio, Columbus, Ohio (30).
Rockwell, Chas. H., Box 293, Tarrytown, N. Y. (28).
Rosenspitz, Dr. Alexander, Rabbi, Lock Box 430, Portland, Oregon (26).
Smiley, Charles W., Smithsonian Institution, Washington, D. C. (28).
Smith, Edwin, Ass't U. S. Coast and Geodetic Survey, Washington, D. C. (30).
Upton, Winslow, Army Signal Office, Washington, D. C. (29).
Walmsley, W. H., 1016 Chestnut St., Philadelphia, Pa. (28).
Webb, Prof. J. Burkitt, Ithaca, N. Y. (31).
Wheeler, Prof. C. G., Univ. of Chicago, Chicago, Ill. (18).
Whiting, Miss Sarah F., Wellesley College, Wellesley, Mass. (31).

The PRESIDENT announced that the time had arrived for selecting the place of meeting for 1884 and that the NOMINATING COMMITTEE had recommended the city of Philadelphia.

The following invitation from men of science and from corporate bodies in Philadelphia was then read:—

PHILADELPHIA, AUGUST 1, 1883.

TO THE PRESIDENT, OFFICERS AND MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:

GENTLEMEN: The undersigned being severally authorized to represent the institutions, corporations, etc., set opposite their names, or joining heartily with the former in their individual capacity as citizens of Philadelphia, do hereby cordially invite your honorable body to hold the thirty-third session of the American Association for the Advancement of Science, for 1884, in the city of Philadelphia.

Prof. H. Carvill Lewis, Prof. Edward D. Cope, Dr. Persifor Frazer, Prof. Angelo Heilprin, Rev. Henry C. McCook, D. D.; *Committee appointed by the Academy of Natural Sciences of Philadelphia.*

Daniel G. Brinton, M. D., Prof. George F. Barker, M. D., Prof. J. Peter Lesley, Henry Phillips, jr., Wm. Morris Davis, Esq.; *Committee appointed by the American Philosophical Society.*

Wm. P. Tatham, Esq., Alex. E. Outerbridge, jr., Prof. Edwin J. Houston, Coleman Sellers, Esq., Wm. H. Wahl, Ph. D.; *Committee appointed by the Franklin Institute.*

Prof. Wm. Pepper, M. D., Provost of the University, Prof. Joseph Leidy, M. D., President of the Acad. of Nat. Sciences, S. Weir Mitchell, M. D., Hon. John Welsh, William Sellers, Esq.; *On behalf of the University of Pennsylvania.*

Isaac Lea, LL. D., John L. LeConte, M. D., Prof. Geo. F. Barker, M. D.; *Ex-Presidents A. A. A. S.*

Hon. Samuel G. King, *Mayor of the city of Philadelphia.*

Hon. Wm. Henry Lex, *President of the Common Council of Philadelphia.*

Edward T. Steel, Esq., *President of the Board of Education.*

Prof. L. M. Haupt, Thos. C. Clarke, Esq., Frederic Graff, Esq., Strickland Kneass, Esq., Rudolph Hering, Esq.; *Committee appointed by the Engineers' Club of Philadelphia.*

Henry C. Lea, Esq., J. G. Rosengarten, Esq., Wm. A. Ingham, Esq., Thos. Cochran, Esq., Edward R. Wood, Esq.; *On behalf of the Philadelphia Social Science Association.*

Right Rev. Wm. Bacon Stevens; *Bishop of Pennsylvania, President of the Board of Trustees of the Philadelphia Divinity School, President of the Protestant Episcopal Hospital, President of the University Club.*

Horace Howard Furness, Esq., *President of the Penn Club.*

Prof. Roberts Bartholow, M. D., LL. D., *Dean of the Faculty of the Jefferson Medical College of Philadelphia.*

Prof. Edward H. Magill, *President of Swarthmore College.*

Prof. Geo. J. Ritchè, *President Central High School.*

Prof. A. H. Fetterolf, *President,* Prof. H. D. Gregory, *Vice-President, Girard College.*

Prof. C. N. Peirce, D. D. S., *Dean of the Pennsylvania College of Dental Surgery.*

James L. Claghorn, Esq., *President Pennsylvania Academy of Fine Arts, President of the School of Design.*

John L. Le Conte, M. D., *President American Entomological Society.*

Lloyd P. Smith, Esq., *Librarian, Library Co. of Philadelphia.*

T. Morris Perot, Esq., *President of the Mercantile Library Co.*

George H. Boker, *President,* and Samuel B. Huey, *Secretary of the Union League of Philadelphia.*

Thomas Chase, LL. D., *President of Haverford College.*

George W. Childs, Esq., *Editor of the Public Ledger.*

Hon. A. W. Tourgee, Esq., *Editor of the Continent.*

Col. A. Loudon Snowdon, *Superintendent U. S. Mint.*

Gen. H. S. Huidekoper, *Postmaster at Philadelphia.*

Prof. F. V. Hayden, M. D., LL. D., *U. S. Geological Survey.*

Franklin B. Gowen, Esq., *President,* and Albert Foster, Esq., *Secretary of the Philadelphia and Reading Railroad Company.*

John C. Sims, jr., Esq., *Secretary Pennsylvania Railroad.*

A. J. Drexel, Esq., John Wanamaker, Esq., Geo. Whitney, Esq., Moro Phillips, Esq., James A. Wright, Esq.

The PRESIDENT said that a formal invitation had been received from New York but considering the prior action of the Philadelphia people the New York claim was not urged.

A vote was taken on the selection of the place of meeting in 1884, and Philadelphia was chosen unanimously.

The time of meeting was fixed for the third of September, with the proviso, that the retiring and incoming Presidents and Permanent and General Secretaries for 1883 and 1884 should be empowered to change the date if they deem it desirable. [This committee has found it desirable to change the date to Thursday, Sept. 4.]

The GENERAL SECRETARY read the list of Fellows recommended by the Nominating Committee for officers of the meeting to be held in 1884, and they were elected by a unanimous vote of the Association. The list is as follows:—

President: J. P. LESLEY, of Philadelphia.

Vice Presidents: **A** — H. T. EDDY of Cincinnati; **B** — JOHN TROWBRIDGE of Cambridge; **C** — JOHN W. LANGLEY of Ann Arbor; **D** — R. H. THURSTON of Hoboken; **E** — N. H. WINCHELL of Minneapolis; **F** — E. D. COPE of Philadelphia; **G** — T. G. WORMLEY of Philadelphia; **H** — E. S. MORSE of Salem; **I** — JOHN EATON of Washington.

Permanent Secretary: F. W. PUTNAM of Cambridge.

General Secretary: ALFRED SPRINGER of Cincinnati.

Assistant General Secretary: EDWARD S. HOLDEN of Madison.

Secretaries of the Sections: **A**— G. W. HOUGH of Chicago; **B**— N. D. C. HODGES of Salem; **C**— R. B. WARDER of North Bend; **D**— J. BURKITT WEBB of Ithaca; **E**— EUGENE A. SMITH of Tuscaloosa; **F**— C. E. BESSEY of Ames; **G**— ROMYN HITCHCOCK of New York; **H**— W. H. HOLMES of Washington; **I**— C. W. SMILEY of Washington.

Treasurer: WILLIAM LILLY of Mauch Chunk.

After announcements by the LOCAL COMMITTEE the General Session adjourned.

On Tuesday evening the Rev. H. C. HOVEY of New Haven, Ct., delivered an interesting lecture, with lantern illustrations, on Subterranean Scenery of the Great Caverns of the World, to a full audience in Plymouth Church. Dr. HOWLAND also provided the lantern on this occasion.

WEDNESDAY, AUGUST 22.

The Association met in GENERAL SESSION at 10 A. M., the PRESIDENT in the chair.

The GENERAL SECRETARY read a list of eight persons recommended by the STANDING COMMITTEE for membership in the Association, and they were duly elected.

The GENERAL SECRETARY announced that the STANDING COMMITTEE had adopted the following rule in regard to papers presented for reading before the various Sections. "Hereafter no paper will be accepted by the Standing Committee for reading before any of the sections, unless accompanied by such abstract as the writer deems ready for publication."

The report of the "Committee on indexing the Literature of the Chemical Elements" was then read by the GENERAL SECRETARY. [See p. 147.]

The PERMANENT SECRETARY announced that a lady member of the Association had that morning placed $150 in his hands for the use of the Association. He said that the same generous lady had made a gift of $850 at the last meeting, thus making $1000 in all. These gifts were conditioned on the non-publication of the name of the donor during the meeting, and the Association expressed its gratitude to its modest benefactor by a hearty round of applause.

The PRESIDENT announced that a popular lecture "On the Theory of Evolution in Modern Science," would be given by Prof. E. D. COPE, at the Church of the Redeemer, on Wednesday evening, by request of a number of citizens of Minneapolis.

The regular business of the Session having been completed, Prof. E. T. Cox, addressing the Chair, said:

In presenting the following resolution, I desire to say that, only those who have had some experience in providing for the entertainment of the American Association for the Advancement of Science can fully understand the labor and responsibility which rests upon the Local Committee, and I am sure that all will agree with me that these duties have been satisfactorily administered here in this beautiful and marvellously prosperous city of Minneapolis. There has been no confusion or clashing of views, but every effort has been successfully directed to administer to our wants and make our stay as pleasant as possible. Most fully have we enjoyed the generous hospitality so heartily tendered by the Local Committee and citizens, and returning to our homes will bear with us an enduring memory of their kind efforts in our behalf.

Resolved, That the thanks of this Association are hereby tendered to the Local Committee, Hon. GEO. A. PILLSBURY, Prof. N. H. WINCHELL, Dr. ELLIOT and Mr. D. BLAKELY, and to the Citizens of Minneapolis, for the very handsome and generous manner in which they have provided for our comfort and convenience, and for the cordial reception which we have received at their hands.

The resolution was unanimously and enthusiastically adopted.

Prof. CHARLES H. HITCHCOCK presented the following, which was duly endorsed by the Association.

Resolved, That the thanks of the American Association for the Advancement of Science is hereby unanimously tendered to the Regents of the University of the State of Minnesota for the use of their buildings and the appliances connected therewith for the meetings; also to the President, Dr. FOLWELL, for his constant effort to promote the success of our gatherings, and to the other officers of the institution, and especially to Mr. GALLOW, the janitor and carpenter, for his many kind attentions to the officers and members.

Principal DAWSON, of Montreal, after a complimentary allusion to Minneapolis and Lake Minnetonka, offered a resolution of thanks to Hon. W. D. WASHBURN and the Minneapolis & St. Louis Railroad Company for the excursion to the lake, which he characterized as one of the pleasant events in the history of the Association. He said the visit to the Northwest had been a revelation to most of the members, who found that they had very inadequate and unworthy conceptions of the great and growing section.

Dr. T. STERRY HUNT complimented the citizens of Minneapolis and of St. Paul on the liberality of their railroad companies, and moved a vote of thanks to Gen. W. D. WASHBURN for the use of the steamer St. Louis for the excursion on Lake Minnetonka and to all the companies who had extended so many and so acceptable favors to the Association. He intended to include all the companies, but there were so many of them that he would crave pardon for not knowing all their names. Dr. HUNT made several complimentary allusions to St. Paul and warmly thanked the citizens for the reception tendered by that city.

The vote of thanks was unanimously given.

Dr. STEPHEN D. PEET made a happy allusion to the race between electricity and thought, which had been brought on by the wonderful achievements in the "chaining of lightning," and concluded by offering a vote of thanks to the Telegraph and Telephone companies for their contributions to the convenience of the members.

The vote was heartily given.

Prof. C. V. RILEY, after some complimentary remarks in regard to the ability and vigor of the whole press of the cities of Minneapolis and St. Paul, and of the accuracy manifested in their printed reports of this meeting, presented the following resolution which was unanimously adopted.

Resolved, That the thanks of the Association are especially due to the Tribune of Minneapolis and the Pioneer Press of St. Paul for the carefully prepared reports they have made of the several proceedings of this meeting and of the papers read in the different sections; and to all the newspapers of both cities for the efforts they have put forth in the service of the Association, in forwarding its work and in making its session in Minneapolis alike profitable and agreeable.

THE PERMANENT SECRETARY announced that the Standing Committee had voted copies of the proceedings and papers to each of the academies of science in Minneapolis and St. Paul, to complete their series.

Mr. D. BLAKELY, of the Local Committee, announced that the St. Paul and Duluth Railroad Company and the corporation of Taylor's Falls would give a free excursion and entertainment to such members of the Association as might wish to see the Dalles of the St. Croix. A large number of the members expressed their desire to accept the invitation. On motion of Dr. T. STERRY HUNT the Association passed a vote of thanks to the St. Paul and Duluth Railroad Company, and to the citizens of Taylor's Falls, for their generous invitation.

Mr. GEORGE A. PILLSBURY, chairman of the Local Committee, thanked the Association for the expressions of gratitude and appreciation. He said he hardly thought the Committee deserved the compliments that had been showered upon them. He could say, however, that it had afforded Minneapolis real pleasure to have the honor of entertaining the Association, and that it had been the intention of her citizens to make every possible provision for the proper reception of the distinguished guests. We feel that we have failed to fulfil our desire and your expectations. There has been some friction and misunderstanding, and moreover our business men have been so occupied with their pressing enterprises that they have not had time to give to the meetings the attention they deserved and would otherwise have received. We have had no jealous feeling toward St. Paul and would greatly regret that any suspicion of a manifestation of a bitter and unwarranted rivalry should have been aroused. The two cities are really one. Minneapolis is proud of St. Paul and rejoices at her prosperity.

Mr. Pillsbury alluded to the many important matters that are now engrossing the attention of Minneapolitans, mentioning especially the new Sault Ste. Marie railroad project. He said Minneapolis was thoroughly in earnest in this matter and was bound to accomplish the end sought. He concluded by calling on Dr. Wm. W. Folwell to respond further to the resolutions offered.

Professor **Folwell** said on behalf of the Regents and Faculty of the University, that they had simply been glad and proud of the opportunity of placing the buildings, library and collections at the disposal of the Association. They would be content to have every plank in the floors worn out by the tread of the members' feet. To be worn out in good service is the true end of men and materials. He spoke of the faithful labors of Professor N. H. Winchell upon whom as Secretary of the Local Committee had necessarily fallen the main burden of making the arrangements for the meeting, and remarked upon the fine sense of the mover of the resolution, in recognizing the valuable services of the "professor of dust and ashes," Mr. Gallow, on whose behalf he begged to return thanks.

He begged the members not to regard him as speaking these last words in a mere formal, mechanical fashion. During this session many former friendships had been cemented, many new ones formed. All human relations are vain and weak except so far as they rest on a basis of true friendship. Hereafter the people of Minneapolis would remember the members of this Association, not merely as a body of great astronomers, biologists, chemists and geologists, but as a congregation of their warm-hearted happy friends. His parting words in their behalf then were simply, "Dear friends, good-bye."

Prof. N. H. **Winchell** was called for and responded briefly, expressing gratitude for compliments and saying that he hoped the Association felt what it had formally said, that the meeting had been a success. He invited the Association to meet in Minneapolis again after twenty-five years, as it did at Montreal,and promised them that they would see a city of 1,000,000 inhabitants and the University grounds covered with costly buildings.

Gen. **Nettleton** followed Prof. Winchell with a word upon the relation of journalism and science. He said he didn't know how the men of science would get the results of their researches before the public,were it not for the press, which presents them to its great audience in interesting, popular form. The newspaper doubtless gives some misinformation in connection with a great deal of information, but it must be credited with doing valuable service to science. He commented upon the reference made by a member to the cosmopolitan character of the newspapers of the Northwest, and explained it by the fact that the thinking people who had settled here would not tolerate any journalism such as is apt to be found in new sections. He said it seemed out of place for one who did not know a trilobite from a hypothesis to attempt to address such an audience in any capacity, and concluded by emphasizing the remarks made as to the honor which Minneapolis felt had been conferred upon her by the assembling here of the American Association for the Advancement of Science.

Mr. D. Blakely of the Local Committee warmly endorsed what his predecessors had said about the feeling of the citizens of Minneapolis. He said St. Paul and Minneapolis were one and indivisible, like the states of the Union, and each ought to glory in the prosperity of the other. He invited the association to come again, and predicted that the members would hardly know the twin cities. This city will then be a Philadelphia, and be able to extend you such a welcome as you expect to get next year.

Mr. Thomas Lowry, President of the Street Railway Companies, responded to the resolutions thanking the companies under his direction, in a humorous speech in which he alluded to the rarity of thanks received by the management of street railways, for in his experience complaints were the rule and were presented with the highest zeal, while thanks were the exception and most highly prized.

The President said that the time for closing the sessions of the thirty-second annual meeting of the Association had arrived, and he accordingly pronounced the Association adjourned to meet in 1884 in Philadelphia.

The last excursion in connection with the Minneapolis meeting was made on August 24, by about two hundred and forty members and friends to Taylor's Falls for a view of the Dalles of the St. Croix River. The St. Paul and Duluth R. R. Co. offered the courtesies of their road and the attention and zeal of the officials of that Company, and the cordial hospitality of the citizens of Taylor's Falls combined to make the excursion a fitting close to the enjoyable entertainments so fully extended to the members of the Association.

J. R. Eastman,
General Secretary.

Note.

By accident the following paragraph was omitted from the Report of the General Secretary for the Montreal Meeting:

Major L. A. Huguet Latour, M.A., N.P., of Montreal, was received at the Montreal Meeting as a duly accredited delegate from the *Φιλολογιχος ευλλογος παρνασσος* (Literary Society, "*The Parnassus*)."

This Society has sent to the Association thirteen scientific books, written by several of its members.

Permanent Secretary.

REPORT OF THE PERMANENT SECRETARY.

FOR the third time the Association has held a meeting on the western side of the Mississippi, thus acknowledging the claims of the western members and giving its aid to their efforts for the progress of science. As was expected, however, the great distance of Minneapolis from the homes of the majority of the members, and the lack of concessions in fares on the part of most of the railroad companies, made it impossible for many of the members to attend the meeting, and, although the register shows the number in attendance to be twice that at Dubuque or St. Louis, it was by far the smallest attendance at any meeting of late years.

While it must be admitted that all parts of the country have claims upon the Association which should be respected, the question of greatest good to the cause for which it was founded should be carefully considered in accepting invitations from places far away from the residences of the majority of the members; not that large numbers are essential to a successful meeting if the working members and those to whom we look for guidance and encouragement in scientific efforts are present. In this last respect the Minneapolis meeting was a success and the good influence which the meeting exerted upon the young institutions of the rapidly growing city is not to be questioned, while the encouragement it gave to the workers in science in the great Northwest was unquestionably gratifying to them: The only problem to be studied, suggested by the late meeting, is how to bring about a proper appreciation of the work of the Association on the part of the managers of the great railroad lines of the country, by which half rates of fare can be secured for all future meetings; a concession which it seems probable would be granted if the managers of the railroads fully understood the character, objects, and, above all to them, the important influence which the meetings of the Association have on the development of the natural resources of the country and in uniting the people into a strong, coöperative nationality. The recent railroad arrangements and liberal government grants made by Canada, in connection with the coming meeting of the British Association at Montreal, show that these points are appreciated by the Canadian authorities, and it seems hardly possible that they should not be understood on this side of the border. With the great distances to be travelled in this country, it is evident that in order to give the whole country the benefit of the influences of the Association the scientific workers should be able to attend the meetings without too great a drain on their limited means.

The register of the Minneapolis meeting contains 328 names of persons regularly in attendance at the meeting, as follows: From Minnesota 60, Illinois 38, Ohio 29, New York 29, Missouri 28, Massachusetts 21, District of Columbia 20, Iowa 18, Pennsylvania 17, Wisconsin 11, Indiana 8, Michigan 7, New Jersey 6, Canada 6, Connecticut 4, Maryland 4, Kentucky 4, Kansas 4, Louisiana 3, New Hampshire 2, Vermont 3, Texas 2, Nebraska 2, Rhode Island 1, Delaware 1, Tennessee 1, Mississippi 1, Alabama 1, Dakota 1, Oregon 1, = 328.

One hundred and eighty-six persons were elected members, of whom one hundred and twenty-nine have accepted and paid their fees, two have declined, and fifty-five have not yet replied to notices sent them.

Fifty-nine members were made fellows, of whom all but eight have accepted.

Since the list was printed in the Montreal volume, notice has been received of the death of 20 members. The names of these, including one original member and past president, are given on p. 476. Since the Minneapolis meeting two other past presidents have died:—Dr. J. L. Le-Conte and Dr. J. Lawrence Smith. Since the Montreal list 22 members have resigned and 2 have been dropped from the list on account of arrearages. During the same period 11 names have been restored, and one patron has been added to the list.

The total number of names now on the roll of members and fellows is 2023, a gain of 101 since the Montreal volume was printed.

With the exception of the vice president elect of Section D, and the secretaries elect of Sections B, D, and I, all the officers were present, as were also three past presidents.

The presidential address, and the addresses of six vice presidents delivered at the meeting, are printed in this volume. The address of vice president of Section I delivered at the Montreal meeting is also printed in this volume.

One hundred and sixty-eight papers were considered by the Standing Committee; of these three were given as public evening lectures with lantern pictures, two were delivered in morning general sessions, four were not accepted, and one hundred and fifty-nine were referred and read in the several sections as follows: A 23, B 19, C 12, D 7, E 35, F 30, G 0, H 23, I 10.

Twenty-four hundred and sixty-two copies of the Montreal volume were printed from the stereotype plates and of these 1733 have been distributed as follows:

To members who have paid the Montreal assessment . .	1500
Sold	25
Distributed by order of the Standing Committee and for exchanges	208
Total number distributed to date . . .	1733

Every effort has been made to get the present volume through the press before June 1, the date assigned by the Standing Committee for its publication, and the gain in time over that of previous years leads to the hope that the new rule of the Committee requiring that an abstract ready for printing in the volume shall be received by the Committee before a paper can be referred to a Section for reading, will make possible the still earlier publication of future volumes.

The statement of the receipts and expenditures for the year covering the Montreal meeting, and closing just previous to the Minneapolis meeting, is given in the cash account on the following pages. The general funds of the Association remain as given in the last account with the addition of $150.00 to the Life Membership Fund, and the additional gift of $150.00 to the Reprinting Fund from Mrs. H. Herrman,

Respectfully submitted to the Association,

F. W. PUTNAM, *Permanent Secretary.*

SALEM, MASS., May 14, 1884

F. W. PUTNAM, PERMANENT SECRETARY,

Dr. THE AMERICAN ASSOCIATION FOR

1882-3.		
To Balance from last account		$808 57
Assessments previous to 31st Meeting	$657 00	
" for " Meeting	2,729 00	
" " 32d Meeting	1,209 00	
		4,595 00
Admission fees previous to 31st Meeting	10 00	
" " 31st Meeting	1,005 00	
" " 32d "	60 00	
Fellowship fees	184 00	
		1,259 00
Publications sold and binding	117 53	
Electrotypes of cuts	1 70	
Clerical services	5 00	
Postage, express, and discount on checks	1 49	
		125 72
General Fund, Estate of Wm. S. Vaux late Treasurer	84 67	
Interest to date	4 23	
		88 90
Life membership commutations	350 00	
Estate of Wm. S. Vaux, late Treasurer	56 00	
Interest to date	73 00	
		479 00
Reprinting; gift of members	850 00	
Interest to date	46 30	
		896 30
		$8,252 49

SALEM, MASS., AUGUST 6, 1883.

I have examined the above account and find

IN ACCOUNT WITH

THE ADVANCEMENT OF SCIENCE. Cr.

1882–3.

By 2500 copies Proceedings, Vol. 31, Montreal Meeting:—		
Composition, stereotyping and boxes for plates	$1,445 98	
Illustrations	6 19	
Paper and press-work	1,682 83	
Covers, two parts	50 00	
Binding, 2430 copies in paper, two parts	583 20	
" 20 half-morocco, 20.00; 50 in cloth, 25.00	45 00	
Making 25 cloth covers extra	5 00	
Extra copies of addresses, etc.	72 68	
Printing wrappers and wrapping	22 25	
		$3,913 13
Expenses of Montreal Meeting	258 03	
600 copies Montreal list of Members, constitution, etc.	84 80	
Printing for special committee	33 74	
3000 envelopes for special circulars	9 75	
Printing circulars, blanks, cards, etc.	186 55	
Extra clerk hire and labor	16 59	
Travelling expenses Ass't Sec'y	2 60	
Publications bought, and binding	10 95	
Postage, postal cards, stamped envelopes	275 44	
Express, 69.51; Telegraph and Telephone, 1.10	70 61	
Post Office Box to Oct. 1, 1883	8 00	
Postal Guide	1 50	
Minor office expenses	34 24	
Rent of office to July 1, 1883	100 00	
Janitor to July 1, 1883	61 00	
Fuel	19 50	
		1,173 30
Salary of Assistant Secretary	500 00	
Salary of Permanent Secretary	1000 00	
		1,500 00
Plate for reprint Nashville vol.	6 40	
Reprinting fund	889 90	
		896 30
General fund		88 90
Life Membership fund		479 00
Balance to new account		201 86
		$8,252 49

the same correctly cast and properly vouched.

HENRY WHEATLAND, *Auditor.*

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